Author's response to: Referee #2'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

Characterizing the properties of oceanic Rossby waves is central to understanding the role of the ocean in the climate as much of the response of the ocean to large-scale forcing is mediated by these waves. Indeed, this issue has attracted considerable attention across the ocean sciences, particularly since the advent of accurate altimetry measurements in 1992 when it became possible, in principle, to observe the signature of Rossby waves at the ocean surface, yet many aspects of such waves remain poorly understood. In particular, it has been found that observed phase speeds derived from altimetry data are systematically faster than the speeds suggested by the theory of Rossby waves. A number of explanations for the disagreement between observations and theory have been proposed, including the effects of the mean zonal flow and bottom topography or the fact that many of the westward-propagating features observed in the altimetry data are, in fact, eddies rather than Rossby waves.

The present study tests the ability of several methods to estimate the phase speed of Rossby waves on simulated data, and finds that such methods very often fail to estimate the true phase speed. The authors then conclude that this is the most likely reason for the differences between observed and theoretical phase speeds. The paper is well written, the figures are mostly adequate and clear, and the experiments designed to assess the skill of the various detection appear to have been conducted appropriately. Unfortunately, although the overall aim of the paper is worth pursuing, some aspects of the paper raise doubts and I do not believe that the results from the performed experiments support the authors' conclusion that "none of the methods is reliable for estimating the phase speed of Rossby waves in the real ocean". The authors are right in concluding that none of the methods is able to estimate the true phase speed in the simulated data, but this conclusion cannot be extrapolated to the observed data since, to the extent that I understand the issue, I don't think the simulated data provides an accurate representation of Rossby waves in the real ocean. In conclusion, I think that the manuscript requires substantial revisions and thus I cannot recommend it for publication as it stands. Details on my main concerns and other minor points are provided below.

Following the reviewer's comments, we will include in the revised manuscript oceanic phenomena other than Rossby waves in which the same radon Transform and 2d-FFT methods are employed. Our findings are relevant to all observations (e.g. near shore dynamics, eddy propagation) where propagation speeds are extracted from time-longitude diagram. Our choice of parameter ranges is drawn from the massive usage of the examined methods in the extraction of Rossby wave phase

speed from time-longitude diagrams of satellite observed SSHA signals.

Main points:

1. It is unclear to me from Section2.1 how exactly the simulated data are generated. The authors state that "The values of the phase speeds, C, are uniformly distributed in the 0 to -18 cm/s range". Does that mean that each of the 20 or 50 modes is assigned a different phase speed within that range?

Yes, that's exactly what we did. We will clarify it in the revised version of the manuscript.

Long Rossby waves in the ocean are approximately non-dispersive and so their phase speed is the same at all frequencies. Hence, assigning a different speed to every mode, if this is indeed what is done here, seems unjustified. Could you please clarify how exactly phase speed are ascribed to each mode? How do the results change if the same phase speed is used for all modes?

The emphasis is on "Long" while we include all wavenumbers, long and short, so the waves should be considered dispersive. In the case when all modes have the same phase speed, the 2D-FFT methods still fail in many cases (see the existing remark in the second paragraph of the Discussion) while the Radon transform methods will probably detect the phase speed correctly (we will add a note to this effect in the same paragraph).

Also, the range -18 to 0 cm /s contains some rather extreme values, do you get the same results if the speeds are generated from the range (-10, -2) cm/s?

The range of phase speed we employ is an "envelope" of observed values of Rossby waves. As per the reviewer's suggestion we calculated the detection accuracy of the 4 methods in smaller ranges of frequency and phase speed and the conclusions from these results will be added to the revised version of the manuscript.

In low latitudes the phase speed of Rossby waves can easily exceed 15 cm/sec, and in high latitudes it is less than 1 cm/s. See e.g. Fig. 7 of Killworth et al. in the Journal of Physical Oceanography (1997), attached below. We will omit the words "in mid-latitudes" in the 2nd paragraph of section 2.1.
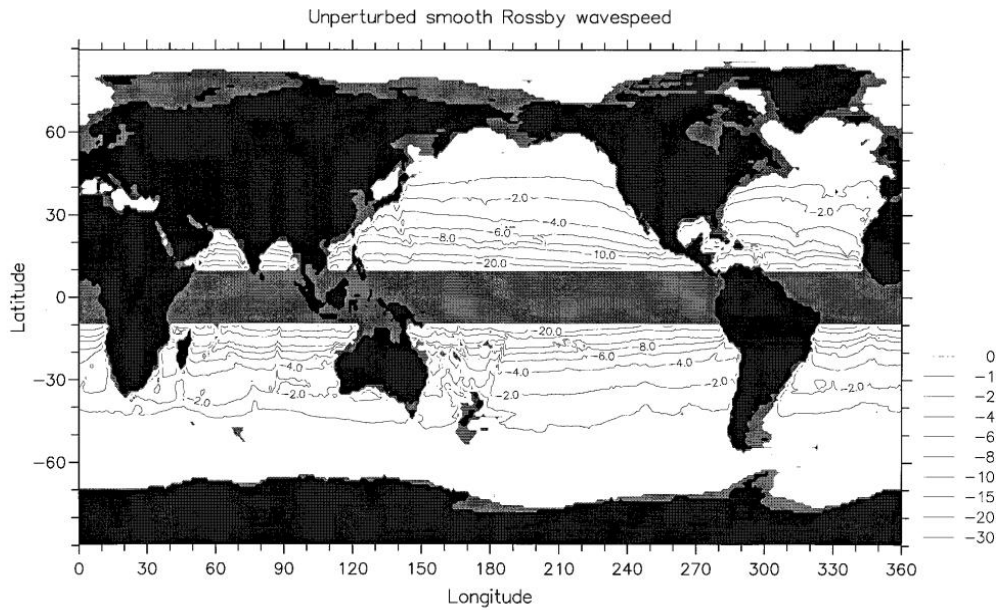
Unperturbed smooth Rossby wavespeed

FIG. 7. The unperturbed fastest long planetary wave speed (in cm s⁻¹) (simply $-\beta/f^2$ times the square of the unperturbed internal wave speed shown in Fig. 6). Contour intervals are nonuniform: 30, 20, 15, 10, 8, 6, 4, 2, 1, and 0 cm s⁻¹ westward. (The values 30 and 0 do not occur, but are added for comparison with later diagrams.) The values are masked within 10° of the equator, where equatorial, rather than long planetary wave, theory should hold.

2. On a similar comment, the theory of Rossby waves indicates that Rossby waves have a maximum frequency, which for the ocean is quite restrictive. For example, no baroclinic Rossby waves with periods shorter than 13 weeks are possible poleward of about 15o latitude. Here, the periods are taken from the range 5 to 200 weeks, which again seems to include some rather extreme values. Could you please provide a reference supporting such high frequencies for observed Rossby waves? How do the results change if you restrict the periods of the Rossby waves to, for example, the range 15 to 100 weeks?

Assume that a typical propagation speed is 5 cm/s and examine a wave with 5000 km wavelength. Then:

$$T = \frac{2\pi}{\omega} = \frac{2\pi}{kC} = \frac{\lambda}{C} \Rightarrow T = \frac{5 \cdot 10^6\, m}{0.05\, m/s} = 10^8 \text{ seconds} \approx 1150 \text{ days} = 165 \text{ weeks}$$

Considering the Nyquist frequency constraint our choice of longest period of 200 weeks does not seem to be an over-estimate for Rossby waves. Lower values of C and higher values of λ will yield longer periods.

The lower value of 5 weeks does not differ much from 13 weeks. However, as stated our response to comment point #1 above, we will include a brief description of the results for smaller ranges of both frequency and phase speed.

3. Theoretical phase speeds are not only different from observations, they are systematically slower. If the simulated data were an accurate representation of the

real ocean and the detection methods were really the issue here, then the authors should also find a systematic bias in the estimated phase speed. However, there is no mention of this in the paper. The authors only state that all methods fail to estimate the true phase speed of Rossby waves. Do you find any systematic biases? Could you please further elaborate on this?

Right, the observed speeds are always faster than the harmonic speeds but have no systematic bias compared to the Trapped wave's speeds. A clear example of this behavior is given in the comparison shown in Fig. 5 of De-Leon and Paldor, 2017b (reproduced below). The red curve is the Trapped wave speed and the Green curve – the Harmonic speed. Symbols are the observational speeds that are distributed systematically **above** the harmonic speed but with no obvious bias compared to the trapped wave speed.

In addition, we don't state "…that all methods fail to estimate the true phase speed of Rossby waves", but that they fail to estimate a dominant input phase speed regardless of its physical origin i.e. Rossby waves are an example.
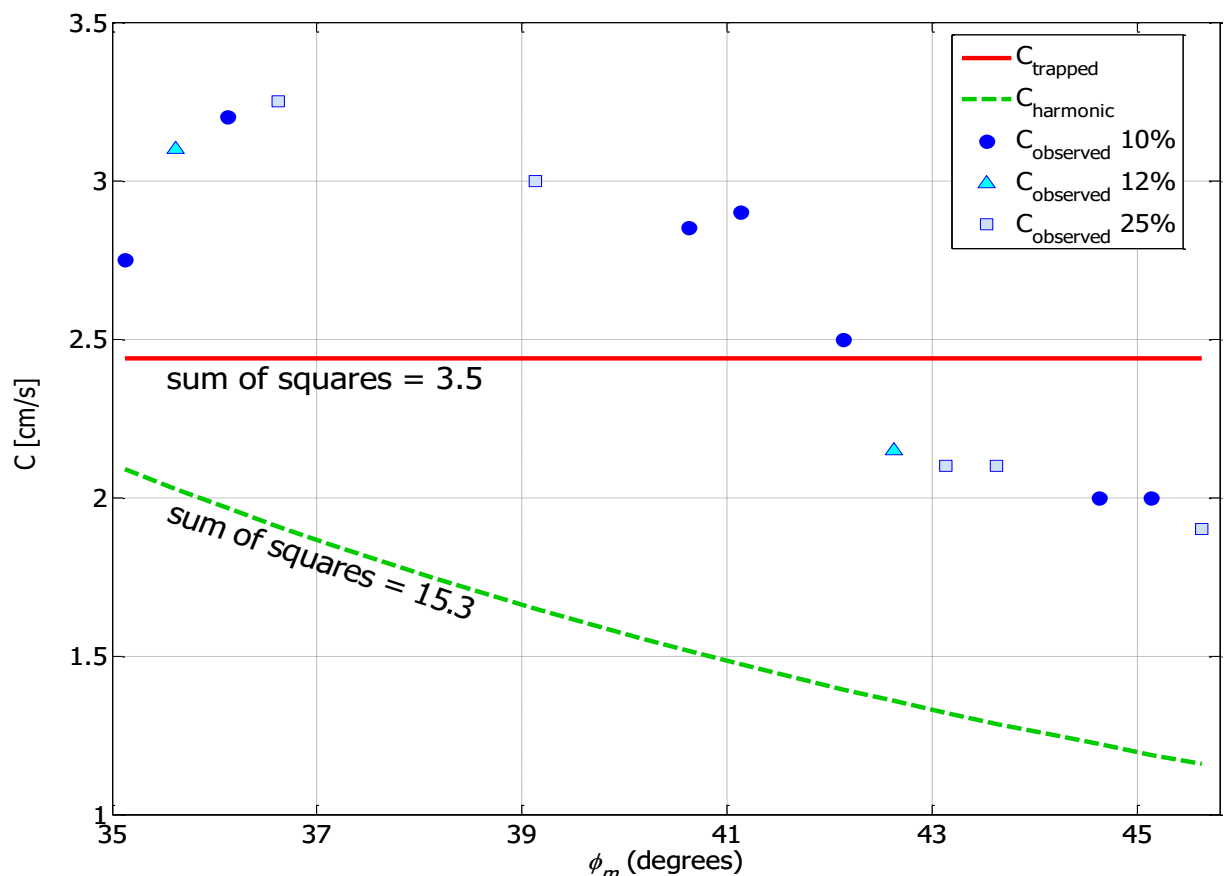

Figure 5. The observed phase speeds and the two theoretical phase speeds (trapped and harmonic) as a function of ϕ_m in intervals of 0.5° latitude. Blue dots denote latitudes where the estimates of at least two methods agreed by 10 % or less, triangles denote latitudes where such estimates agreed by 11 to 12 % and squares denote latitudes where the agreement is 25 %. No reliable estimates were obtained north of 35° S and in some more latitudes. The sum of squares of the distances in (cm s^{-1})^2 between trapped wave phase speeds and observed speeds (3.5) is much smaller than that of harmonic phase speeds (15.3).

4. In assessing the skill of the various methods, the authors assign a score of ½ if the dominant mode falls in one of its nearest neighbors. This seems to me like a rather arbitrary choice. Why not the second nearest neighbor or the third one? Can you estimate a "standard error" for the phase speed estimates based on the multiple realizations and assign a score of 1 when the true value is within one standard error and zero otherwise? This would be, in my view, a fairer metric for skill. Also, I think that 50 realizations is not sufficient and would suggest you use at least 100, if not 1000.

Indeed, our choice is arbitrary but so is any other choice. We will emphasize it in the revised text. The number of cases where the detected mode was 1-bin away from the dominant input mode (i.e. the score was ½) is very small in all signals we examined. As for the number of realizations, we didn't find significant difference between 25, 50 or 100 repeats.

Minor points:
Page 1. The spatiotemporal resolutions quoted here for the altimetry data refer to the grid size and time step of the altimetry gridded products rather than the scales that can actually be resolved by altimeters. Depending on latitude, the spatial separation between altimetry tracks can be of several hundred kilometers and altimeters take measurements over the same location once every 10 days at most. I think that some clarification is needed here, along with some references.

We define the grid in the same way it is defined by Aviso in their description of the altimetry gridded products they distribute to the community.

Page 1. "these features propagate..." What features? Please clarify. We removed this sentence.
Page 1. "Rossby waves that propagate westward" I suggest you remove "that propagate westward" as this seems redundant in this particular sentence. We removed this sentence.
Page 1. replace "diagrams at certain latitude" with "diagrams at a certain latitude". Done.
Page 2. "phase speed exceeds". We removed this sentence.
Page 2. I suggest "in the -18 to 0 cm/s range". Done.
Page 5. I suggest "None of the methods can identify a dominant input ..." Done.