"The contribution of eastern-boundary density variations to the Atlantic meridional overturning circulation at 26.5°N"

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Response to Referee #1

General Comments

This paper is an elegant demonstration of how the transport in the Eastern Boundary (EB) of the North Atlantic Subtropical Gyre provides an important contribution to the Meridional Overturning Circulation (MOC) in the North Atlantic. As authors mention, these investigations are interesting as they reveal that the transport at the EB must be continuously monitored in order to determinate the transport fluctuations in the MOC. To find out these results, authors compare the transport from an offshore mooring (EBC) and a set of moorings approaching the African continental slope (EBH).

We would like to thank the Referee for the supportive comments.

My strongest disappointment about the manuscript is that authors investigate three different mechanisms to determine the transport variability at the EB, namely, Kelvin waves, Canary Current and coastal upwelling. Nonetheless, they have forgotten to include another dynamical mechanism that does contribute to the transport fluctuation found by the authors. This dynamical mechanism is the generation of cyclonic and anticylonic eddies at the flank of the Canary Islands. There have been a large amount of papers regarding these eddies in the last 15 years. These eddies were first described using remote sensing images (see, for example, Hernandez-Guerra et al., 1993; Pacheco and Hernandez-Guerra, 1999) and their dynamics has been extensively studied in recent years (see, for example, Sangra et al., 2005; Jimenez et al., 2008; Piedeleu et al., 2009; Sangra et al. 2009). Eddy shedding the islands may explain the stronger density anomaly found in EBH than in EBC and the dominant period of approximately 13 days present in EBH. This doesn't mean that the contribution of the EB to the MOC is not important. The importance is clearly seen in Fig. 15 that resembles other studies carried out in the EB (besides references included in their manuscript, authors may look at, for example, Hernandez-Guerra et al. 2002; Machin et al., 2006).

Thank you very much for bringing this interesting topic to our attention. However, we find that it is doubtful that eddy shedding south of the Canary Islands can explain the 13-day variability present in the transports derived from EBH, or the stronger density anomalies found at EBH compared to EB1 (see below). The text has been modified as follows:

The fourth paragraph in Section 1 now reads:
"Among the mechanisms that may change densities at the eastern boundary at 26.5° N, and thus the strength of the AMOC, are Kelvin waves propagating poleward (Kawase, 1987; Johnson and Marshall, 2002), or wind-driven changes in the strength of the Canary Current, or coastal upwelling created by anomalies in the local wind stress along the coasts (Köhl et al., 2005), or the generation of cyclonic and anticylonic eddies at the flank of the Canary Islands (Hernández -Guerra et al., 1993)."

We have added the following text in the first paragraph in Section 7:
"It could be that the 13-day period is associated to variability induced by the eddy shedding south of the Canary Islands. However, the previously reported eddy generation sites are mostly south of Gran Canaria (distant from the shallower measurements at EBH) and subsequently the eddies tend to propagate downstream to the west (e.g., Sangrà et al., 2005; Sangrà et al., 2009). Therefore it seems unlikely that they can be responsible for the density variability observed at EBH. Furthermore, the density fluctuations we observe are coherent over a large depth range of up to 3500 m, while the maximum depth associated with anomalies of eddies shed by the Canary islands is roughly 1000 m (Piedeleu et al., 2009). "

Specific comments

1. Eq. 1 uses depth layers. Density layers should be more convenient.

No change applied. We prefer to keep using depth layers to be consistent with the earlier estimates of the AMOC strength at 26.5° N using the RAPID/MOCHA data (e.g., Cunningham et al., 2007). Given the fact that at 26.5° N the isop ycnals do not exhibit a strong zonal slope, it is expected that there won't be much difference between the depth layers and the density layers.

2. Authors should give the analytic relationship between Ekman transport and z.

We have added the following text in the third paragraph in Section 3:

"T_{EK}(t) is computed by zonally integrating the Ekman transport between the shelf of Abaco (Bahamas) (X_A) and the African coast (X_E) following

$$T_{EK}(t) = -\int_{X_A}^{X_E} \frac{\tau_X(x,t)}{\rho f} \mathrm{d}x$$

where τ_x is the zonal component of the wind stress, ρ is a reference density and f is the Coriolis parameter. In order to obtain vertical transport per unit depth profiles T_{EK}(z,t) that are consistent with previous studies, the transports in T_{EK} are equally distributed in the upper 100 m (Kanzow et al., 2007; Cunningham et al., 2007)."

3. According to eq. (6), Vc is considered constant in x and z. As Vc is clearly independent on z, that is harder to assume with x. Several studies using LADCP data have provided that Vc is NOT constant on x.

The computation of V_C is done following Hirschi et al. (2003). They add a spatially constant correction V_C to the mid-ocean meridional geostrophic flow to ensure mass balance. In reality V_C will have a dependence on x, but the effect on basin-wide transports by the use of a zonally independent V_C will be small.

4. “psu” units for salinity should be deleted through the manuscript.

Fixed. We have deleted the "psu" units.

5. Plots of salinity anomalies in Section 4 are missed. They could help to understand the MW/AAIW contribution at mid-depths in this region.

The main purpose of the manuscript is not to concentrate on the hydrographic properties or to perform a water mass analysis at the eastern boundary of the North Atlantic, but to focus on AMOC-related transport variability arising from eastern-boundary density variability at 26.5° N. We believe that the analyses discussed in the manuscript can be conveyed to the reader on the grounds on temperature and density alone. We do not include the plots of salinity anomalies as they add hardly any information to our topic and we fear that their inclusion would distract the reader from the main subject of the manuscript (given the already large number of figures).

6. EOFs are composed of a spatial pattern (shown in Fig. 11) and a temporal pattern that is missed.

The purpose of Fig. 11 was to show that the vertical structure of the transports per unit depth shown in Figs. 9 and 10 is very different. The difference in the temporal structure of the transports is clearly seen in Fig. 12. We prefer not to add an extra figure to show the temporal pattern of the EOF analysis since it does not add much new information to the one that is already present in the manuscript.

7. Pag. 2510, line 21. It seems that the location of EB1 here matches that shown in Table 1, but both don't match the position as seen in Fig.2a.

Well spotted, thank you very much. On the first year (2004) EB1 was deployed at a nominal position of 24°31.4' N, 23°26.9' W. From the seco nd year (2005) onwards, EB1 was moved to a nominal position of 23°48.6' N, 24°5.7' W with the purpose of locating it on a satellite track. Please note that Fig. 2 displays the mooring positions for year 2007's deployment (the latest period of data included in the analyses presented in the manuscript). We have now clarified this issue in the revised manuscript as follows.

We have added the following sentence in the first paragraph in Section 2.1:
"On the first year (2004) EB1 was deployed at a nominal position of 24°31.4' N, 23°26.9' W. From the second year (2005) onwards, EB1 was moved to a nominal position of 23°48.6' N, 24°5.7' W with the purpose of locating it on a sat ellite track."

Table 1 now shows the two nominal positions at EB1. We have added the following text to Table 1's caption:
"Note that the position of EB1 changed slightly between the first year's deployment (2004) and the subsequent deployments (2005 onwards). The position of EB1 corresponds to year 2005 deployment, while the position corresponding to the year 2004 is given in brackets."

Response to Referee #2

Overview: This is a clearly written, well organized paper describing the impact of variability near the eastern boundary at 26.5N on the basin-wide integrated MOC. The authors isolate the variations of the eastern boundary and evaluate both the representativeness of the two eastern 'moorings' for use as the eastern boundary of the MOC calculation and the spectral character of the eastern boundary variability (primarily seasonal). While the authors use first-person stylings in the paper, which I generally do not like, in general this is one of the better written papers I have reviewed in recent years. My recommendation is for the paper to be accepted, although I do have a number of minor suggestions and/or comments below that I would encourage the authors to address. I do not need to see the paper again prior to publication.

We would like to thank the Referee for the supportive comments and appreciation of the manuscript.

Specific comments:

1. This is really more of a comment relating to the journal submission system than to the authors, however as a reviewer it is very inconvenient not to have the figure number printed right on the figure. Requiring the reviewer to print out all of the individual figures and the text file rather than providing a single document file is less than optimal.

We leave this comment to the Journal's editorial board.

2. Page 5, Data section paragraph 1: Not all of the moorings in the RAPID/MOCHA array are serviced every 12 months - some are serviced on 18 month turnarounds. This is a minor detail, however given the fact that the results of this paper are dependant on all of the components of the array and not just the UK moorings it is perhaps preferable for the different components to all be described accurately (with proper acknowledgement as well).

We have added the following sentence in Section 2:
"Some of the moorings of the western-boundary sub-array (WB0, WB3, WB5) (Johns et al., 2008; their Fig. 1) are serviced at 18-months intervals; the remaining moorings of the full array are serviced at annual intervals (during autumn for the eastern boundary)."

3. Page 7ff: There is not a particularly detailed discussion of the errors associated with mooring motion in this paper. Given the fact that a reader may attribute some of the differences between EB1 and EBH as relating to differing amounts of mooring motion at the two moorings, it may be to the advantage of the authors to clarify this issue.

We have added the following sentence in the second paragraph in Section 2.2:
"Each MicroCAT has a pressure sensor so that when interpolating the temperature and salinity profiles between adjacent pressure levels of measurements on a regular pressure grid, the measured pressures at each time step are taken into account to avoid mooring motion effects."

4. Page 8, equation 1: Is it necessary to indicate that 'z' denotes 'negative depth'? As the equation itself is not shown, I think it would be sufficient to indicate that 'z' denotes 'depth'.

Fixed. We have changed 'negative depth' to 'depth'.

5. Page 8, last paragraph: Given the fact that the Gulf Stream data used in this calculation is based on the cable, and hence is a vertically-integrated quantity with no vertical structure information, it is probably a good idea to clarify here with a few more wordshow you are obtaining T_GS(z,t).

We have added the following text in the third paragraph in Section 3:
"The cable measurements give an estimate of the vertically integrated transport $T_{GS}(t)$. The modal vertical structure of the flow through the Straits of Florida is estimated from historical Pegasus measurements across the straits. Subsequently, the vertical structure $T_{GS}(z,t)$ is obtained by projecting T_{GS} onto the leading vertical mode of the meridional transport per unit depth, which accounts for 87% of the variance (Baringer et al., 2008)."

6. Page 19, line 5: Do you have a citation to back up the claim that the easternboundary bottom velocities are small? I'd suggest you add one so the reader doesn't have to take this claim on faith.

We added the following text in the third paragraph in Section 7:
"At the eastern boundary, observations by Knoll et al. 2002 in the Lanzarote Passage at 29° N show that the mean bottom velocity close to 1200 m only amounts to –1.0 cm/s. In addition, at

the deep part of EB1 the temperature fluctuations are of O(10^{-2} °C) (Fig.4, Sect. 4), therefore it can be expected that the bottom currents are small near EB1. Thus, our reconstruction of the AMOC from densities at the eastern boundary is unlikely to be affected significantly by a possible misrepresentation of the external mode.”

7. Page 19, paragraph 3: Might be useful to include the annual cycle amplitudes of all of the contributing components to the total here. Does the seasonal cycle in the total agree with those of all of the components in a square-root of the sum of squares sense?

We have added the following text in the fifth paragraph in Section 7:
"The peak-to-peak amplitudes of the seasonal cycles of the remaining contributing components are 3.0 Sv, 2.1 Sv, and 3.9 Sv for T_{GS}, T_{EK}, and the western-boundary contribution of the mid-ocean section, respectively. Therefore, the rms of the seasonal amplitudes of all the components is 7.5 Sv, thus slightly larger than the 6.7 Sv of the *total* AMOC. This indicates that a small degree of compensation occurs between the components on seasonal scales. "

8. Conclusions: A few words about the other (longer) time scales in the data may be useful here even though it is at the limit of what is possible given the 3.5 year record. I do not think the authors want the reader walking away from this paper thinking that the eastern boundary contributes only a seasonal cycle, as that might suggest that the eastern boundary moorings are unnecessary as a ’mean seasonal cycle’ could be modeled from the first few years and added to the longer time-scale variability of the western boundary.

Thanks for the suggestion. We have added the following text in Section 8:
"At present the long-term contribution of eastern-boundary density variability to the AMOC is uncertain. The annual cycle at the eastern boundary, however, is larger than expected. This may mean that on longer time scales the contribution from eastern-boundary densities to the AMOC could be significant. Long-term sustained density measurements at EBH are necessary to quantify the role of eastern-boundary densities on AMOC changes at 26.5°N on inter-annual and longer time scales."