Corrections

Referee comments are in Times New Roman bold italics, author responses are in Calibri for comments and Calibri bold for changes/additions to the text of the manuscript.

Referee 1

) In the discussion, the authors referred several time to other studies in the North Atlantic (Lecointre et al., 2008; Chu et al., 2007; and Hagen, 2005). I wonder how some of the differences between these studies and the manuscript could be explain by the location. Actually, the authors clearly show that the longitude could change the behavior. So what about the latitude?

It is difficult to say how the structure may change with latitude, although it is most plausible that it does, especially as other Rossby wave characteristics are known to vary with latitude. To know for sure the analysis would have to be repeated for each of the other study locations, which, while possible, is outside the scope of this study, however a discussion of the way the vertical structure may change with latitude and how it may/may not be responsible for the discrepancies has been included in the discussion (after line 15 page 1108):

"The differences between the structures found by Chu et al. (2007) and Hagen (2005) and those found here could be due to differences in the latitudes of the study locations. Indeed it has been shown here that the structure can vary substantially with longitude, while other aspects of Rossby waves are known to vary with latitude, such as phase speed, so it is possible that the vertical structure could vary too. Several studies have also noted a change in the surface Rossby wave behaviour polewards of approximately 30^{o} (e.g. Chelton et al., 2007; Tulloch et al., 2009), which may also be reflected in an altered vertical structure. Tulloch et al. (2009) found that in the Pacific Ocean the surface intensification of the first baroclinic vertical structure of Rossby waves derived from standard linear theory reduced polewards, this is however at odds with findings from Hagen (2005) (albeit in the Atlantic) as surface intensification increased with latitude in the observationally derived structures. Latitudinal dependence may not be the underlying factor to the discrepancies however, as the three locations in Hagen (2005) (32^{o}N, 21^{o}N and 10^{o}N) span the latitude used in this study, albeit in the northern rather than southern hemisphere, and if latitude alone were the influential factor the behaviour would be symmetric about the equator, resulting in comparable structures. Local factors, such as topography, stratification and flow regimes, may act to mask such latitudinal dependence and are likely to be the main source of the discrepancies; but it is difficult to determine from the sparse data."

***) In Fig.5d, there is an inversion of the slope at the surface (roughly between 0 and 200 m). This behavior only appears for U BPD. How do the author explain that? Is it a well known behavior of this particular theory (as compared to the 3 others). This inversion is also visible in Fig.7d, is there anyway to test that in observational data to validate or invalidate U BPD as compared to the other?**

The cause of this inversion is unknown, however Chu et al (2007) found something similar when constructing vertical structures from Argo floats in the North Atlantic, although they found a strong influence of the second baroclinic mode in their structures, which was specifically excluded here. In contrast, the comparable structure for U BPD in Aoki et al (2009) showed no such inversion for Pacific Rossby waves in a numerical model. This comparison has now been mentioned briefly in the discussion when discussing the profiles of Chu et al. (2007) and then again when Aoki et al.'s (2009) results are discussed (new words are in bold):

"Their findings showed a large contribution from the second baroclinic mode in the structure, **but did display a similarity to the U BPD profile with an inversion seen in the top 200m, which is not seen in the model profiles here**." (line 4, page 1108)

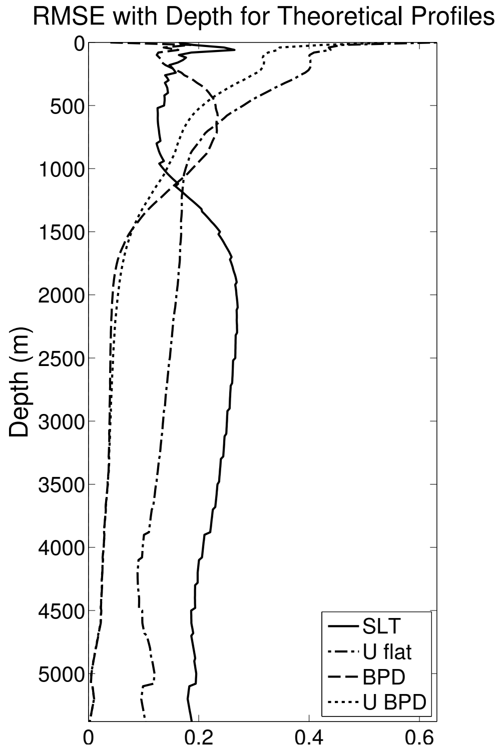
And:

"In common with the findings here, using both mean flow and topography generated the best fitting vertical profile, as shown by the profile RMSEs (Table (2)) and qualitatively by their Fig. (7), however the surface fit of their mean flow and topography theory is better than that of the U BPC profile **as there is no inversion in the top 200m,** although they both show a common region of worse fit between 500 and 1000m." (line 13, page 1109)

The inversion cannot at this stage be considered a characteristic behavior of the theory, particularly with the small number of studies available to generalize from. In terms of validating/invalidating the theoretical profiles using observational data it is very difficult, hence the use of a numerical model. One possibility might be using current meter databases, however the vertical resolution is generally too coarse to provide a proper comparison and in addition separating the mean from the fluctuations and the Rossby waves signal from other signals in the fluctuations is very problematic.

***) I feel that adding a figure summarizing the RMSE as a function of depth could be useful (equivalent to Fig.9 but as function of depth). It will help following the discussion of the authors on which level is better from one theory to another.**

Figure is now included as Fig. (11). The caption reads:



“Figure 11. Root mean square error of the theoretical sections compared to the mean model section as a function of depth.”

It is reported in the results as: (line 16, page 1106)

“The RMSE can also be examined over depth, see Fig. (11), this provides an idea of the variability of the goodness of theoretical fit across the basin at each depth, effectively summarizing the errors seen in Fig. (8). Both theories including topography show very little error below 1500m, however U BPD shows a rapid increase in RMSE above that depth, consistent with Fig. (8d) where alternating under and over estimation can be seen in this depth range. This gives a different impression than when the theoretical profiles are subtracted from the model profiles (Fig. (6c)) as U BPC is the best theory over much of this depth range. BPC however performs well at the very surface, but has an increase in RMSE between 200m and 1000m associated with the too gentle surface intensification. SLT has a large RMSE at depth, which improves towards the surface, resulting in the lowest RMSE of all the theories in the 200m to 1000m depth range, consistent Fig. (8a). The RMSE of the U Flat theory is intermediate between U BPD and SLT at depth and similar, although larger than, U BPC at the surface. The addition of topography reduces the RMSE at depth, while the inclusion of mean flow increases the RMSE at the surface.”

Much of the existing discussion text already applies to the new figure so is unchanged, but a sentence is included: (line 27, 1110)

“The RMSEs with depth (Fig. (11)) highlight that the goodness of fit of a theory is not always consistent at a particular depth range, with U BPD in particular showing a large variability in error above 1500m, further emphasizing the dependence of the best theory with location.”

Minor Corrections

- ***p.1103 - l.14&16: Please, replace BPC by BPD.*** Changed.
- ***p.1102 - l.16&22: I guess that the "three" referred to the mean, mean+STD, and mean- STD. Please, clarify this in the text.*** “the three’ has been replaced with “both the mean and SD”

Referee 2

One of the most striking (if unsurprising) results of the study is that the Radon-transformed fields clearly demonstrate vertical coherence for the waves (Fig. 2). Vertical coherence is necessary to even define an oceanic Rossby wave, but it is not trivial to find in the model data.

The vertical coherence of the waves is not 'unsurprising' given that Lecointre et al. (2008) found that there was a slow down in phase speed with depth (using the same model, but for the North Atlantic). The point is that all assumptions must be tested for their validity in different circumstances, however obvious the outcome of that test may seem and the contradictory results of this paper and Lecointre et al. (2008) suggest this may need closer examination.

The extracted vertical structures are then compared to those predicted by four different theories. The four theories are all variants of plane-wave solutions to the linearized quasigeostrophic equations, varying only by their inclusion or neglect of zonal mean flow and topography (bottom-pressure compensation). Not surprisingly, including both mean flow and topography results in the best fit of vertical structure.

To our knowledge, only one study by Aoki et al. (2009) had previously suggested that the linear theory including mean flow and topography could reproduce composite vertical structures simulated by high-resolution numerical model. This was done for only two locations in the Pacific, however. It is therefore important to investigate whether these results can be reproduced in a different model, at different locations, and how important the methodological issues are in testing the linear theories. Although the theory including mean flow and topography does better than the SLT, it is also clear it does less well than in Aoki et al. (2009), and that occasionally, the best agreement may vary between BPD and U BPD depending on the longitude and the depth range considered. Therefore, even though our results seem to clearly demonstrate that the SLT is inappropriate to account for simulated vertical structures, it is still not entirely clear to us that the linear theory with mean flow and topography is necessarily the ultimate answer to predict real vertical structures.

Comments:

I do not understand why the authors chose to include the three methods for extraction of vertical structure. No rationale is offered for why any is superior to the other, apart from the noted limitation of direct analysis of the Radon-transformed fields, which removes all longitudinal dependence. In my opinion, the EOF method seems clearly superior to the other two. Taking a 'model mean' of the three methods is completely unsupportable. I would strongly recommend the authors choose the best method, explain the logic of their choice, and go on with the comparison to theory. This would clean up the presentation. In addition, the

description of the EOF method should be improved, perhaps by adding an appendix.

In this case the 'best method' is undefined – if you don't know what the structure is, how can you tell which method will best represent it? It is interesting to see where the methods agree (and disagree) and lends credibility to the structures found. The three methods represent different levels of data processing and it is interesting to see how the more 'naïve' methods compare to the 'traditional' EOF analysis method. There is no reason a priori that the theoretical structure should agree better with EOF analysis than any of the others. The rationale of taking a model mean is that each method produced similar structures (it is the standard deviations that vary more between methods) and as you suggest analysis is simplified by only having to refer to one structure. An average is appropriate because it summarizes the results from each method into one structure and the resulting structure falls mostly or wholly within one standard deviation of all of the individual structures, so is within the range of possible structures suggested by each method. A justification for the use of three methods has been added to the method:

(after line 28, page 1095)

"In order to investigate the possible importance of the particular methodology used, three different methods were used to construct empirical vertical structures from the meridional velocity component, respectively based on:

1) **using the Radon-filtered meridional velocity;**
2) **using the amplitude of the Radon transform;**
3) **using empirical orthogonal functions (EOFs)**

The underlying motivation for using these three different methods was to contrast the structures found using the more traditional and established method based on EOFs with simpler and more experimental methods involving less processing of the raw data. Their agreement (and disagreement) provides insights into the nature of the data, as it is not known a priori what the ``true'' vertical structure should be. Indeed, even though one might expect the EOF-based method to be superior, it is important to note that there is no theoretical justification or mathematical proof that this should be necessarily the case. For instance, it is well known that the vertical modal structures of the generalized linear theory in presence of background mean flow and topography are not orthogonal for the natural inner-product serving to construct the EOFs. "

(after line 19, page 1097)

"The Radon amplitude method is associated with the least data processing, as it is performed on the data before it is back-transformed, and the EOF analysis is associated with the most data processing. This makes it possible to see how each

added level of processing affects the data, with similarities across the different methods bringing re-assurance that methodological aspects do not introduce biases (at least in any significant way) to the empirically determined vertical structures.“

The description of the EOF method has been clarified to read (from line 12, page 1097):

“The third method uses empirical orthogonal functions (EOFs) to determine the structures in the back-transformed filtered velocity data. EOF analysis is conducted **on the time series at each longitude**, using MATLAB’s SVD function **(For more information about EOFs and their calculation see Hannachi et al. (2007)).** The structure represented by the first EOF accounts on average for 72% of the variance in the data (maximum 91% (over rough/sharp topography), minimum 58% in the flatter regions), therefore the first EOF was taken to be the representative structure in the data. **When the data is reconstructed using the first EOF there is a vertical structure defined for each location at each time and the data is then treated in the same way as the normalized velocity method by** normalization, averaging and re-normalization. The standard deviation is again treated as above.”

The new reference included above is:

“Hannachi, A., Jolliffe, I. T., and Stephenson, D. B. Empirical orthogonal functions and related techniques in atmospheric science: A review. International Journal of Climatology, 27, 1119-1152, 2007”

My major comment is directed somewhat at this paper, but perhaps more at the entire trail of papers that have followed Chelton and Schlax (1996, hereafter CS). Nearly all the theories considered are based on linear, quasigeostrophic, plane-wave solutions. The fact that CS compared their observations to the "Standard Linear Theory" (SLT – local, flat-bottom, no-mean-flow, no-topography, plane-wave QG solutions) ensured this would become a hugely cited paper, because SLT is hugely over-simplified relative to what I would expect is needed to properly understand Rossby wave propagation in the ocean. If SLT had worked, that would have been amazing; that it does not fully explain the observations, on the other hand, is hardly surprising. Likewise, the ’radical’ ideas of including the effects of mean flow and topography were sure to improve the theory, and these seem to get outsized attention compared to the range of other possibilities, including, for example:

- ***Non-quasigeostrophic effects (e.g. Paldor et al, 2007, JPO 37, 115–128)***
- ***Basin-modes (e.g. Isachsen et al, 2007, JPO 37, 1177-)***
- ***Finite-wavelength effects and baroclinic instability (e.g. Tulloch et al, 2009, JGR-O 114)***

We agree with the referee that the SLT oversimplifies the description of Rossby wave propagation, and hence that it is natural to expect that a more sophisticated theory accounting for a background mean flow and topography should work better. As shown by Killworth and Blundell, and tested by Maharaj et al., this can generally be shown to be the case for comparisons limited to the surface signature of the waves. From a fundamental viewpoint, however, whether an improved description of Rossby wave propagation can be achieved within the context of linear theory is by no means obvious, given that if we are to believe Chelton and Schlax (2007), the majority of observed propagating signals should be regarded as strongly nonlinear. The problem, however, is that nobody currently knows how to predict what the theoretical vertical structures should be in presence of strong nonlinear interactions. To convince ourselves that linear theories are adequate or inadequate to account for observed Rossby wave propagation, and hence that more efforts should be devoted to include nonlinear effects, we still have to test the generalized linear theory rigorously and unambiguously. This is a nontrivial exercise, because linear vertical structures can be sensitive to the details of the background mean flow and bottom boundary conditions, raising methodological issues about how best to chose these parameters, which we could only touch in our paper for lack of clear ideas on how to do it more rigorously.

For this reason, it is difficult to agree that the linear theory in presence of mean flow and topography is receiving outsized attention as compared to other ideas, because the other ideas, apart from Tulloch et al., are clearly immature or too idealized to have a chance to ultimately lead to a complete theory. Indeed, one of us (RT) has had extensive discussions with Nathan Paldor, and convinced him that his ideas were irrelevant to account for the too-fast Rossby wave issue. Paldor's ideas only pertain to the propagation of channel modes in channel wide enough that it becomes important to move away from a latitudinally independent Rossby radius of deformation. Other than that, Paldor's approach is clearly not adapted to investigating vertical structures, as it has so far been only applied to reduced gravity models. Moreover, it is limited to linear effects. The study by Isachsen et al. (2007) is interesting, but rather irrelevant to the issue of vertical structures.

On the other hand, we agree that the paper by Tulloch et al. (2009) is highly relevant, but as far as we understand, their study is also based on comparing observed structures with precisely the same kind of linear theoretical structures as used in the present study.

In any case, I believe that any investigators is free to push the idea that he/she believes is the most relevant to account for observations. My impression is that Tulloch et al's study is bound to receive significant attention in the future, while this is unlikely to be the case of Paldor and Isachsen's studies, which are difficult to extend to realistic settings. For the moment, only three studies, Lecointre et al. (2008), Aoki et al. (2009), and the present one have made any attempts at comparing theoretical vertical structures against simulated ones. Clearly, all these studies are preliminary, and remain to be extended to global domains and done more systematically, which we hope to contribute in a subsequent study.

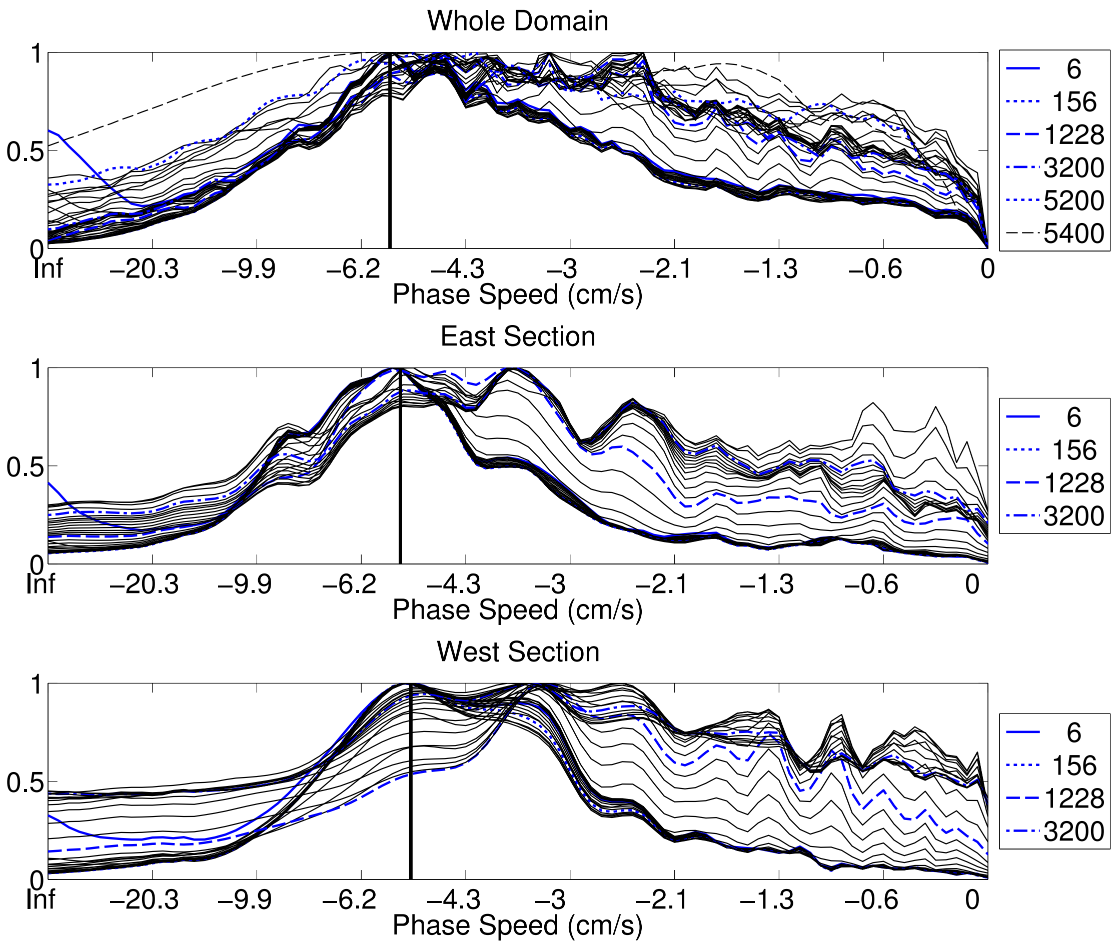
Text acknowledging the different approaches to Rossby waves has been added to the discussion (after line 7, page 1112:

“This study has focussed on how adaptations of linear theory affect the realism of the vertical structure they predict, however it must be noted that there are several other approaches to Rossby waves, besides linear theory, for example, Paldor et al. (2007) examine non-quasigeostrophic effects and Isachsen et al (2007) investigate basin modes. The applicability of these theories could be investigated in future when more fully developed and generalized to 3D.”

Other Corrections Made

- Fig 3(b) legend is changed from ”minus – SD” to “minus SD”
- Eq (2) the “+” in the second term is changed to a “-“ (line 12, page 1098)
- The caption for Fig 8c has had the words “bottom minus” removed.
- The caption for Fig 9 has been changed to distinguish this RMSE figure from the new one by adding ‘as a function of longitude’ to the end of the caption.
- Figure 2 (included below) has been modified so all depths are shown, with addition figures 2b and 2c showing the Radon transform for the east and west basins separately. The caption now reads:

“Figure 2. The normalized amplitude of the Radon transform with phase speed for (a) the whole domain, (b) the East Atlantic and (c) the West Atlantic. Black vertical lines indicate the dominant phase speed.“



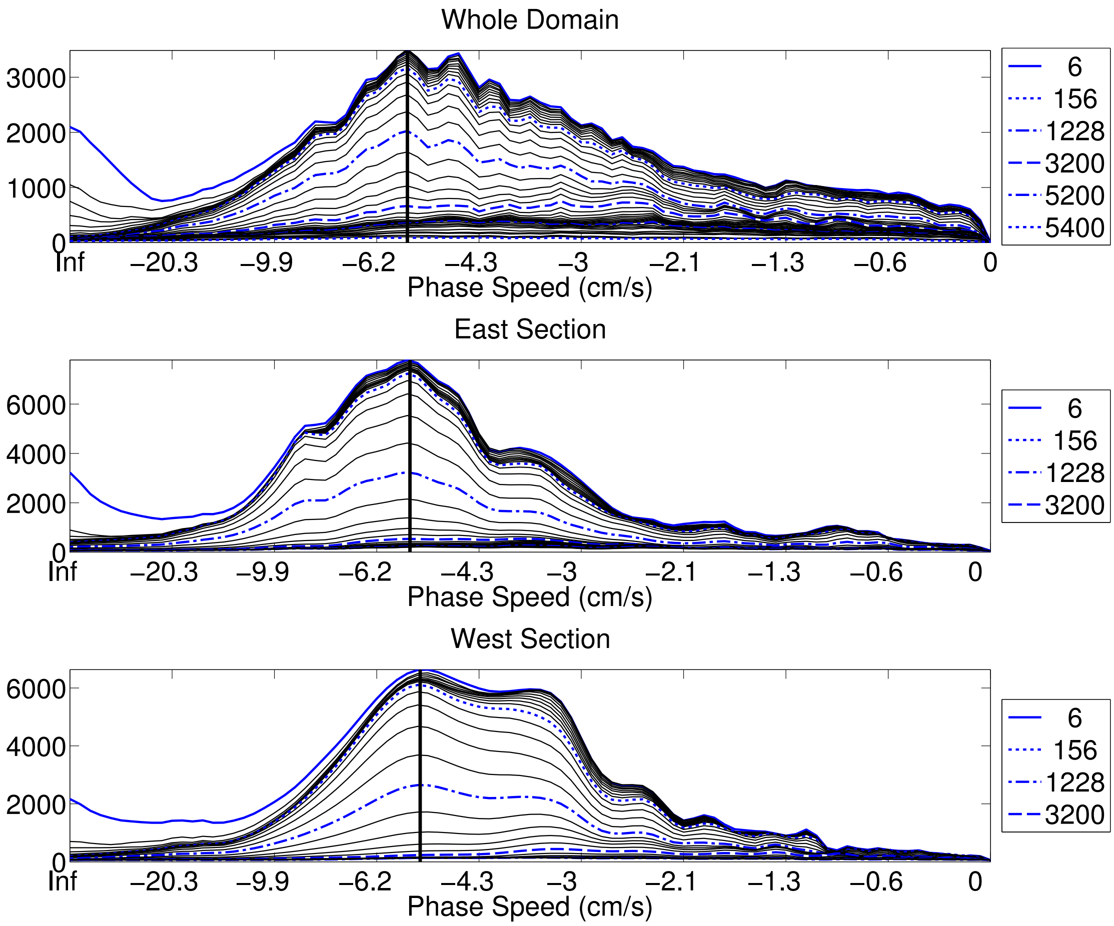
and a section has been added to the results to describe it (after line 6, page 1101):

“When split into East Atlantic and West Atlantic sections the same vertical coherence is seen, (see Fig. (2) and Fig.(3) with very similar phase speeds identified (shown by the vertical black line), with the dominant phase speed in the east at 5.5

cm/s and 5.3 cm/s in the west. This justifies selecting a single phase speed to filter for the entire basin. "

- A new figure has been inserted (figure 3, with all other figure numbers adjusted accordingly) showing the same as figure 2, but non-normalized. The caption reads:

"Figure 3. The non-normalized amplitude of the Radon transform with phase speed for (a) the whole domain, (b) the East Atlantic and (c) the West Atlantic. Black vertical lines indicate the dominant phase speed."



- Paragraph at line 20, page 1097 is changed to read:

"Theoretical structures are calculated for each of the following scenarios: no mean flow + flat bottom (corresponding to the standard linear theory); mean flow + flat bottom (corresponding to Killworth et al., 1997 theory); no mean flow + bottom pressure decoupling (BPD) (corresponding to Tailleux and McWilliams 2001 theory) and mean flow + BPD (corresponding to Killworth and Blundell 2005 theory in the limit of infinite topographic slopes). Note that the BPD theory essentially corresponds to computing the classical surface-intensified first baroclinic mode obtained from Rhines (1970)'s theory in the limit of infinite Topographic slope. Samelson (1992) also suggests that such

surface-intensified modes can result from interactions with rough topography, so that in this respect, the BPD theory can be regarded as a ad-hoc way of representing such effects. “

- The preprocessing of the data using the Radon transform has been clarified in the method. The paragraph between lines 0-16, page 1096, is changed to:

“Firstly, to isolate the Rossby wave signal from other dynamical processes, we apply the Radon transform (Radon, 1917; Deans, 1983; Cipollini et al., 2006) to meridional velocity anomalies (calculated by removing the long term mean). The propagating signals with the largest westward amplitude are then isolated by using a Gaussian filter centred at the phase speed that most often contains the maximum amplitude across all depth levels (i.e. the mode of the location of the maximum), which was co-incidentally the same as the dominant surface phase speed., although it was not required to be so. The filtered Radon transform is then back-transformed into physical space to produce a field that contains only westward propagating signals around the dominant propagation speed. This procedure is repeated for all depth levels. For depth levels that intersect topography the longitudes occupied by the topography are set to zero and the Radon transform still applied to the entire section. This does not affect the result of the Radon transform as it is basically a summative process and so not affected by additional zeros, however the proportion of the section occupied by water necessarily decreases with depth so the Radon transform is based on less data as the depth increases. It was occasionally found that the dominant speed at a particular level would differ from that exhibited by the surface level; in general, however, the Radon power would nevertheless exhibit a secondary peak corresponding to the dominant surface speed, in which case the Gaussian filter would therefore be applied to the secondary peak, rather than to the dominant peak, in order to isolate a vertical Rossby wave structure associated with vertically coherent propagation, as is usually assumed in WKB theories of Rossby wave propagation. This procedure was repeated for two sub-domains representing the east and west Atlantic basins, spanning -37.75^{o} to -16.42^{o}, down to 4000m and -9.91^{o} to 4.42^{o}, down to 4200m respectively, to find the dominant phase speed in each basin. The Radon analysis of the western and eastern Atlantic basins suggests that the dominant phase speed does not vary appreciably in longitude,. This motivates the use of a Gaussian filter centered on a common phase speed and justifies carrying out the analysis over the full Atlantic basin. Moreover, as neither the western nor the eastern sections intercept topography while still leading to a dominant phase speed similar to that obtained over the full section, the analysis suggests that our handling of the topography does not impact the results. Each of the methods constructs profiles that are normalized by their absolute maximum to produce profiles that are easily comparable both between methods and with the theoretical profiles.”

- A paragraph is added after line 16, page 1106:

"As made clear from the theoretical section, the vertical modal structures depend by construction on the zonal mean flow U, the buoyancy frequency N, and the total ocean depth H, which all vary with longitude. "

- Sentences from line 16, page 1112 changed:

"Theoretical considerations not included in this study but which could potentially improve the model-theory fit are finite wavelengths, the examination of non-purely zonal wave propagation and the effects of baroclinic instability. This study emphasises the importance of considering all aspects of a theory when assessing its relevance, and suggests that the surface signature alone is not sufficient for that purpose. "

- New references:

Isachsen, P. E., LaCasce, J. H., Pedlosky, J. Rossby wave instability and apparent phase speeds in large ocean basins. *J. Phys. Oceanogr.*, 37, 1177–1191, 2007

Paldor, N., Rubin, S., Mariano. A. J. A consistent theory for linear waves of the shallow-water equations on a rotating plane in midlatitudes. *J. Phys. Oceanogr.*, 37, 115–128, 2007

Rhines, P. B. Edge-, bottom-, and Rossby waves in a rotating stratified fluid. Geophys. Fluid Dyn., 1, 273-302, 1970.

Samelson, R. M. Surface-intensified Rossby waves over rough topography. J. Mar. Res., 50, 367-384 1992.

Tulloch, R., Marshall, J. and Smith, K. S. Interpretation of the propagation of surface altimetric observations in terms of planetary waves and geostrophic turbulence. J. Geophys. Res., 114, C02005, 2009