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Interactive comment on "The mesoscale eddy field in the Lofoten Basin from high-resolution Lagrangian simulations" *by* Johannes S. Dugstad et al.

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This manuscript uses a novel approach to assess the contributions of eddies to the Lofoten Basin. In this study, Lagrangian particles are simulated numerically using the ROMS model and are diagnosed using multi-variate wavelet ridge analysis, an approach which allows the authors to readily identify the presence of coherent vortex like structures. The manuscript was written as part of the lead author's PhD thesis, which I also had the pleasure of reviewing, and in a second reading, I remain impressed by the effectiveness of the analysis approach and the clear delineation of contributions from anti-cyclonic vortices, cyclonic vortices, and ambient flow. The approach is effective,

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Discussion paper

the results are clearly presented, and the findings will be relevant to readers of *Ocean Science*.

I have considered the 15 criteria provided for *Ocean Science* reviewers, and on the whole, I think the manuscript is in excellent shape.

There are a few issues that should be addressed prior to publication.

First, the analysis in section 3.5 examines the net transport into and out of the Lofoten Basin due to ambient flow, anti-cyclonic vortices, and cyclonic vortices. Although the analysis approach is clever and original, I think that it runs the risk of over-interpreting effects. The analysis pairs separate trajectories for flow into the basin and flow out of the basin to consider the net impact on the basin. However, as the authors note, very few particles actually transition from being in an anti-cyclonic flow on entrance to a cyclonic flow on exit (or vice versa). If particles don't actually experience this change, then using a bootstrapping approach to assess the net contribution due to this unrealistic scenariio seems risky. The manuscript would be stronger if the authors simply examined the net flux into the domain from each of the categories of particles and then separately examined the net flux out of the domain from each category of particle. (Alternatively, if pairing particles at entrance with particles at exit seems imperative, then this should be done using single particles only, without randomly matching entrance particles with other exit particles.)

Related to this, at about line 390, the authors explain the use of a boostrapping routine to estimate the contributions of particles of different types to the net flux. It's not clear that a bootstrapping approach is necessarily needed for this. If the statistics are relatively Gaussian, then it should be sufficient to compute the mean temperature flux and the standard error of the mean, without needing to go through the computational effort to compute a large bootstrap sample. If bootstrapping is formally necessary, then a bit more explanation would help readers understand why.

Figures 5 and 9 are identified as probability density functions, but neither appears to be normalized so that area under the curve integrates to one. Either they should not be labeled as pdfs (perhaps "distributions of relative frequency"?) or the plotted curves should be normalized by bin width, so that integral of the area under the curve is one.

Figure 12 shows line plots that would be enhanced if statistical uncertainties could be added to the lines. This wold allow readers to judge when the LB region differs statistically from the full domain.

In Figure 2d, I'm used to seeing wavelet transforms shown with an envelope to indicate the range of validity. Is there an applicable envelope in this case?

There are a number of typos, and I will separately upload a commented version of the pdf, in which I have marked suggested edits.

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2020-103/os-2020-103-RC1-supplement.pdf

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