Author response to reviewer Stefanie Ypma's comments on "The mesoscale eddy field in the Lofoten Basin from high-resolution Lagrangian simulations" by Dugstad et al.

The reviewer's comments are given below in black Times New Roman font, with our response in red Arial font.

Reviewer 2 - Stefanie Ypma

This manuscript by Dugstad et al. presents a thorough analysis of the eddy field in the Lofoten Basin using a multivariate wavelet ridge analysis. Doing so, they've increased the understanding of the formation regions and characteristics of anticyclonic and cyclonic eddies and their respective importance for the heat transport and water mass transformation in the basin. The approach is novel, the paper is very clearly structured and written and regarding the 15 criteria provided for Ocean Science reviewers, I agree with Sarah Gille that the paper is in excellent shape. I would like to add three comments in addition to the issues already raised by Sarah Gille that should be addressed prior to publication.

We thank the reviewer for a detailed reading ad constructive comments on our manuscript. We address each comment as described below.

1. A discussion is missing on the sensitivity of your results to the spatial and temporal seeding distribution of the Lagrangian particles. You seed particles on a 40x40 rectangular grid, but what is the distance between two particles and how does that compare to the average radius of the eddies? In other words, how many particles generally reside in 1 eddy? Regarding the temporal scale, you only seed particles once every week. As you discuss that it is difficult for particles to 'enter' eddies due to high vorticity gradients, aren't you under-sampling the eddy field due to the relatively low seeding frequency?
This is an important issue. The spacing between the drifters is roughly 20 km which is comparable to the average radius of the eddies, meaning that at each seeding round, 4-5 drifters will be deployed in existing eddies (given that the diameter is about 40 km). This will unlikely lead to a problem of under-sampling. Importantly, for any time step a certain fraction of the domain is covered by eddies and a certain fraction is not. By deploying the particles uniformly, we expect the tracers to trace out the area fraction of eddies in a correct manner. This can be seen in the Okubo-Weiss parameter at time=0 in Figure 4 in the manuscript, which shows that the fraction of drifters that are deployed in a region with OW<0 is similar to the fraction of model grid cells that has OW<0 (upper green dashed line compared to green solid line). We therefore conclude

that under-sampling is not an issue, as we have enough seedings and drifters to compare with the model.

The choice of seeding every week is motivated by a hope of achieving a certain level of statistical independence. Note that we seed every week for three years, leading to 156 weeks of seeding. The longer lifetime of an eddy, the more drifters would trace it on its way, but an eddy with a lifetime of about a month would be captured by 20-25 drifters plus the ones that might enter after deployment. Given that there are many eddies that are tracked by roughly 60.000-70.000 drifters for each deployment depth (from Table 2 in the manuscript), the statistics should be well covered when we compute different characteristics of the eddies.

We thus argue that our seeding strategy can be defended. The fact that particles at early times after seeding appear to be preferentially thrown out of anticyclones and drawn into cyclones (compare blue and red dashed lines in Fig. 4) points to interesting real dynamics of which we merely speculate about here. But a typical early-time drop of particles residing in anticyclones from about 0.22 to about 0.14-15 can not explain e.g. the very small relative small contribution from anticyclones (or cyclones) to fluxes shown in Fig. 13.

But your concern of possible sampling issues is one we take very seriously. A revised manuscript will contain more detailed information about the drifter seeding in the Data and Methods section. The revised text will also raise and discuss these issues even more overtly in the Conclusion section.

2. You mention that you don't add any diffusivity, so the particle displacement is purely advective. As the Lofoten Basin is characterised by strong heat losses, there is quite some convection going on. How well can your particles describe vertical motions and temperature changes of water parcels if this convective behaviour is not included?
This is a good point, and we agree that there are issues with reproducing the 'true' vertical motion of water parcels. But first: Our ROMS model has a very high spatial resolution and also employs the GLS vertical mixing scheme. So we are fairly confident that the resolved velocity field (and hydrographic field, hence baroclinic currents) of the model is of high quality, including vertical motions associated with the mesoscale eddy field. A check of the lateral spreading, against real drifter observations was done by Dugstad et al. (2019). It is nonetheless true that our drifter trajectories lack the impact of unresolved vertical motion, including some sub-mesoscale flows and all of small scale turbulent mixing. To add vertical diffusion as a random walk would be an option. But tuning of such a random walk parametrization for realistic simulations is a

fairly involved endeavour which is undergoing active debate within the Lagrangian community.

We have here resorted to the intuitive expectation that the integrated effects of the unresolved vertical motion would primarily lead to a *larger spread* of vertical motion. The kinematic boundary condition at the sea surface is the one obvious place where the (unresolved) mixing might cause a gradual deepening—for all drifter categories (AF, AC and C). In the updated manuscript we will add comments on this issue in the Data and Methods section, in the specific discussion around vertical movement (related to e.g. Fig. 12) and also in the Discussion section.

3. Some of the figures can be improved by adding more clear labels. Comments on the figures, and some other minor comments are marked in the supplement.
Thanks for the input. We will update the labels on the axis and colorbars as you suggest. However, we believe there are some changes that will not lead to a more clear figure. One example of this is Figure 12 in the paper where you suggest we should add legends for both 2D and 3D drifters. Since 2D and 3D have the same color, we find it sufficient to only include 3D drifters in the legend and instead mention in the caption that the 2D results are dashed. This makes the legend smaller such that we can find a good fit for it in the figure.

Some comments to your suggestions/comments in the supplement:

Line 219: Would this not change if you deploy particles at the same time resolution as you have your model-output? So every 6 hour instead of every week?
No, we believe the OW-fraction would be the same independent of deployment frequency. This would only lead to more drifters, but the fraction (the relative amount of drifters in eddies compared to not in eddies) would be the same.

Line 247: How do these results (differences between AC and C's) compare to other studies that estimate eddy characteristics from observations e.g. Sandalyuk et al. (2020)? Could these results be dominated by a larger number of particles that may be residing in the Lofoten Vortex? (something that is also discussed in the Sandalyuk paper). Also, are these differences between the two eddy types something that is specific for the Lofoten Basin, or is this similar in other areas?
In agreement with Raj et al. (2016) we estimate that AC's have longer lifetimes than C's. Furthermore, our estimates indicating larger radius of anticyclones compared to cyclones are in agreement with Raj et al. (2016) and Volkov et al. (2015). We also estimate more anticyclones than cyclones in agreement with

Volkov et al. (2015) and Sandalyuk et al. (2020). You are right that the more elongated shape of cyclones compared to anticyclones can partly be due to many drifters that reside in the Lofoten Basin Eddy (which is circular and anticyclonic). However, Figure 7,e,f in the manuscript suggests that the ellipse linearity is larger for cyclones compared to anticyclones also outside the central Lofoten Basin. The elongated shape of cyclones compared to anticyclones therefore seem to be a more ubiquitous feature. In the revised text we will expand the comparison with other studies of cyclone-anticyclone asymmetries.

All of your other minor comments have been followed up on. An updated manuscript will be accompanied with detailed references to all specific changes.

Paper:
Dugstad et al. (2019): Vertical Structure and Seasonal Variability of the Inflow to the Lofoten Basin Inferred From High-Resolution Lagrangian Simulations