

## ***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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### **General comments:**

- This article proposes an analysis of HF-radar observations of surface currents over the Mid-Atlantic Bight in terms of coastal trapped waves dynamics, using EOF methods, with a strong emphasis on the "scattering" of low-order CTW modes into higher-order modes by topographic/bathymetric features of the domain.
- The article is well-written, clear, and the figures are of good quality.
- The survey of the existing literature is well-written and fair.

C1

- It is a follow-up on a previous study published by the same authors in Brunner et al, J. Phys. Oceanogr., (2019), based on a different dataset (in-situ mooring observations from the OMP field program).
- As an observational oceanographer, I am impressed by the courage and determination the authors have shown in getting to grips with these two very different and I imagine bulky datasets.
- My concern is that the analysis in both papers appears to me as extremely biased, CTWs and their dynamics being proposed as the sole and only possible explanation for the observed surface currents features, the -very large- discrepancies between observations and this theoretical framework being systematically ascribed to "scattering" processes of the CTWs. In all domains of physics I know of, wave propagation models are only considered useful in situations where the order-0 phenomenon is propagation and other mechanisms such as forcing, dissipation or scattering in response to environmental heterogeneities can be relegated to higher orders. I have not seen evidence of wave propagation as a dominating phenomenon in either of these articles (except for the black line in Fig11b of the JPO paper), and I have not found a statement of the existence of CTWs as a leading-order process in the MAB in the literature cited by the authors. I am thus under the impression that the surface currents dynamics in the MAB should not be described as CTWs dynamics, perturbed by the geometry of the domain (which is the analysis angle advocated by the authors), but by a range of processes, among which CTWs may or may be. I thus consider that the authors' main claim that they have observed "scattering" of CTWs is not substantiated.
- In fact, rather than an attempt at assessing the impact of CTWs on the MAB dynamics, which does not seem very promising in the view of the paper as it stands, I think an unbiased analysis of the dataset aimed first at extracting its dominant features and identifying the dominant physical processes governing MAB dynam-

C2

ics would be more interesting. This however appears like a completely different paper, and exceeds even the scope of major revisions. The current article should thus probably be rejected, or put on hold by the authors until sufficient evidence of its relevance can be added to it.

**Specific comments:**

- an alternative to wave-mediated propagation of external forcing that could explain a coherent response at the MAB scale could be local responses to meteorological forcing coherent over the MAB scale. Has the MAB been analyzed along these lines?
- I am rather skeptical regarding the use of C-EOFs. I admit extracting propagating signals from a dataset is a difficult task, but I'm not sure C-EOFs is a satisfactory tool for that. I do not know if a R-EOF method considering as variables the band-pass filtered  $[u \ v \ du/dt \ dv/dt]$  has been proposed and studied in the signal processing literature. Such a method would introduce time variation in a computationally lighter way than time-lagged covariances, and preserve the mathematical structure of the problem, which is completely lost in the construction of the complex velocity signal.

**Technical corrections:**

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