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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 #1

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Reply to short comment by K. Lwiza:

Reply to point 1:

- I am not as familiar with the phenomenology of the Mid-Atlantic Bight as the authors. For that reason, I find the position of the authors that CTWs are the only physical process left to explain the band-pass filtered velocity signal needs to be substantiated. The MAB has been the subject of very many studies, whether observational of numerical. I'm surprised the authors could not propose any reference to support their view.

- I'm sorry my discussion of wave phenomena (order-0, etc...) has been misleading. The point I was trying to make is that waves, basically, propagate. And that I was surprised that the authors had ascribed their failure to observe propagation in their JPO paper (I'm perfectly happy with that conclusion) to scattering, which is in my opinion an even harder process to observe unambiguously, and had put all their effort on this one hypothesis, seemingly to the exclusion of any other.

Reply to point 2:
I apologize for not having read carefully enough the original article. There are many methods termed "EOF" around. When I had tried to understand which version of which the authors were using, I had read the Kaihatu et al (1998) paper, which mentioned a complex-vector EOF method with which I completely disagreed. I am a lot happier with the C-EOF method described in Barnett et al 1983, which I found the authors have used. (I'm a bit curious about the practical implementation of the Hilbert transform, of which I've personally always preferred to stay clear). Also, I think this method could be applied jointly to the U and V fields, removing the need for the R-EOF method.

Point 3:
One point I am also not happy with is the fact that even the sensible incarnations of the EOF methods are limited to extracting eigenvectors which are orthogonal with respect to the Euclidean scalar product, while it is a classical result that the CSW modes are not (see the paper by Huthnance 1975). This is something I did not mention in my first review, as I felt I needed to expand on it quite a bit to make the review constructive, and I was too unhappy already with the C-EOF method I thought the authors had used. Basically, the method used by the authors seeks only eigenmodes which possess a property the "true" eigenmodes don't possess. The first eigenmode structure may be correctly obtained, but the subsequent eigenmodes *have to* point in the wrong direction, as the method wrongly requires them to point orthogonally to the previous modes. I am thus skeptical that the variance in the subsequent modes, which are not correctly determined, ends up there because of scattering of the first mode.

I have been uneasy about this particular point for many years, ever since I read the Huthnance 1975 paper, in fact. Orthogonality is always linked to a particular scalar product, and I felt that it should have been possible to define a scalar product for which at least some of the modes discussed in this paper would be orthogonal. I'm attaching a text in which I explain how I think this can be done, and how the C-EOF method could be modified to make the empirical eigenvectors orthogonal with respect to the correct scalar product. This analysis is restricted to the barotropic CSW case, but the authors only have access to surface currents anyway, and they will probably agree that the text is complicated enough... I imagine J. M. Huthnance, who is editor for this journal, and has already posted a comment, will have opinions on this. I agree I'm not happy with the boundary condition on the shore side, and I have not investigated in any depth what happens to the Kelvin wave and the waves of the continuous part of the spectrum. Then, no one reviewed *my* contribution, so I am the one with no safety net here...

I agree writing this amount of text is not reasonable in a review, and I would perfectly understand the authors to consider I'm going too far. Still, with such complexities lurking in the system, I can not agree with their statement that they have "unequivocally identif(ied) CTW mode scattering within observations" (their line 235). I'm afraid there are too many basic things about this problem that are not well understood for unequivocal statements about scattering to be made...

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2020-46/os-2020-46-RC2-supplement.pdf

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