Author's response to: Referee #1's comments on "Commonly used methods fail to detect known phase speeds of simulated signals of Sea Surface Height Anomalies" *by* Y. De-Leon and N. Paldor

Summary and recommendation:

The main aim of this paper is to challenge the reliability of the observational basis for the 'too-fast' Rossby waves evidenced by Chelton and Schlax (1996) based on 4 years of Topex-Poseidon satellite altimeter data. The authors derive their conclusion from showing that it is possible to construct a synthetic Rossby wave signal composed of 20 to 50 sine waves with random known speeds, which standard techniques such as the Radon and Fourier transforms fail to identify accurately. In a previous study, Paldor et al. had showed such techniques to work well for a synthetic signal composed of three basic waves only, so the difficulties experienced by the Radon and Fourier transforms in this paper appear to result from the increase in many more basic waves in the synthetic signal constructed. As to the motivation for the present study, Nathan Paldor's group has been working on the 'too-fast' Rossby wave issue for many years, promoting the view that the observed phase speed enhancement results from latitudinal trapping due to Earth's curvature. So far, however, Paldor's group appear to have found it difficult to vindicate their theory from observations; but rather than concluding that the problem might rest with their theory, as others theoreticians may have done, the present study proposes that the blame should lie with the observations and the kind of techniques used to analyse them instead, not their theory.

We appreciate the concise summary the reviewer has written about Paldor's work in the last decade but neither the theoretical work itself nor the reviewer's summary have anything to do with the work under review that examines the applicability of Radon Transform and 2D-FFT methods to time-longitude (Hovmöller) diagrams. We share the reviewer's frustration with the minute impact that a higher-order theory that **consistently** accounts for the latitudinal variation of Coriolis parameter (instead of the traditional paradigm that "f is constant though its derivative is non-zero") had in planar GFD (not only spherical as the reviewer erroneously claims!).

Since no additional assumptions or approximations are employed in the Trapped wave theory (in comparison to the Harmonic traditional theory), and only higher order terms are consistently included, we see no basis for the claim: "… that the problem might rest with their theory". The reviewer is invited to refute the Trapped wave theory in another forum.

As far as presentation is concerned, the paper is clearly written, and the analysis appears to be competently done. However, as a contribution to the general issue of what satellite altimeter data actually tell us about westward propagation in the ocean and about the usefulness/validity of the standard Rossby wave theory, this study appears to be very biased in its approach and therefore of very little scientific value, clearly failing to meet the required standards for publication. This is unfortunate, because I otherwise find Paldor's work on the rigorous analysis of the waves supported by the shallow water equations to useful and valuable. As far as I understand the issue, their work appears to be essentially concerned with refining the standard flat-bottom, no mean flow, linear theory of the shallow-water waves on the sphere, and has therefore no bearing with real Rossby waves, which theoretical advances over the past 50 years have clearly showed to be strongly affected by both the background mean flow and topography. The rationale for my assessment is contained in the following remarks and observations.

Again, the current work does not deal with the consistent wave theory of Rossby waves (on a sphere or a plane) but with methods for extracting propagation speeds from slopes of contour levels on time-longitude (Hovmöller) diagrams. In our view, the reviewer's assessments: 1) that the paper is "clearly written" and 2) that the analysis is "competently done" along with the prevalent usage of these methods in recent (see the response below to main point #1) interpretations of various oceanic observations should render the paper suitable for publication in Ocean Science.

Main points

1. The authors fail to mention that the reliability of Chelton and Schlax (1996)'s conclusions has already challenged by Dudley Chelton himself and his collaborators in Chelton et al. (2011), in which the authors argue that westward propagation in the oceans is dominated by meso-scale eddies rather than linear Rossby waves in contrast to what CS96 had previously assumed. Since then, how to disentangle the meso-scale eddy field from the background Rossby wave field has been a major challenge that only a few authors have tried to tackle. Since we know that meso-scale eddies tend to have an equatorward or poleward drift depending on whether they are cyclonic or anti-cyclonic, it is clear that determining their propagation characteristics cannot be easily achieved from the use of Hoevmuller diagrams in longitude/time, which is why eddy tracking algorithms have been developed. Since we don't really know to what extent the propagation speed of eddies differs from that the more linear background Rossby wave field, it seems clear that there is some degree of uncertainty about how CS96's results should be interpreted. In any case, it is clear from Chelton et al. (2011) that there is no observational basis for their synthetic signal.

We changed the focus of the paper from satellite derived SSHA signals to propagation speeds derived from time-longitude diagrams (but we cannot ignore the simple fact that the Radon transform and 2D FFT methods were heavily employed in SSHA signals derived from satellites). Both the Hovmöller diagrams and the methods employed to interpret them were used in recent years (last 5-6 years) and not only prior to 2011. Additional such references will be included in a revised version of the manuscript.

2. Theoretical developments prompted by Chelton and Schlax (1996) have clearly revealed that the background mean flow and bottom topography have a major impact on the propagation and vertical structures of Rossby waves, and hence that the standard theory can never be a satisfactory description of actual Rossby wave propagation regardless of what satellite altimeter data actually tell us. Indeed, Aoki et al. (2009) and Hunt et al. (2012) have both convincingly established that the standard theory cannot account for the features of simulated Rossby waves propagation, which can only be satisfactorily explained when both the mean flow and bottom topography are accounted for. Flat bottom, no mean flow, modes are completely unable to capture the vertical structure of simulated Rossby wave variability. Irrespective of what the observations tell us, I believe it is pretty clear that the authors' approach cannot tell us anything about actual Rossby waves.

Again – the manuscript does **not** deal with the theory of Rossby waves (be it Trapped or Harmonic)! We only examine the accuracy of the methods used to extract propagation speeds from time-longitude diagrams. Indeed, the manuscript does not "tell us anything about actual Rossby waves" and the reviewer's comment belongs somewhere else and not in a review of the issue our paper addresses.

3. Contrary to what this paper and previous ones assert, theoretical studies of the standard theory based on the WKB approximation are able to account for both the trapping of the Rossby waves as well as for Earth curvature, and it is misleading to refer to such theories as harmonic theories. In WKB theory, one will typically express the pressure anomaly in the form

$$p = A(x, y, z, t)e^{i\Sigma(x,y,t)}$$
$$k = \nabla\Sigma \quad , \quad \omega = -\frac{\partial\Sigma}{\partial t}$$

In such an approach, the amplitude is slowly varying, and will in general decay with latitude, thus capturing the trapped wave behaviour emphasised by the authors. The function Σ is a rapidly varying phase function, allowing to define a local wave vector and frequency. Note that a single WKB wave mode is able to represent the observed beta-refraction and a latitudinally varying phase speed.

In contrast, the basic wave mode considered by Paldor's group is separable in latitude, and typically chosen of the form

$$p = A(y)e^{i(kx-\omega t)}$$

Arguably, if the term 'harmonic mode' needs to be used, it seems more appropriate to the modes considered by Paldor's group, since it is clearly what they chose for the temporal and zonal dependence of their mode. As a result, such a mode does not capture the beta-refraction pattern described by Shopf et al. (1981) for instance, raising the question of how useful this kind of mode is to describe mid-latitude Rossby waves.

Though this point has nothing to do with the sermon of our paper we agree with the reviewer. The difference between the Trapped wave theory and the Harmonic wave theory is precisely the form of A(y) in the last expression for *p*: In the traditional, Harmonic, theory the variation of *p* is sinusoidal so these waves (that spread over the entire latitudinal domain) are named Harmonic while in the Trapped wave theory A(y) has the form of Airy function whose maximum is located near the equatorward boundary (southern in the northern hemisphere). In mid-latitudes Schopf's theory employs the usual "f is constant though its derivative does not vanish" while in his equatorial ray theory the frequency is y-dependent so the concept of separation of variables, that underlies the form of *p* is entirely lost (d/dy should include the latitudinal derivative of the frequency). Again – we emphasize that these (interesting) issues have nothing to do with the sermon of the present work!