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Ocean Science Discussions



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

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Depth dependence of westward-propagating North Atlantic features diagnosed from altimetry and a numerical 1/6 deg model by A. Lecointre T. Penduff P. Cipollini R. Tailleux and B. Barnier

This manuscript examines the vertical structure of westward propagation in the North Atlantic basin from the CLIPPER high resolution numerical model principally using the 2-dimensional Radon Transform. Simulated sea surface anomalies are first compared to remotely sensed observations to establish the merits of examining this model's out-

put. Then further investigation is carried out on the characteristics of the first baroclinic Rossby wave vertical structure in the model simulation. One of the most important contributions of this study is the subsurface analysis of the isopycnal displacements. These results suggest that there is a deceleration with depth of the westward propagating signal contrary to the normal mode assumption in the Extended RW theory. Interestingly regions where the phase speed is more coherent with depth corresponds to areas of high eddy activity rather than corroborating the normal mode assumption for Rossby waves.

This is a well written thorough and interesting paper which I found a pleasure to read. There is a nice review of the literature in the introduction section particularly regarding the emerging theories on RW propagation which gives context to the outcomes of this study. The methodology is thorough and explained well and the figures are of high quality.

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 figure 3. I think the reader would like to see these.

With respect to the Radon Tranform processing it seems to me that the 20deg hovmoller plots are extracted from the SLA at a 1/3deg by 1/3deg resolution and from the simulated fields at 1/6deg by 1/6deg 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 2DRT. 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.



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Minor comments:

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

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

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

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

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

p.823 l. 18: if you havent 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.

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

p.826 l. 19: Since the field in question here is SLA shouldnt the (xt) average be (sufficiently close to) 0?

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

p. 829 l.1: Since there are two sets of white dots it would be good to clarify which ones are Ct1 (e.g. top set of dots etc)

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?

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

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...".



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

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.

Figures:

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

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 couldnt 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 Ct1 and Ct2 is in section 3.4 (and not at the initial reference to this figure in the manuscript).

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

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

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

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Interactive comment on Ocean Sci. Discuss., 4, 817, 2007.