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Interactive comment on "Evidence of coastal trapped wave scattering using high-frequency radar data in the Mid-Atlantic Bight" *by* Kelsey Brunner and Kamazima M. M. Lwiza

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Thank you Dr. Lwiza for replying to the first comment by Reviewer 1 on our behalf. I will now be responding to the second comment provided by Reviewer 1.

Response to the second comment by Reviewer 1:

Point 1:
The reviewer states that:
"The MAB has been the subject of very many studies, whether observational

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of numerical. I'm surprised the authors could not propose any reference to support their view."

Compared to other subfields of ocean physics, the field of coastal trapped waves has a very thin literature. However, in the manuscript we mention a few studies in the region from lines 37 to 38. There are two other references we did not include in that list, i.e., Zhang and Lentz (2017) and Zhang and Lentz (2018), but are included elsewhere in the manuscript. We agree with the reviewer's point that they should be included in the first list. We do acknowledge that there may be other processes in that bandwidth, but the wavelet analysis shows that their energy is small compared to the coastal trapped waves (CTW). We do agree that waves propagate, but it should be remembered that as they do they are either scattered or absorbed (in lossy media). However, it might not be fool proof, but what our results show is more than anecdotal evidence that waves are being scattered to higher modes. This has never been shown before, and it provides an important contribution to the field.

Point 2:

The reviewer admits oversight, but we also see how one could easily make that mistake based on the way we presented both C-EOF and R-EOF in such a compact form. We will try to separate the statements to emphasize the distinction and elaborate more on the methods. We also agree that it would be desirable to be able to combine the U and V fields, but this is not a paper that will address every challenge that exists in coastal trapped waves. Our major objective is to analyze the data than developing a new method. Hence we used available tools. Modification or development of a new method is left for another study.

Point 3:

This is the main contention the Reviewer has against the manuscript.

It is true that CTW modes are not orthogonal as shown by Huthnance (1975). However, we want to draw the reviewer's attention to the literature showing that the orthogonal condition for CTW does exist. It includes a bottom slope term, dH/dx (see Eqn. 29a in Wang Mooers, 1976; the first term can be neglected without loss of generality), whereas the EOF does not. If one ignores the Georges Bank region and the Hudson Canyon, the MAB bottom slope is approximately constant as the data are mostly over the broad shelf. We ran Brink's model to produce the theoretical modes, which were applied in the Wang Mooers Eqn 29a written as:

$\int_{-H_0}^{0} P_n^0 P_m^0 dz + \int_0^{\infty} \left(\frac{dH}{dx}\right) P_n^1 P_m^1 dx = 0$ for $n \neq m$ (1)

where H_0 is depth at the coast, P_n^0 is nth mode at the coast, P_n^1 is nth mode computed across the shelf. We then discretize (1) and drop the first term as follows:

$\sum_1^{N_x} P_n^1 P_m^1 = 0$ for $n \neq m$ (2)

As expected the shelf being broad keeps the dH/dx term constant and near zero until it reaches the shelf break (see attached Figure 6). Therefore, for most part of the shelf to prove the existence of orthogonality between modes we need to show that:

$\sum_1^{N_x} P_1^1 P_2^1 \ll \sum_1^{N_x} P_1^1 P_1^1$ and $\sum_1^{N_x} P_1^1 P_2^1 \ll \sum_1^{N_x} P_2^1 P_2^1$ (3)

We used the data for the transects shown in attached Figure 1, except MA, to compute $\sum_1^{N_x} P_1^1 P_2^1 / \sum_1^{N_x} P_1^1 P_1^1$ and $\sum_1^{N_x} P_1^1 P_2^1 / \sum_1^{N_x} P_2^1 P_2^1$, with the expectation that both will be much less than 1. Attached Figures 2 – 5 show the first three theoretical modes from Brink's model in the top panel and the bottom panel shows the plots of the modes multiplied by the dH/dx following Equation 29a of Wang Mooers (1976). The ratios are less than one in magnitude, but only the Delaware transect has absolute values which are much less than one. This could be because the real topography that is used is introducing errors of deviation from orthogonality. This is an area that needs to be investigated further; for instance, there seems to be a need of smoothing the bathymetry before calculating the modes. Another source of discrepancy may stem from the fact that winds over the study area are not entirely uniform as is the assumption in Brink's model. In any case, we have one transect that satisfies orthogonality which we can use to examine the theoretical modes in relation to the EOF modes.

References:

Huthnance, J. M., 1975: On trapped waves over a continental shelf. J. Fluid Mech., 69, 689-704.

Wang, D.P. and Mooers, C.N., 1976. Coastal-trapped waves in a continuously stratified ocean. Journal of Physical Oceanography, 6(6), pp.853-863.

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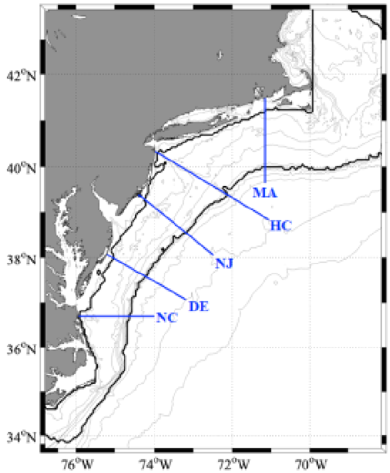

Figure 1. Transects for Brink's CTW mode analysis (blue lines). Solid black lines indicate controlled HFR coverage and light grey lines are bathymetric contours.

Fig. 1.

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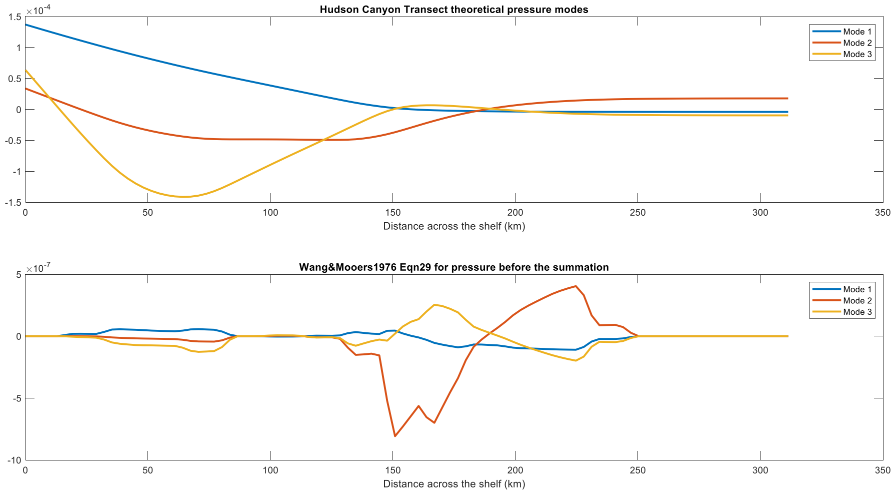

Figure 2. Hudson Canyon transect Brink's modes (top) and the modes multiplied by dH/dx term (bottom). sum(p1*p2)/sum(p1*p1)=-0.2063, sum(p1*p2)/sum(p2*p2)=-0.6174.

Fig. 2.

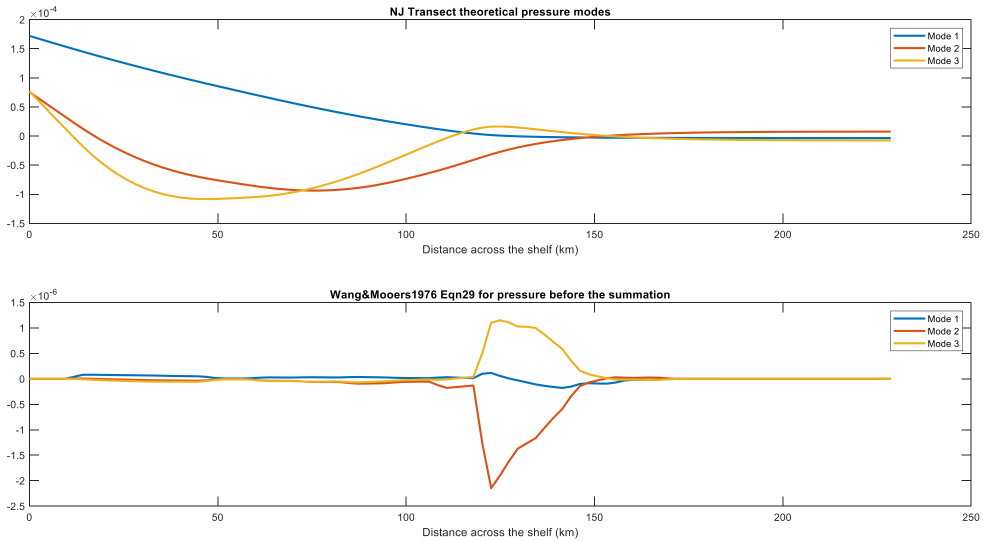

Figure 3. Same as Figure 2 for New Jersey transect. sum(p1*p2)/sum(p1*p1)=-0.2683, sum(p1*p2)/sum(p2*p2)=-0.4701.

Fig. 3.

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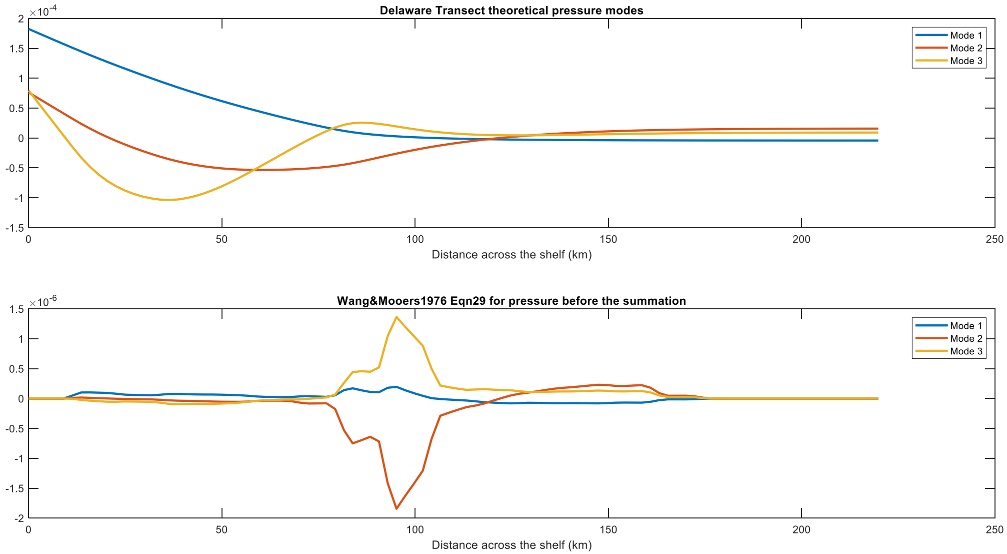

Figure 4. Same as Figure 2 for Delaware transect. sum(p1*p2)/sum(p1*p1)=-0.0007, sum(p1*p2)/sum(p2*p2)=-0.0031.

Fig. 4.

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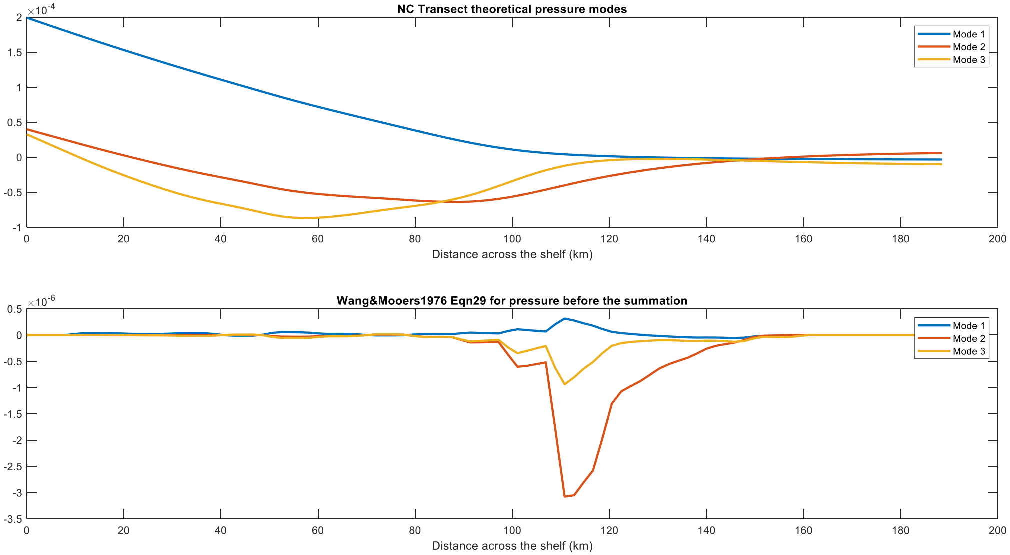

Figure 5. Same as Figure 2 for Delaware transect. sum(p1*p2)/sum(p1*p1)=-0.0961, sum(p1*p2)/sum(p2*p2)=-0.5008.

Fig. 5.

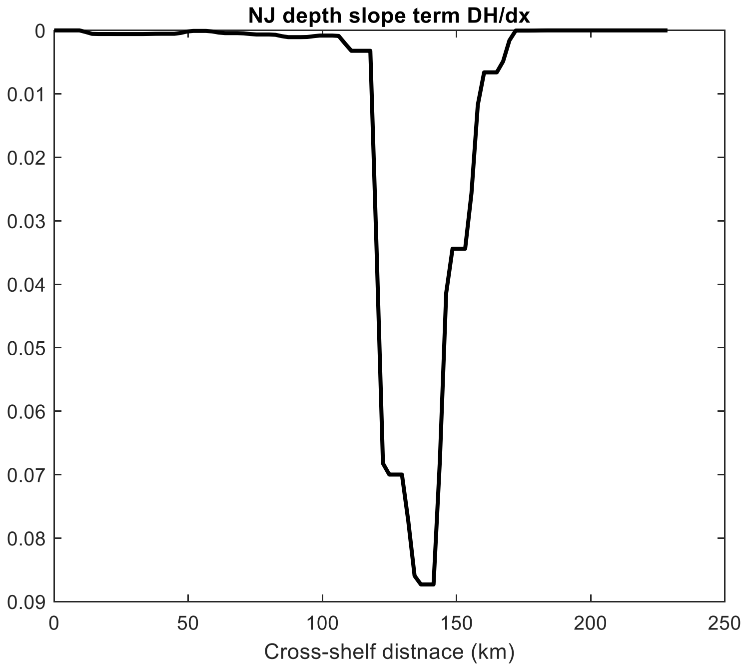

Figure 6. dH/dx term for the New Jersey transect. All transects have a similar (not exact) shape showing a relatively flat shelf.

Fig. 6.