Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-46-RC3, 2020
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Ocean Science
Discussions
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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

Anonymous Referee #2

Received and published: 10 September 2020

Summary: This is an interesting paper that uses a year of HF radar surface currents from the Mid-Atlantic Bight to examine the potential for coastal trapped waves over the continental shelf. The MAB is a broad, flat shelf with significant HFR coverage and is likely an ideal place to do this. I've not seen other studies that directly examine CTWs within HFR results, and thus the work is novel for this reason alone.

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 theory for free CTWs

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is described as poor. 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. Unfortunately these two assumptions underlay the bulk of the analysis described. The O(1) influence of the local wind in forcing surface flows appears to be largely ignored.

As I said, I’ve not seen very much HFR analysis of CTWs in the literature. I’d assumed that this was due to a mismatch of timing; big HFR arrays have only been operational for the past decade (but their data quality is always suspect), and much of the CTW work was done in the 1980s to 2000. I think this type of effort would be great to see more of, but this present work appears to rest on assumptions that need significant support.

Other Major comments

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?

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

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?

Winds over the MAB are not completely uniform. The likely heavy influence of the local winds in forcing surface currents at the same frequency bands is not investigated. How are each mode related to the local winds? There are enough buoy observations in the

MAB to test this. I'll suggest that an alternative interpretation of the difference between winter and summer mode 2 results are instead due to changes in the local wind forcing.

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

Minor comments

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?

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

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?

Line135: This sentence contradicts itself.

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 circulation.

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.

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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.

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?

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?

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

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?

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?

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

Line 215: please refer to fig9 here.

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

Line 234: I disagree with your use of the term 'unequivocally'. In the previous section,

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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.

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

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?

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.

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

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