

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

Kelsey Brunner and Kamazima M. M. Lwiza

keliz.brunner@gmail.com

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Response to the first comment by Reviewer 2:

Point 1:

The reviewer states that:

“However, the analysis hinges on use of EOF modes as direct representations of dynamical modes of CTW variability. The linkages between EOF modes to CTW modes are simply assumed a priori and never significantly tested. The one effort to do so, comparing the across-shelf structure of modes to simple modal

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theory for free CTWs.”

This has already been addressed in response to Reviewer 1 comments.

Point 2:

A second assumption that breaks in the c-EOF phase or a decrease in local amplitude is unequivocal proof of ‘scattering’ into higher modes is also not well justified.

Please see Wang (1980) as cited for a more greater detailed explanation of how sudden changes in phase and amplitude are indications of scattering. We agree that our use of the word “unequivocal” may be a bit strong and will re-word.

Point 3:

big HFR arrays have only been operational for the past decade (but their data quality is always suspect)

We beg to differ. HFR data quality has improved considerably. See Brunner Lwiza (2020), which uses the data to compute tidal velocities and residual velocities. Also, the US Coast Guard now uses HFR data for search and rescue operations.

Point 4:

The propagation of features down the wave guide is a significant part of CTW theory. Propagation is never confirmed here. Additionally, data analysis textbooks often show that propagating plane waves can be separated into different modes of variability within an EOF-type decomposition. Is this happening here?

This is introducing a different avenue which is not our focus.

Point 5:

By eye the mode 1 R-EOF amplitude time series has a non-zero mean (Fig 4g), which would suggest a background time-mean flow in the EOF mode itself, is this the case? I’d think that, as the time mean is removed from the EOF

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calculation, that the time mean of the mode should also be zero.

Unless we made a mistake the time mean should be zero as the data is demeaned prior to EOF analysis. Regardless, we will check this.

Point 6:

‘Given the uniform distribution of mode 1 spatial structure and its slow ‘rotation’ over time, isn’t this just the influence of the time-varying large scale wind field (which is predominantly in the 3-12 day band) on the surface currents?’

We will plot wind versus mode 1 to investigate this point.

Point 7:

‘the difference between winter and summer mode 2 results are instead due to changes in the local wind forcing’

The reviewer may be right. We need to test this, although it may be difficult to separate the influence of changing wind forcing and stratification.

Point 8:

Do you think these are free or forced CTWs? This would be important for both the propagation and ‘scattering’ effects.

These should be the free waves that propagate once the forcing has relaxed.

Additionally, given that the MAB is a broad, shallow shelf, the role of friction on CTW should be significant as the size of the waves is long relative to the along-shelf distance. Can this be addressed in your comparisons?

Theoretical frictional decay scales calculated by the Brink model are generally less than one day for mode 1 and on the order of several days for higher order modes. Relative to the period of the waves, in addition to their fast phase speed, friction is not significant in this region.

Point 9:

Line 55: Optimal interpolation applies some modal analysis to determine vectors from the radials. What role might this play in pre-conditioning the EOF calculation

We were not aware of this, but it can easily be proved by comparing it to linear interpolation.

Point 10:

Line 65: The location of the buoy is not defined.

This is a good point. It will be rectified.

Point 11:

Equ 1-5, if you are going to present the governing equations in this detail, you might also wish to discuss each equation and/or relate the results at the end to this presentation, otherwise, is it needed information?

The governing equations are important to present in this manner as they are the underlying equations for the Brink model, in addition to the general physics. Furthermore, with the argument for orthogonality using Wang Mooers (1976) formula, we need the equations even more.

Point 12:

Line 135: This sentence contradicts itself.

It does not contradict itself, but we will elaborate in the revised version.

Point 13:

Line 140: Again, jumping to scattering as the reason for this difference seems presumptuous. What might the role of plume dynamics downstream of the Del and Ches. Bays play on the across-shelf structure of along shelf currents? While Zhang and Lentz worked specifically on the HC area, Zhang's earlier work also showed a natural break in circulation at the canyon due to wind driven

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circulation.

This is discussed in the manuscript and Brunner Lwiza (2019).

Point 14:

Line 155: The sharp discontinuity that exists along the eastern edge of Cape Cod (for both modes 1 and 3) is somewhat troubling. There is no bathymetric feature, or coastline orientation that might align to explain the scattering. Knowing the MARACOOS array, I know that one of their radars is located exactly at the location of the discontinuity, raising the possibility that this aspect of the results is due to instrumentation issues.

See Brunner Lwiza (2020). We agree that it is unusual, but other observations and numerical model results also show that discontinuity.

Point 15:

Figure 4 and 5: You are presenting the results as along and across-shelf, but interpreting the winds as zonal and meridional. This does not allow for an easy determination of forced vs. free modes. The changes in coastline orientation does not necessarily preclude this, as the CTW's should propagate southward, and be most affected by upstream winds.

Point well taken. We took the winds to represent the whole region, but will rotate the winds to along and across-shelf and add data from other buoys in the region.

Point 16:

Line 168: I'm not sure I understand the argument here. You are suggesting that increased variance in higher modes suggests that 'scattering' of low modes into high modes is occurring. yet, the Mode 3 energy at the HSV is low, not high. . would this suggest that scattering is not occurring at this location?

It is not clear to us what is happening in HSV. Probably not scattering, per se, but it could be absorption. When you watch videos of the CTW activity, the signal

propagating from the north dies out when it reaches the HSV, and then reappears to the south on the New Jersey side.

Point17:

Line 175: By extension, this statement suggests you believe that EOF modes 20-30 are also representative of CTWs, is this the case? If not at which modal number to you think your assumption of EOF mode = CTW mode breaks down?

We do not know for sure, but probably modes 20-30 represent 0.01

Point18:

Line 180: why would freshwater inflow induce scattering into higher dynamical modes?

That is what we observe, we do not know exactly why. It is one of the many questions that need further investigation.

Point 19:

Line 185: You are suggesting that the summer mode 1 does not feel the canyon due to stratification. This would also suggest that, if it is an CTW, it is a baroclinic mode 1 wave, which would have a dramatically slower, and measurable phase speed. Is this observed in the data?

We do not observe a dramatically slower wave with measurable phase speed in summer. As stated above, there appears to be more absorption than scattering at this location that may also be explanation for this unusual behavior.

Point 20:

Line 190: The CTW velocities are band pass filtered between 3 and 12 days. What about the energy in the 0 to 3 day and 12 to monthly energy bands?

The 0-3 day bandwidth is dominated by tides, and based on wavelet analysis 12-day to monthly bandwidth contains very little CTW energy.

Point 21:

Line 214: It is not clear what part of the vector field in figures 7c and 8c are representative of wave reflection?

When the phase has sudden change (almost reversed).

Point 22:

Line 215: please refer to fig9 here.

We agree – we will.

Point 23:

Line 217: Is this assumption also true in HSV? I ask as you seem to make the opposite argument there. . . Why are they different?

The assumption holds for most parts except HSV, and we do not know why. Observations do not always agree with theory. Further work beyond the scope of this paper needs to be done to find out why.

Point 24:

Line 234: I disagree with your use of the term ‘unequivocally’. In the previous section, you ascribe spatial variations that are occurring in the same area as the Chesapeake outflow plume as due to CTW just because they occur in the 3-12 day band. This is not proof of CTW scattering.

We agree that the word “unequivocal” was too strong. We will rewrite the statement.

Point 25:

Line 237: I disagree that you have the ‘necessary framework to demonstrate that scattering was occurring’.

We disagree with the reviewer here, because we think we do.

Point 26:

Line 240: ‘ . . .large, sudden jumps in phase on the C-EOF phase maps are indications of scattering. . . ’ **What other processes might cause the same effects?**

We do not know of any other process that can cause a sudden change in phase like we are observing except scattering.

Point 27:

Line 244: **See my above comment. . .the sharp change in phase/amplitude is not in an area of strong change in bathymetry. Why else would CTWs be changing here? Additionally, the discontinuity causes divergence and convergence, depending on the sign of the amplitude timeseries, not just divergence.**

In the area you are referring to we see the phase jumps even in the tidal signal, and even model results indicate similar features. However, we do not know why. Since it seems an area that brings more confusion to an already complex analysis we will exclude it from the results, because it does not add value.

Point 28:

Line 260: **Its not obvious how you are defining reflections from an observational point of view. What evidence suggests this in the data?**

Reflection by definition is scattering, but we will change the wording here – estuary to freshwater flow.

References:

Brunner, K. and Lwiza, K. M. M., 2020. Tidal velocities on the Mid-Atlantic Bight continental shelf using high-frequency radar. *Journal of Oceanography*, 76, pp.289-306.

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pp.1-5.

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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