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Comment

Interactive comment on “A vertical-mode decomposition to investigate low-frequency internal motion across the Atlantic at 26° N” by Z. B. Szuts et al.

Anonymous Referee #2

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The manuscript presents a modal analysis of the full depth mooring measurements from the RAPID/MOCHA array at 26N of North Atlantic. It concludes that away from boundaries the vertical structure is almost entirely described by the 1st baroclinic mode (BC1), which is highly correlated with the altimetric SSH. While at the boundaries, both the western and eastern boundary, the BC1 is less dominant and only represents less than 10% of the transport variance. My main concern is the cut-off of the upper 140-200m of the original observations during the mode fitting to exclude the influence of surface forcing. However, given the dominant signature of surface intensification

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especially in the upper 200m (see, for example, fig.10 of Kanzow et al. 2009 and fig.10, 11 of Bryden et al. 2009) it is unjustifiable to leave the surface layer out during the mode fitting. There might be occasions when top sensors are missing in the surface layer, the interpolation scheme developed by RAPID project using historical T and S vertical gradient is designed to recover at least part of this surface intensification signature. In a series of papers, the RAPID group seems to be comfortable and confident in estimating the full water column overturning even when top sensors were down. The authors are therefore recommended to investigate the influence of top 200m in the modal analysis. This might change the conclusions for the boundaries.

Other significant concerns:

1. “away from boundaries the vertical structure is almost entirely described by the 1st baroclinic mode”: Really? What about the barotropic mode (BT). Previous studies, e.g. Wunch (1997), show comparable contributions from BT and BC1. Is the conclusion based on the correlation of BC1 and total signal of geopotential and transport anomaly of Fig. 7 and 8? Figures similar to Fig.4 and 5 but only using BC1 re-construction are needed to back up this conclusion.

2. At WB2 the western boundary, the low correlations between geopotential anomaly at 200db and SSH, and between local transport anomaly and SSH in this study are in contrast to the much higher correlation (0.8) between the SSH and dynamic height calculated at mooring WB2 in Bryden et al. (2009) and Kanzow et al. (2009). The first chunk of the time series (2004-2005) in Fig.7 and 8 is strikingly different than the time series in Fig.12a of Bryden et al. Why is this? Close examination of Fig. 7 and 8 show significant difference between the 1st chunk of the time series (2004-2005) and the rest in the correlation between the geopotential anomaly and SSH, local transport anomaly and BC1 and SSH. It seems to me, much higher correlations can be derived during 2006-2009 at the western boundary WB2, more consistent with Bryden et al. and Kanzow et al.

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Minor concerns:

1. Line 9 on page 2052, “at periods a significant fraction of . . .”: delete “periods”.
2. 2nd paragraph on page 2053: the description of depth-leveling is confusing. The dT/dP and dS/dP are all functions of (T) and do not have the information of depth. The depth-leveling scheme would require information of extra pressure sensors.
3. Line 20-23, page 2054, “ No sensors are at exactly the same depth . . . (1-1.5 year)”, also line 10-12, page 2067: frequent 400-800m mooring knock-downs would require the depth-leveling/interpolation scheme to place the measurements to nominal depths. The differences between nominal depths in different deployments are much smaller than the 400-800m mooring knock-downs. Why not interpolate the measurements to a set of common nominal depths so as to have much longer continuous time series.
4. Line 24-28, page 2068, “average error”, “rms errors”: Wonder if it is more appropriate to call them rms deviations, instead errors.
5. Line 20, page 2072, “is similar magnitude to”: “change “similar“ to “of similar”.
6. Line 15, page 2073: change “we contribute to” to “can be attributed to”.
7. Line 23, page 2073: change “small an insignificant” to “small and insignificant”.
8. Line 5-12, page 2074: Still, it seems confusing to call the integrated geopotential anomaly at one location the local transport anomaly.
9. Line 6-7, page 2077: where do these numbers (80% for EB1 and 56% for EBH) come from? Fig.8c? Transport anomalies and BC1 correlation at these two mooring are comparable in Fig.8c.
10. Section 5.6 (application to EBH) and 6 (Discussion): BT and BC1 can reconstruct the pressure anomaly at EBH reasonably well (Fig.4 and 5). BC1 is also highly correlated (close to 0.8) with geopotential and local transport anomaly at EBH (Fig.7 and 8). Then why do density profiles need substantially more vertical modes to be properly

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described. Is this due to the lack of upper 200m density measurements during the mode fitting?

11.Line 22 on page 2087, “west of including WB5 have no velocity measurements”: they do have velocity measurements on WB moorings.

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