

Interactive comment 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

Anonymous Referee #2

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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 aspect 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 bot-

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tom 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.

Main points:

1. It is unclear to me from Section 2.1 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? 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?

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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?

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 15° 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?

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?

4. In assessing the skill of the various methods, the authors assign a score of 1/2 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.

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

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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.

Page 1. “these features propagate...” What features? Please clarify.

Page 1. “Rossby waves that propagate westward” I suggest you remove “that propagate westward” as this seems redundant in this particular sentence.

Page 1. replace “diagrams at certain latitude” with “diagrams at a certain latitude”.

Page 2. “phase speed exceeds”.

Page 2. I suggest “in the -18 to 0 cm/s range”.

Page 5. I suggest “None of the methods can identify a dominant input ...”

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