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Comment on bg-2022-103

Bror Jonsson (Referee)

Referee comment on "Lagrangian and Eulerian time and length scales of mesoscale ocean chlorophyll from Bio-Argo floats and satellites" by Darren C. McKee et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-103-RC1>, 2022

The manuscript analyses decorrelation in time and space from both a Lagrangian and Eulerian perspective with the ultimate aim to estimate how well Argo float act as Lagrangian platforms. The motivation for the paper is sound and it addresses some very important questions. I'm excited to use the results from a published version of the MS in future studies and believe it to have a wide potential utility. There are, however, a couple of major questions/concerns I need resolved before recommending publication.

1. The first equation suggests, to my understanding, that the Chl field is fixed in space. This is a bold assumption that needs to be carefully motivated. I would have expected the advection decorrelation term to be applied to the Eulerian observer since Chl is advected with the velocity field. One could possibly argue that biomass might originate from stationary processes at for example seam mounds, but this is rather the exception than the rule. As a consequence, I expect that a Lagrangian sampling platform in general, with some specific exceptions, experiences longer temporal decorrelation time scales compared to the Eulerian observer. I'm willing to admit that I might have misunderstood Eq1 and the reasoning around it, but I don't think I'm the only one if so. This need to either be explained better or changed.

2. The use of Chl fields with a 0.25° spatial resolution and the removal of sub- and mesoscale variability weakens the study significantly. It is abundantly clear that sub-mesoscale processes are of first-order importance in controlling the variability of Chl, as mentioned in the MS and cited publications by Amala Mahadevan or Marina Levy. A general analysis of decorrelation time- and length scales can get away with using coarser grids by defining the domain of interest carefully but this study doesn't have that luxury. One specific aim, as I understand, is to evaluate the utility of float which requires the use of the highest resolution possible. I would have preferred that a 1km product had been used (OC-CCI at 1km is for example available from Plymouth Marine Laboratory), but I understand if a 4km product is used out of necessity. Aren't the results quite dependent of the rather arbitrarily chosen 0.25° pixel size? How much would the results differ if 0.125° , 0.5 , or 1° pixels were used instead?

3. The use of geostrophic velocities to estimate QPI is problematic. There are many processes that attribute to Lagrangian decorrelation missing from these fields- I'm not even sure if Ekman drift is included? Many of these forces are also likely to affect the upper ocean to larger extent, creating an even further biasing when being omitted. One easy test is to calculate QPI for the drifters the same way as the floats to see how representative the geostrophic velocity fields are. Another option is to conduct the excercise in a high resolution ocean model using virtual drifters and floats.

4. I'm not happy with how the Chl data for the floats is handled. The mean Chl concentration in the mixed layer is not what is observed by satellite. This is of particular importance in regions with deep Chl maxima where most Chl is close to the base of the mixed layer and not visible from space. This issue can easily be amplified in this study if there is MLD variability over short timescales or if the isolines are sloping. Each case could lead to spurious variability in Chl observed by the float, compared to the drifters. The correct approach would be to use attenuation or PAR from the float (or Kd490 from satellite in if not available on the float) and average the Chl data down to the first optical depth. An even better approach would be to match satellite Chl to the floats the same way as to the drifters. I don't see any benefits in using In-situ observations for one platform and satellite-derived data for the other when comparing the two. 5. Finally, while the formalism in the MS is thorough and impressive, I think it might scare many potentially readers away. Cleaning up the text by explaining the reasoning in a way that can be understood by a wide audience and move a portion of the analytical description to an appendix would probably increase the readership statistics and potential of citations.