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Ocean Science Discussions

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Interactive Comment

Interactive comment on "Depth dependence of westward-propagating North Atlantic featuresdiagnosed from altimetry and a numerical 1/6° model" *by* A. Lecointre et al.

A. Lecointre et al.

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Final author comments : Response to referee comments

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1 Reviewer A

1.1 Main comments

A1. I note that none of the isopycnal longitude-time plots are illustrated in the paper (p. 825 l.15). While I am sympathetic to not cluttering up the manuscript with a large number of redundant figures I believe (as do the authors it seems) that the most important contribution of this paper is the subsurface analysis. Therefore I think it would be instructive for the authors to include plots for 3-4 out of the 9 isopycnal immersion depths for the same domain as the plots currently in Fig. 3. I think the reader would like to see these.

We agree with both reviewers' remark. We have included in Fig. 3 Hovmöller diagrams of the displacements of two isopycnals: $\sigma_1 = 32.05$ (centered at 1000 m depth) and $\sigma2 = 36.95$ (centered at 2000 m depth). Comments are included in section *2.3 Comparisons*.

A2. With respect to the Radon Tranform processing it seems to me that the 20° Hovmöller plots are extracted from the SLA at a 1/3° by 1/3° resolution and from the simulated fields at 1/6° by 1/6° resolution. If this is the case then the aspect ratio of the SLA plots and the modelled surface plots are quite different and could potentially lead to different biases in the speed estimates derived from the 2D RT. Wouldn't it be better to keep the same aspect ratio for the observed and modelled plots if comparisons are being made? Please clarify or qualify.

Indeed, there is a difference in resolution in fields of SLA(x,y,t) from the observations (dx=1/3° x dy=1/3° Mercator x dt=7days) and the model (dx=1/6° x dy=1/6° Mercator x dt=5days).

Both datasets have been interpolated on the same regular dy=1° resolution latitude grid (on the latitudes of Radon analysis). No difference in C emerges at that step. Now, our answer will consider the impact on C of differences in zonal (x) and temporal (t) resolutions of Hovmöller datasets [1].

To evaluate the impact on C of these different (x,t) resolutions, the simulated SLA field has been interpolated on the same spatio-temporal grid as the observations, then the entire processing method (detailed in section 3 of the manuscript) is applied to the $\mathrm{SLA}_{\mathrm{model-interp}}$ field to extract the first baroclinic mode phase speeds of Rossby waves. The maps of simulated phase speeds with and without this preliminary interpolation onto the altimeter (x,t) grid can be compared: C(model-interpolated)-C(model) is about 0.1 cm/s and reaches locally 1.5 cm/s (see Fig. 1 on http://meolipc.hmg.inpg.fr/Web/pages-perso/Thierry/Figures_Lecointre/).

Reciprocally, the observed SLA field has been interpolated on the same spatio-temporal (x,t) grid as the model, and the Rossby waves phase speeds have been

[1] We have decided to perform the Radon analysis on the initial ("native") grids in order to stay close to the initial datasets and keep the small-scale structures of the model.

computed. The results concerning observed phase speeds with and without this "inverse" interpolation are almost exactly opposite to those mentioned above.

The impact of different resolution datasets (including the impact of a different aspect ratio [2]) on the estimation of the phase speeds is thus small, and much smaller ($\simeq$ 10%) than the bias between observed and simulated phase speeds, which was -0.4 cm/s on average over the domain. Note that this impact of different resolution datasets does not concern the analysis of vertical structure phase speeds, since the surface and subsurface simulated Hovmöller diagrams have the same aspect ratio.

Unless the editor demands more details, we propose to add following sentence in section *3.1*: "We have verified that the different zonal and temporal resolutions of the simulated and observed datasets have no significant impact on the diagnostic of phase speeds".

1.2 Minor comments

A3. p.819 l. 12:"led" rather than "lead".

OK, corrected.

A4. p. 820 l. 24: "..impact than the background" rather than "impact that the background".

OK, corrected.

[2]see the paragraph 3.1 in De La Rosa et al, 2007: working with zero-mean Hovmöller diagrams cancels any phase speeds bias due to the different aspect ratio of Hovmöller plots.

A5. p. 822 l. 24: "a quantitative" rather than "an quantitative".

OK, corrected.

A6. p.823 l. 6: "justify why only" rather than "justify that only" ?

OK, corrected.

A7. p823 l.16: "westward propagating [missing word] as observed...".

OK, corrected: "westward propagating features as observed..."

A8. p.823 l. 18: if you haven't already please confirm with the editor or author's notes that it is ok to use the abbreviated form of section (Sect.) as used here a few times.

The word "section" has been changed into Sect. by the editors themselves.

A9. p. 824 l. 4: There is a reference to the 20° analysis window before the methodology is explained and it feels out of place. A couple of concise sentences establishing that a 20° moving window is being used and why (presumably to detect spatial variability across the domain) would solve the problem.

We agree with the reviewers' comment. This reference to a 20° analysis window and the choice of the spatial domain 5° N-50° N has been put at the end of the section *3.1 Radon Transform*, where the use of a 20° moving window is explained.

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A10. p.826 l. 19: Since the field in question here is SLA shouldn't the (xt)-average be (sufficiently close to) 0?

Our local Hovmöller diagrams are not treated on the 1993-1999 period over which altimeter SLA timeserie have zero-mean. Their (x,t)-average should thus be removed before applying the Radon Transform.

A11. p. 828 l. 24: Might be worth qualifying here again that the extended theory takes into account "slowly varying" baroclinic flow and bathymetry.

OK, corrected.

A12. p. 829 l.1: Since there are two sets of white dots it would be good to clarify which ones are $\mathrm{C_{t1}}$ (e.g. top set of dots etc).

OK. Figure 8 has been improved (see item A19), the markers for $\mathrm{C_{t1}}$ and $\mathrm{C_{t2}}$ are now white lines and are described precisely in the legend of the figure.

A13. p. 829 l.3. I am not sure what is "not shown" here. Looks to me like everything is shown-perhaps this is a mistake?

This is the difference between $(C(altimeter) - C(model))$ and $(C_{t1} - C_{t2})$ which is not directly shown. Actually, this can be seen along a few lines (33° N, 25° N, 21° N, 42° W) in Fig. 8, by comparing the plots for C(model) and C(altimeter) (grey lines on the plots) with $\mathrm{C_{t1}}$ and $\mathrm{C_{t2}}$ (white lines on the plots). We can see that the distance between C(model) and C(altimeter) is smaller than the distance between $\mathrm{C_{t1}}$ and $\mathrm{C_{t2}}$ (this explanation has been added in the manuscript). Figure

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2 (on http://meolipc.hmg.inpg.fr/Web/pages-perso/Thierry/Figures_Lecointre/) shows an histogram over the investigated basin of $\frac{C(altimeter)-C(model)}{C_{t1}-C_{t2}}$ and shows that except at one location, this ratio remains clearly between -1 and +1 over the entire region under investigation (5° N-50° N).

A14. p. 829 l.6: The Radon analysis (also?) detects...

OK, corrected.

A15. p. 831 l.2: I found this sentence a little confusing as I read through it while looking at the figure. May I suggest that the authors write it as "Both observed (Fig. 9f) and simulated (not shown) phase speeds tend...".

OK, corrected.

A16. p.831 l.5: I think this is part of the source of my confusion. The caption for Fig. 9f states that the plot is the difference between simulated and theoretical estimates when it is observed minus theoretical.

OK, corrected in the legend of the figure.

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A17. pp. 831 l. 18: I feel that the use of the term "substantial" is a bit of an overstatement here particularly since there are regions where phase speeds do not decrease much with depth. Also this statement begs the question of whether these changes are significant with respect to the smoothing done in the processing of S. The reader is reassured of this later in the manuscript but I would suggest the point be made here right off the bat.

OK, corrected. It has been replaced by "...indicating that zonal phase speeds change with depth."

1.3 Comments about figures

A18. Figure 2: this figure appears to be black and white while some other figures are in colour in my copy of the manuscript. It would be more instructive for this figure to be in colour.

OK, this figure is now in colour.

A19. Figure 8: this is a good quality figure but it is very busy and I struggled to take it all in the first time I looked at it. I couldn't really tell thick open circles from thin open circles and I am not sure if there are white lines in (a)-(d). Perhaps the figure could be enlarged? Also it might be good to point out that the definition of C_{t1} and C_{t2} is in section 3.4 (and not at the initial reference to this figure in the manuscript).

OK, the figure and its legend have been improved.

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A20. Figure 9: I have pointed out the discrepancy in the caption and title for 9f already.

OK, corrected (see our answer to item A16).

A21. Figure 11: In my printout the black and grey colours cannot be easily distinguished. Perhaps the grey could be made even lighter?

OK, corrected.

A22. Figure 12: "show" rather than "shown" in "Thin grey lines...".

OK, corrected.

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2 Reviewer B

2.1 Main comments

B1. My interpretation of the results in this paper is quite different from that presented by the authors. Evidence is conclusive now that the vast majority of westward energy propagation observed in satellite altimetry that has been (mis)interpreted in the past as linear Rossby waves modified by mean currents and bathymetry is actually nonlinear eddies. Studies of this phenomenon that predated the availability of the merged dataset from the TOPEX/Poseidon and ERS-1/2 altimeters were unable to distinguish linear from nonlinear variability because of inadequate spatial resolution of the SSH field. In the merged dataset, more than 90% of the observed westward propagating features are nonlinear at least once during their lifetimes. When it is accepted that nearly all of the observed variability is nonlinear eddies,

We agree that observed and simulated westward-propagating features likely have a nonlinear character. But it is difficult to know for sure, without making extensive further specific diagnostic, to assert whether the simulated features are truly nonlinear. This, however lies beyond the scope of the present study. It has been precised in the introduction that the terminology Rossby waves is used as a generic descriptor of westward propagating signals, whether these turn out to be linear waves or nonlinear eddies.

B2. there is no reason to expect a priori any particular vertical structure of the features.

We do not make any apriori assumption about the vertical structure of their westward propagation. We precisely aim at characterizing this issue.

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B3. As discussed by Roemmich and Gilson (2001, J. Phys. Oceanogr., p.675) and many others, the vertical tilt of nonlinear eddies results in an eddy flux of heat. The vertically nonuniform propagation speeds presented in this paper may therefore be indicative of zonally nonuniform meridional eddy heat flux. If an eddy starts out as with vertically uniform phase and propagates westward with faster propagation speed at the surface than at depth, the eddy would gradually tilt toward the west. As discussed by Roemmich and Gilson (2001), a westward tilt results in northward heat transport. The slow-down of westward propagation with increasing depth would thus result in westward intensification of northward eddy heat flux.

The vertically nonuniform propagation speeds presented in this paper may therefore be indicative of zonally nonuniform meridional eddy heat flux. If an eddy starts out as with vertically uniform phase and propagates westward with faster propagation speed at the surface than at depth, the eddy would gradually tilt toward the west. As discussed by Roemmich and Gilson (2001), a westward tilt results in northward heat transport. The slow-down of westward propagation with increasing depth would thus result in westward intensification of northward eddy heat flux.

As stated in the title and in the introduction, our aim is not to diagnose the northward eddy heat flux throughout the basin, but to carefully quantify the spatial variations of westward phase speeds in a realistic model experiment through the Radon Transform. On the other hand, the possible existence of progressive eddy tilting toward the west and enhanced eddy heat flux does not question our approach, nor our conclusions. The reviewer actually builds upon our results: his interpretation does not seem to differ from ours. This remark is thus an interesting hypothesis (as mentioned in his third paragraph) which might inspire further studies, but not question our interpretation.

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B4. The stated objective of this paper (e.g., the last paragraph of the Introduction and the first sentence of the Discussion section) is to investigate and describe the vertical structure of westward propagation. However, that is not actually what is presented in this paper. The authors have documented the vertical structure of the propagation speed, not the vertical structure of the variability.

We agree, our sentences might be slightly misleading. We have modified them.

B5. The latter could be done from lagged correlation analysis, which would quantify the vertical phase structure of the westward propagating features. This would then provide the information needed to test my hypothesis in the preceding two paragraphs that the vertically nonuniform propagation speed represents westward intensification of eddy heat flux.

This is indeed one of the methods we could use for such an investigation, which is, however, not part of our paper's objectives. A detailed analysis of the vertical structure of westward propagating signals is presently underway.

B6. It is noteworthy that the authors do not present any figures in this paper that show the nature of the westward propagation at any levels other than at the surface. In lieu of lagged correlation analysis of the vertical structure at a few selected locations (or summarized statistically on a regular latitude-longitude grid), time-longitude plots at a few selected depths would allow a qualitative assessment of the vertical phase structure of westward energy propagation along the latitude of the plot.

Such information were not included initially for concision. Figures 3 (c,d) and 9 (g,h) now include Hovmöller diagrams of simulated isopycnal immersions (see also question

A1) and westward phase speed maps, respectively.

B7. Because of the importance of physical interpretation of the vertically nonuniform westward propagation speed, I encourage the authors to expand their analysis to investigate whether the slow-down of westward propagation with increasing depth is a surrogate indicator of westward intensification of northward eddy heat flux.

See item B3 above.

2.2 Minor comments

B8. p.819, lines 8-11: The authors state that the validity and accuracy of the normal mode expansion is questionable. This is not technically correct. The normal mode expansion is valid and accurate, regardless of its dynamical relevance. The normal modes provide a complete basis set. But just like sines and cosines, the normal mode expansion may not be an efficient representation of the vertical structure of the variability.

The main purpose of physically-based theories of Rossby waves and eddies is to identify what are the relevant dynamical modes in the oceans. The terms accuracy, validity, and relevance therefore refer to the possibility of using the normal modes as physical modes, as opposed to just a basis on which to project the equations of motion, which is clearly not the meaning intended. As to technicalities, we disagree that the standard modes would constitute a suitable basis to decompose oceanic motions in presence of variable topography, as the latter are defined over a flat-bottom. This means that such a basis is not complete for actual motions, which means that the series would not converge continuously toward the actual solution, and would likely result in a Gibbs phenomenon. We have corrected the introduction to highlight that the relevance of the

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SLT in our case might be hampered by various features, in particular the complex topography and possible interactions between vertical modes. We now state more clearly that the validity, relevance and accuracy of the dynamical modes the SLT predicts have never been really tested.

B9. p.826, line 13: The vertical stripes in the rotated coordinate system are parallel to the t' axis, not the x' axis.

The reviewer is right that the diagonal features become parallel to t' at the right θ. The description of the Radon Transform in section *3.1* might be misleading and has been reworded.

B10. p.833, lines 18-19: The overall slow bias of 1 cm/s in the westward propagation speeds in the model is an interesting result that is worthy of further investigation. From a WKB analysis, the westward propagation speed of Rossby waves in the nondispersive limit is proportional to the square of the vertical integral of the buoyancy frequency (see Appendix A part b of Chelton et al., 1998, J. Phys. Oceanogr., p.448). The bias of the propagation speeds of the model may thus be due to the stratification being too weak in the model. This could be assessed by comparing the buoyancy frequency profiles from the model with historical hydrographic data.

This is an interesting question. We have tested whether the slow bias of C(model) (which is exactly -0.4 cm/s on average over the domain, this has been precised in the manuscript) could be due to a destratification of the model during the 21 years simulation. Theoretical phase speeds were computed as in Chelton et al (1998) from

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the initial (Reynaud's climatology [3]) and final (1993-2000 average) model stratifications. Their domain-averaged difference are unsignificant (0.03 cm/s) and does not allow to link the (very) slow bias of the model and a possible destratification.

3 Other minor modifications

- Slight modifications have been made in the paragraph "If further confirmed ..." (in the conclusion), and at the end of the abstract, to clarify the implications of our results.
- In the introduction, we now explain that "The 2D RT is used in the present study to extract from observed and simulated data in the subtropical North Atlantic the signals whose propagation speeds are the closest in magnitude to those of the first baroclinic mode as predicted by the extended theory of Killworth and Blundell (2003)". This clarifies the way we make use of the Killworth and Blundell (2003)'s theoretical predictions.
- We have added a paragraph at the beginning of the acknowledgements to dedicate this paper to the memory of Peter Killworth.

[3]Reynaud, T., Legrand, P., Mercier, H., and Barnier, B.: A new analysis of hydrographic data in the Atlantic and its application to an inverse modelling study, Int. WOCE Newslett, 32, 29-31, WOCE International Project Office, Southampton, United Kingdom, 1998.

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