

Interactive comment on “Trapped Planetary (Rossby) Waves Observed in the Indian Ocean by Satellite Borne Altimeters” by Yair De-Leon and Nathan Paldor

Anonymous Referee #2

Received and published: 23 March 2017

The Authors use sea surface height anomaly data from satellite altimeters to identify Rossby Waves at the Great Australian Bight. They employ three different methods to estimate the phase speed of the waves, and then compare their results with both the classic harmonic theory and a more recent trapped waves theory. The study main conclusion is that the observed phenomena are Trapped Rossby Waves.

The manuscript is well written, however I have some issues with the content.

Major reviews

1) The trapped wave theory is very recent and I'm not sure the community is well aware of it. Since it is of utmost importance for this work, I'm not satisfied by the

C1

provided explanation in page 9. I do not expect the authors to derive the full theory, but a more detailed explanation is due. Although, I'd like to see the explanation to start with equation 4 from Gildor et al. (2016), I think it would be appropriate if it started at least with equation 6. The first paragraph of page 9 was impossible to follow without Gildor et al. (2016) open beside it.

2) This comment is more general about the classical linear theory phase speed used in this study. Yes, it is slower than the observed one, as was found in many other studies. However, all the theoretical advances made by Killworth and Blundel (one of their papers is cited) managed to bring the theoretical linear speed closer to the observed one. Watanabe et al. (2016, Ocean Dyn.) showed that by using an effective- β , as proposed by Herrmann and Krauss (1989, JPO), the linear theory is good enough to explain the observations at least in the tropics. Although, the tropics is not the authors' case, the main idea is that it is not a surprise that the linear theory fails to reproduce the observations, if it was calculated without considering other parameters (as in the extended theory of Killworth and Blundel, or parameterized in the effective- β as in Watanabe et al.). Also, the linear theory is for free waves, and Rossby waves can be forced by Ekman pumping. In my opinion, there should be some discussion about these matters.

Minor reviews

1) Since it is a study about Rossby waves, I missed some more recent references, such as O'Brien et al. (2013, Remote Sens. Environ.) and Polito and Sato (2015, J. Geophys. Res.), among others. There is also quite a few works from Dr. Angela Maharaj that could be useful as reference. However, the reference I missed the most is Potemra (2001, J. Geophys. Res.), since it is one of the first studies of Rossby waves in the Indian Ocean.

2) The described method based on variance (page 9) is very similar to the one used by Polito and Liu (2003, J. Geophys. Res.), the only difference is that they used in

C2

filtered data, however that does not change the method, so I think they should be cited. Also, they showed how this method is superior to the traditional Radon transform, so the relevant comparison is between the Radon transform based on variance and the 2DFFT one.

3) The explanation about the boundaries of the trapped wave theory is made in the introduction (page 3, lines 11-14). I believe it would be more appropriated if it is moved to section 4 or at least repeated there.

4) Figure 1 looks like from Google Maps. There should be some referencing, right? Ignore this comment if it was indeed rendered by Authors.

5) In page 8, the explanation of harmonic theory (line 15 until end of page) is more detailed than needed (unlike the trapped wave theory). I recommend the Authors to just present the general phase speed and the one for the long waves. In this case, to just cite the basic literature is sufficient.

6) I did not like the math notation. Since the authors are using the β plane, there is no reason to not use f_0 and β . The full expression of both are cumbersome. Also, it is easier to realize what is the last term in the denominator of equation 3 (and whenever it is shown again), if it is shown as a multiplication of 2β and $f_0/(g'H')$, instead of the full expression $(2\Omega)^2 \sin(2\phi_w)/(ag'H')$. So please change accordingly.

7) Lines 11-12 of page 18: add a "in mid latitudes" after "[...] the harmonic theory is valid only in domains narrower than a few hundred kilometres [...]". In the tropics, it works for a few thousand kilometers, as shown by Watanabe et. al (2016, Ocean Dyn.).

8) In the abstract, in the penultimate sentence, please add something like "as was observed by previous studies" after the "[...] 140% to 200%". This result is not exactly unexpected, and the abstract will be better if it acknowledges this.

Other

C3

The jet colormap is not very good. A sequential colormap for spectral amplitude and a divergent one for SSHA would be substantially better. However, since the colormap jet is well accepted by the community (no idea why), I leave it to the Authors' discretion.

Final remarks

I would like to add that I enjoyed reading the manuscript, and I think it is very interesting. I'm sure the Author's will have no difficult in addressing the proposed reviews, and I'll be glad to recommend it to publishing then.

Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2017-3, 2017.

C4