Ocean Sci. Discuss., 8, C995–C1011, 2012
www.ocean-sci-discuss.net/8/C995/2012/

Ocean Science Discussions

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.

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Received and published: 13 March 2012

Anonymous Referee #1

Received and published: 11 December 2011
Ocean Sci. Discuss., 8, C815–C818, 2011
www.ocean-sci-discuss.net/8/C815/2011/

The vertical structure of ocean circulation is an important problem in oceanography, and, when combined properly with the available satellite altimetry data, can be used to infer depth-integrated oceanic properties, such as energy fluxes and transport variability. The underlying assumption, supported by past observations, is that the altimetry data reflect mostly the first baroclinic mode in the open ocean. In this paper, the authors revisited this problem by decomposing the RAPID/MOCHA hydrographic mooring data into vertical modes. They found that the vertical structure away from boundaries is almost entirely described by the first baroclinic mode, while the first baroclinic mode is less dominant close to the boundaries, especially the eastern boundary. Even though these conclusions are not entirely new, we find this study is a timely reminder that the altimeter data does not reflect the first baroclinic mode everywhere. The authors did a good job in describing in details the mode fitting procedure adopted in this paper, which makes it easy for the readers to follow.

My main concerns are:

(1): *The first baroclinic mode is indeed less dominant at WB2. However, at WB3, which is only 50 km east of Bahamas (Fig. 1a), the first baroclinic mode seems to, again, explain most of the variability (Fig. 7). Does this mean the assumption that the first baroclinic mode dominates the sea surface variability break down only when it is VERY close to the western boundary, at least, at this latitude? Instead of saying "near*

the boundaries" or "at WB2 and WB3", it will be more informative to estimate how far from the boundaries the assumption that the sea surface variability reflects the first baroclinic mode is not acceptable any more.
Indeed, this would make the manuscript more precise. Although we can't be any more precise than the mooring spacing available, the Rossby radius is roughly 45 km at this latitude and corresponds to the distance that WB3 is offshore. Thus, the relation between BC1 and SSH breaks down within one Rossby radius of the boundary. Changes were made in the abstract, in section 5.5, and in the conclusion.

(2): *The amplitude or energy of each mode varies with the number of modes and fitting methods chosen. Some of the choices seem a bit arbitrary. How robust are they? Especially given WB2 is located on a very sharp slope. Fig. 6b seems to suggest that the contribution of the first baroclinic mode increases significantly at most moorings when the bottom slope is roughly taken into account.*
It is correct but unavoidable that choices related to mode fitting do not have strong justifications: there is poor theoretical or observational expectations about what the vertical structure should be, and vertical structure is invariably under-resolved in one way or another. This applies even to the data set investigated here, which, for such studies, is by far the highest-resolved to our knowledge. The best we can do is to be clear with what choices we make and why. The reasoning behind these choices is applicable to many of the comments made by both referees. Much insight and caution about mode fitting can be gathered from articles by Wunsch (Wunsch, 1997; 2010; and other recent articles of his).

The question of the robustness of the fits is what has driven our choices. We expected that the more modes are included in the fit, the less over-determined the system and the less robust the fit. For instance, a least-squares fit with as many modes as data points is a formally well-determined system and gives results with no error. Such an estimate is not robust if the basis functions (mode shapes) are only weakly orthogo-

nal, however, which is why the Gauss-Markov method is necessary. Though Gauss-Markov limits this erratic behavior to a physically realistic degree, it cannot compensate for inhomogeneous data sampling. For instance, when a microcat data record is not available for a specific deployment and too many modes are fit, the mode amplitudes change noticeably compared to other deployments. Fitting too many modes also has the side effect of giving mode amplitudes that are correlated in time, something undesired from a theoretical point of view. We note that Wunsch (1997), in implementing Gauss-Markov mode fits to data from current meter moorings, chose to fit for 5 modes (barotropic and first 4 baroclinic) despite most of the moorings available having only 3-5 current meters. Given his discussion of calculating the variance described by correlated modes, we conclude that his mode fitting suffered from this deficiency.

Both of the reviewers requested a more detailed analysis of the surface layer. With regard to robustness of results, including the surface layer leads to large instability in the fitting because all of the modes are surface intensified. With only 0 to 2 instruments in a region of low-stratification — the exact number and exact stratification being indeterminate/unsampled — the fitting becomes less well-determined because all of the modes have similar and large amplitudes near the surface. Another factor that leads to ill-conditioning is that high vertical resolution in the surface layer is not matched at depth, which prevents deep maxima of higher modes from being properly resolved. The fitting will thus be forced to balance between resolving short vertical structure in the surface layer (energy to higher modes) with smoothly fitting the widely-spaced (500–1000 m) deep measurements (energy to lower modes). It is a strong request for any numerical analysis technique to give uniform results from non-uniform sampling. Hence our choices to remove non-uniform near-surface data and to resolve many fewer modes than the number of sensors.

Equally important to this numerical criterion are physical arguments for the signal we want to study. Though the modes are surface-intensified, there are also many forced processes in the surface layer that are not efficiently represented by vertical modes.

Surface buoyancy forcing (temperature, salinity) and wind-stress forcing (velocities, Ekman divergence/convergence) are localized to the surface and scale in time and space with the atmospheric patterns that generate them. In the sense that vertical modes are an arbitrary basis function for resolving such features, then yes, modes are suitable for decomposing surface motions. We are not interested in forced surface motions, however, and place our focus on signals that have already been transmitted to the full water column. As such, neglecting the surface layer only excludes a small fraction of the water column (150 m out of 5000 m of water is 3%, at EBH 200 m out of 3000 m is 7%) while significantly reducing contamination by atmospheric-forced 'noise.' The question of energy transmission from the surface layer to the full water column is indeed an interesting question, but would require consideration of other data sets to establish horizontal and temporal scales — this is outside the scope of our analysis.

We have replicated our decomposition including the shallow instruments and find that differences are minor, especially for the low modes. This is independent confirmation that the choices in our method give robust results. The recalculation with shallow instruments shows the following minor but undesired aspects: there is one deployment at MarWest that has results inconsistent with other deployments because of the specific mooring geometry; and WB5 and MarWest have increased variance for BC modes 3 and 4.

The points discussed above have been emphasized in the article, in particular in section 4.2, but also in the introduction, section 2.2, and the conclusion.

(3): *It is important to emphasize that only data at about 6 moorings at a single latitude were used for vertical mode decomposition. Therefore, the results of their vertical mode analysis are not readily to be generalized to other latitudes or other basins.*
Yes. We thought this point was made clearly, but have gone through to make sure we do not overstate our results. We do agree that our results apply to a specific latitude, but this does not necessarily restrict it from other basins or other hemispheres, to the

extent that the forcing of these waves is similar in other basins and that the boundary affects are similar.

(4): *The authors found the variability has long periods in the center of the basin, but short periods at the boundaries. No explanations were given. There are dynamical reasons to expect such difference between the side boundaries and ocean interior.*
Dynamical reasons were added to this discussion.

(5): *No instruments at depth shallower than 140-200m were used for mode fitting. Imagine there are good data to use at these shallower depths so that the fitting is much more constrained by data at these depths, will the agreement with the altimeter data be better?*
Although we expected to find similar results, redoing our mode decomposition with data in the upper 140–200 m gave very similar results. This comment is also addressed in our response to comment (2) above, and in our response to the main criticism of referee #2.

Other comments:

1): *The EBH synthetic profile is created by vertically concatenating the series of short moorings on the African continental slope. This sounds very crude. Have the authors done any test whether this works? even maybe in a high-resolution regional ocean model?*
This is indeed a very rough treatment, and initially our analysis was not extended to EBH because this location would stretch the assumptions of vertical modes too far. Nevertheless, because previous articles have implemented this approach (Cunningham et al., 2007; Kanzow et al., 2009, 2010), we do the same here so that our results can be compared to previous results. We have not done any tests for this, and refer the

reviewer to Baehr et al. (2004) and Hirschi et al. (2007).. Although this could be done in a high-resolution ocean model, for comparison to the observations the model requirements would be high (frequent air-sea coupling, high spatial resolution, accurate winds). As there is poor understanding of the seasonal and high frequency variability at EBH (Chidichimo et al., 2010), the exact model requirements are poorly defined. One study of vertical structure from a numerical model is by Hunt et al. (2012), but the model they used is an ocean-only model with daily wind-forcing and 5-day averages. Even with this model they find that all theories of vertical modes perform poorly at the boundary. As regards the vertical concatenation, we imagine that this is a much more tenuous assumption for considering vertical modes than for the main purpose of the array, which is to measure the boundary density profile.

The initial description of EBH in the 'Mooring Observations' section was modified slightly to include the points made above, and the reference to Hunt et al. (2012) was added to the discussion.

2): *It is not clear to me how the depth-leveling approach works. On page 2054, it says "at the depths of sensors 1 and 2 the interpolated values are unchanged from those directly measured." Does this mean at the sensor depths the directly measured values are used with no corrections? A schematic will be useful here.*
This section was rewritten to be more explicit about the procedure, and a references was provided. The revision also accounted for minor comment 2 from referee #2.

3): *The zonal average of SSH across the Atlantic is subtracted to remove seasonal heating and cooling effects. Why is it important to remove the seasonal cycle of SSH? After all, no seasonal cycle is removed from the mooring data. If the seasonal cycle indeed needs to be removed, why not removing it by fitting a seasonal cycle at each SSH grid point?*
It is important to remove a zonal average of SSH because seasonal heating and cooling

of the surface mixed layer leads to a change in SSH that is not appropriately described by vertical modes. Doing so on a point by point basis for SSH will be noisy. In certain regions, such as the eastern boundary, the seasonal cycle of SSH is minimal, reflecting the presence of additional processes on top of seasonal insolation.

This signal is not removed from the mooring data because they do not uniformly and sufficiently sample the surface layer that undergoes this seasonal heating and cooling. This approach is consistent with our choice to exclude near-surface data from the mode fitting. No action necessary.

4): *On page 2068, the inversion to data is limited below 140 m. The signal in the top 140 m must also project onto the first few baroclinic modes. Including good data in the top 200 m may change your relative mode energy.*
This topic is discussed in point (2) above, and in the response to the main criticism of referee #2.

5): *Page 2068, the last paragraph. "The averaged error of the original signal." Do you mean "standard deviation"?*
We do not mean standard deviation: we refer to the quantity introduced in the previous sentence, the "rms error", and simply take its time-average. Taking its standard deviation would entail removing its time-average before taking its second moment. The rms error already is a second moment, and so we simply average it in time. [Note that an rms error is only equivalent to a standard deviation if the time-average is zero, which is not an assumption we want to make here.] This comment overlaps with minor comment (4) from referee #2, in response to which we adopt the suggestion of using "rms deviation" instead of "rms error". Hopefully this change reduces the confusion apparent in the original manuscript.

6): *As the authors pointed out, it is problematic to directly compare data on a mooring*

with altimetry data, since the data processing procedures involved are so different. Satellite altimetry data near the coast have large errors due to the large errors in the standard tidal and atmospheric corrections there. This may contribute to the lack of agreement at WB2 and EBH.
Yes, this is yet another way that SSH and geopotential height can differ, and is now included in this discussion.

7): *Page 2074, line 22. "The BC1 reconstructions accurately recover the....." It seems to me that the BC1 reconstructions underestimate the standard deviation of the original signal everywhere in Fig. 8b.*
Yes. The paragraph was modified to include this point.

8): *Page 2084, line 17. "Although these findings are a clear explanation for why mid-ocean waves or eddies do not strongly influence the methodology of using end-point density profiles to obtain basin-wide geostrophic transport...." I don't think a clear physical explanation is offered in this paper.*
We did not say that we offer a physical or dynamic explanation, simply that it is a clear explanation that the large-amplitude BC1 structure of mid-ocean waves/eddies is not present at the boundaries. Given that BC1 signals in the interior are large are were (falsely) posited to bias the array (Wunsch, 2008), this article further adds to the result of Kanzow et al. (2009) (that the surface SSH variance decreases at the boundary) by showing that the BC1 mode itself is not dominant at the boundaries, and thus that other forms of motion will be needed to explain the (weak) fluctuations seen.

The concluding paragraph was modified to be clearer.

Anonymous Referee #2

Received and published: 14 December 2011
Ocean Sci. Discuss., 8, C819–C822, 2011
www.ocean-sci-discuss.net/8/C819/2011/

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 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.

The major concern mentioned — our purposeful removal of signals in the upper 140-200 m — is discussed in the response to referee #1 (major points 2, 5). Mode fits have been recalculated with shallow sensors, and there is minimal influence on the mode fits. This is independent confirmation that the method we present is robust for the chosen purposes of this article, which is to consider full-water column signals instead of the insufficiently resolved near-surface layer.

In a general sense, we consider signals in the surface layer to come from two sources: from atmospheric forcing that is localized to the surface, and from the expression of full-column signals at the surface. Both are strong, the former because the surface is the source region of water mass modification (or buoyancy anomalies), and the latter because vertical modes are surface intensified. For lack of sufficient sampling, however, we consider that the good sub-surface coverage of the moorings is sufficient to recover the latter but not the former. In the sense that vertical modes are a random vertical basis function, then variance will indeed be lost but without any obvious or possible way to compensate for insufficient near-surface sampling. In the sense that vertical modes recover dynamic motion spread throughout the water column, then the surface expression of the mode is a minor fraction of its variance. Insufficient resolution of the surface layer will make the mode decomposition ill-conditioned. This is why we originally excluded shallow measurements, to minimize the influence of processes we are not interested in or capable of studying here. Having performed the calculations and found little difference, we have confirmation of the robustness of our method.

Though we use data from the Rapid project, the processing done to obtain basin-wide transport is significantly different from the processing undertaken here. Rapid processing also only grids the temperature/salinity profiles up to the shallowest instrument. To obtain transport profiles (transport per unit depth) above this depth the project relies on mass continuity (Kanzow et al., 2007): when all profiles of unit transport (density-gradient, FL Straits transport, and Ekman transport) are added together mass needs to be conserved, and so the shallow portion of the geostrophic unit transport profile is determined such that the total unit transport profile returns to zero at the surface (and also at the bottom). This clarification has been added to the end of section 2.1.

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. Wunsch (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.*
The role of the barotropic mode is unclear because it cannot be measured by density measurements. Preliminary results were heavily criticized because of confusion about barotropic signals, and so we have prepared this manuscript to minimize our reliance on such matters but discusses them in an appendix A2 for completeness.

Barotropic motion is well-defined for velocity — the vertical average of velocity profile — but has a very weak pressure/density signal that cannot be recovered from hydrographic measurements. For geostrophic velocity or transport, however, a reference level is required, whether implicitly or explicitly. For maximum generality, we reference all geopotential anomalies or pressure perturbations relative to the bottom, but this introduces a depth-average to such quantities. The depth-averaged barotropic amplitude is strongly correlated with the BC1 amplitude, further evidence that the barotropic mode cannot be independently interpreted. The Rapid array avoids this ambiguity by invoking mass conservation to provide a reference level. Our analysis of absolute pressure perturbation (Fig 7) requires the addition of the barotropic signal, but when we integrate vertically to obtain transport (Fig 8) then the influence of the barotropic mode is removed as it does not contribute to vertical shear.

For a mode decomposition, it is necessary to include the barotropic part of the mode fitting, because of the strong correlation of depth-uniform and BC1 amplitudes, to maintain consistency between bottom-referenced geopotential anomalies (or geostrophic transports) and mode reconstructions. We are doubtful that the absolute magnitude of the BT pressure signal is meaningful in an absolute sense, and at most suppose that it gives some sense of the magnitude of bottom velocities. In any case, though interesting, the manifestation of barotropic signals in density measurements is tangential to our analysis and so is only included as an appendix. A clear distinction is made in the methods section about this with a reference to the appendix. These descriptions have been modified for clarity. It does not seem possible to convey this involved point in the abstract, for which we rely on the reader being familiar with the reference level problem for hydrographic data.

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.*
There are two points here: the time period, and the data used.

Both of the mentioned articles had shorter time-series available to them, and so correlations may indeed be different. Colleagues of mine with Rapid are investigating such matters, possibly as a result of different circulation regimes, but our interest is on the full time-series available to evaluate the mean state. Though we readily acknowledge that an average from a 5.5 year time-series with large low-frequency variance may not converge as quickly as desired on the true average, there are few observations able to do better.

Second, our data processing was undertaken very carefully to be suitable for our analysis and does not necessarily reflect what is done by others. Bryden et al. (2009)

mention geopotential anomaly evaluated at the surface, but does not attempt to verify how accurate this extrapolation to the surface is (but note that there is no independent data for comparison). Further, Bryden et al. (2009) and Kanzow et al. (2009) use a WB2 profile that includes measurements from deeper moorings (WBH1 and WBH2) to extend the vertical profiles deeper. It is not surprising that our results should not be identical.

The proper comparison with Fig. 12a of Bryden et al. (2009) is our Fig. 7 (both present dynamic height or geopotential anomaly). To our eye, the signal at WB2 during 2004–2005 is remarkably consistent. Our Fig. 8 presents local transport anomaly, which is a different quantity from that shown in Fig. 7. It is not expected that the two figures should show the same signals.

Minor concerns:

1.: *Line 9 on page 2052, "at periods a significant fraction of ...": delete "periods".*
Done.

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.*
As the matter of griding and depth-leveling was not clear, as evinced by a related comment from referee #1, we rewrote this paragraph to be more explicit.

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.
This was initially attempted, but our consideration of perturbations leads to a much higher requirement for accuracy between deployments from the griding procedure. In contrast, the time-mean over each single deployment is well constrained and readily gives an accurate perturbation quantity that is insensitive to minor differences in absolute value. For obtaining perturbation quantities, our single-deployment method is more accurate and requires less effort than the method suggested. The suggested method requires exact instrument calibration from one deployment to the next and relies on the vertical gridding procedure to be exactly indicative of the true mean stratification. These two requirements take much additional effort and are not necessary. We note that our fitting method was developed to give consistent results for all deployments, which is one diagnostic of generating a long time-series as suggested.

4.: *Line 24-28, page 2068, "average error", "rms errors": Wonder if it is more appropriate to call them rms deviations, instead errors.*
Yes, deviations is clearer and is adopted in this paragraph, the preceding paragraph, and the following paragraph. Note that this comment overlaps with minor comment (5) from referee #1.

5.: *Line 20, page 2072, "is similar magnitude to": change "similar" to "of similar".*
Done.

6.: *Line 15, page 2073: change "we contribute to" to "can be attributed to".*
Done.

7.: *Line 23, page 2073: change "small an insignificant" to "small and insignificant".*
Fixed.

8.: *Line 5-12, page 2074: Still, it seems confusing to call the integrated geopotential anomaly at one location the local transport anomaly.*
We do not understand this comment: why "still"? The referee uses this terminology in other questions/comments, which is perfectly comprehensible to us, and neither is this point raised. We chose this as a convenient short hand. The suggestion of "integrated geopotential anomaly" hides that this quantity is dynamically related to transport — the main purpose of the Rapid array — and why our analysis is particularly interesting.

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.*
These do come from Fig. 8c, but note that it is R that is graphed, whereas the text discusses R^2. The correlations are equivalent for the KB modes, but are on the edge of equivalent for the FB modes. Consider Fig. 8b, where there is significantly more variance at EBH than at EB1. A reference was added in the text to Fig. 8b for clarity.

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 described. Is this due to the lack of upper 200m density measurements during the mode fitting?*
The point being made was from considering the vertical structure (Fig. 9), which shows that many vertical modes are required at EBH to describe the observed density perturbations. At EBH, BC1 is well correlated with the original signal, with geopotential anomaly, and with local transport anomaly. Note, however, that the standard deviation of the reconstruction of transport anomaly (Fig. 8b) is much lower than the original signal. It is possible for the BC1 signal to be well correlated but to still miss much of

the (vertical) variance. This is possible if the BC1 amplitudes are correlated with higher modes. Equivalently, this same fact can be stated that the residual from the BC1 mode is not random in the vertical but instead integrates to a non-zero quantity. This aspect is not capture by the results of Figures 4, 5, 7, or 8c.

We added points from the discussion above to section 5.6 and the discussion.

11.: *Line 22 on page 2087, "west of including WB5 have no velocity measurements": they do have velocity measurements on WB moorings.*
Correct. Not sure what action is requested. Follow up studies take advantage of the velocity measurements, but they fall outside the focus of this analysis.

Interactive comment on Ocean Sci. Discuss., 8, 2047, 2011.