Lagrangian-Eulerian statistics of mesoscale ocean chlorophyll from Bio-Argo floats and satellites

Darren C. McKee[1], Scott C. Doney[1], Alice Della Penna[2,3], Emmanuel S. Boss[4], Peter Gaube[5], Michael J. Behrenfeld[6], David M. Glover[7]

[1]Department of Environmental Sciences, University of Virginia, Charlottesville, VA, 22904, USA
[2]Institute of Marine Science, University of Auckland, Auckland, New Zealand
[3]School of Biological Sciences, University of Auckland, Auckland, New Zealand
[4]School of Marine Sciences, University of Maine, Orono, ME, USA
[5]Applied Physics Laboratory, University of Washington, Seattle, WA, USA
[6]Department of Botany and Plant Pathology, Oregon State University, Corvallis, OR, USA
[7]Department of Marine Chemistry and Geochemistry, Woods Hole Oceanographic Institution, Woods Hole, MA, USA

Author Comments in response to Referee #1

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 coupleof major questions/concerns I need resolved before recommending publication.

Thank you for your close read and evaluation of our manuscript.

Before we address each of your comments individually, we would like to preface with an overview. The matters you bring up in your comments 1-3 are related by a single overarching point that we perhaps did not make clear enough in our original manuscript. Our intention in this study was to analyze time- and length-scales of mesoscale ocean chlorophyll variability (and velocity). This choice of scale dictated our choices of data products and filtering. Given this, our responses to your comments 1-3 are related.

1. The first equation suggests, to my understanding, that the Chl field is fixed in 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.

Equation (1) is simply a material derivative "budget" for chlorophyll and does not make any assumptions about the properties of the chlorophyll field. In our reading, nothing about the presentation of Equation (1) prescribes a behavior for the chlorophyll field. When the equation is scaled (equation (5)), Eulerian scales are indeed used for the advection term. The discussion that introduces Equation (1) (lines 84-107) is based on the velocity field, for which the theoretical and observational studies cited show that, for velocity, it is true that $T_L < T_E$. However, prior to conducting this study, it was not known whether there was any systematic relationship between $T_{L,\mathrm{Chl}}$ and $T_{E,\mathrm{Chl}}$. By scaling Equation (1) with scales derived from the chlorophyll fields and Lagrangian or Eulerian chlorophyll time series, we are able to consider how the movement of an observer relative to "movement" of the chlorophyll fields (u' versus $L_{E,\mathrm{Chl}}/T_{E,\mathrm{Chl}}$) influences the Lagrangian decorrelation time.

Like you, we were surprised to find $T_{L,\mathrm{Chl}} \leq T_{E,\mathrm{Chl}}$. We suspect there are several possible origins for this behavior. Firstly, this may be the manifestation of an observer moving across existing gradients in the chlorophyll field, as would happen when a mesoscale eddy stirs a horizontal gradient. The empirical curves in Figure 6 (from equation 11) would support this, as described in Section 4.5. Another possibility is that chlorophyll may actually be conserved for longer along a trajectory than our results would indicate: if patches are organized in small scale filaments that are not fully resolved in an 0.25º product, the inability for a drifter-projected time series to resolve such near-constant chlorophyll levels along a filament will result in an early temporal decorrelation. The result that the ratio $T_{L,\mathrm{Chl}}/T_{E,\mathrm{Chl}}$ is approximately 1 relative to the smoothed subtrahend (where sub-pixel variability probably dominates) while the ratio is less than 1 relative to the climatology subtrahend (where larger and/or slower processes dominate) supports this interpretation. We plan to add the preceding discussion to the manuscript.

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 submesoscale 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?

We acknowledge that submesoscale processes are of first-order importance in driving surface chlorophyll variability. Further, we do believe that our results are dependent on the ocean color pixel size. That being said, the choice of 0.25º was not arbitrary, and we believe our results are still novel, meaningful, and useful. As indicated by the title (though perhaps the manuscript needs to more clearly convey this), our interest is in studying the mesoscale chlorophyll field. Our motivation for this interest is both a practical and intellectual matter.

As a practical matter, there is a tradeoff between resolving more variance and dealing with increased gaps when moving to a finer resolution ocean color product. For the purpose of this study, we chose to prioritize data coverage, leading us to select a blended, 0.25º product, and a focus on the mesoscales. As an additional practical matter, given the relatively sluggish motion of floats (as quantified in this study), they may not capture the full spectrum of submesoscale processes. Given that we wanted to incorporate in-situ data in this study, we felt it best to focus on mesoscale variations. The choice of product is also consistent with the grid size of the

Eulerian velocity field (0.25º altimetric geostrophic currents), and we aimed for consistency since we compare the two variables.

Intellectually speaking, combined Lagrangian-Eulerian scales of chlorophyll are unknown at any scale, and we believe that contributions at the mesoscale are useful. New results are still being gleaned about geostatistics of the mesoscale chlorophyll field and their biophysical origin (e.g., Eveleth et al., 2021). We believe the results here stand on their own and our mesoscale study may lay the groundwork for follow-up studies targeting the submesoscale, either utilizing a more spatially or temporally expansive drifter and ocean color dataset or a high-resolution model.

Finally, we do suspect that our results are dependent on the choice of ocean color product resolution. We suspect that the major consequence is that $T_{L,\mathrm{Chl}} \leq T_{E,\mathrm{Chl}}$ for the reasons outlined in our response to your comment (1).

We plan to include a more detailed discussion of why a 0.25º product was utilized and specifically delineate what the limitations of this choice are and how it likely influences our results (incorporating the last paragraph of our response to your comment (1)). In a revised introduction we plan to clearly motivate an analysis of mesoscale variability as done here, and in a revised conclusion we plan to recommend subsequent studies of submesoscale variability as done here.

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.

The QPI is calculated using trajectories computed from the global altimetry product, which includes a geostrophic term based on sea level anomalies and nothing else. The study of Della Penna et al. (2015) that developed the QPI compared distributions of QPI for SVP (real) drifters using trajectories calculated from different altimetry products (see their Supplementary Information Figure 4), including a global altimetry product (geostrophy only), a regional altimetry product (geostrophy only), and a regional Ekman corrected product (geostrophy + Ekman). Their conclusion was that "[using] different products does not alter significantly the shape and the extent of the [distribution of QPI], yet differences in the distributions can be observed in the tails". Though their study was performed in the Southern Ocean and though they compare trajectories in a slightly different manner than we do, we took this to mean a geostrophic term would likely dominate the trajectories, especially in the vicinity of the Gulf Stream and North Atlantic Current where a geostrophic balance is generally reasonable. Our choice was further motivated by our desire to study mesoscale variations in the velocity fields, which the altimetric geostrophic fields are known to capture reasonably well.

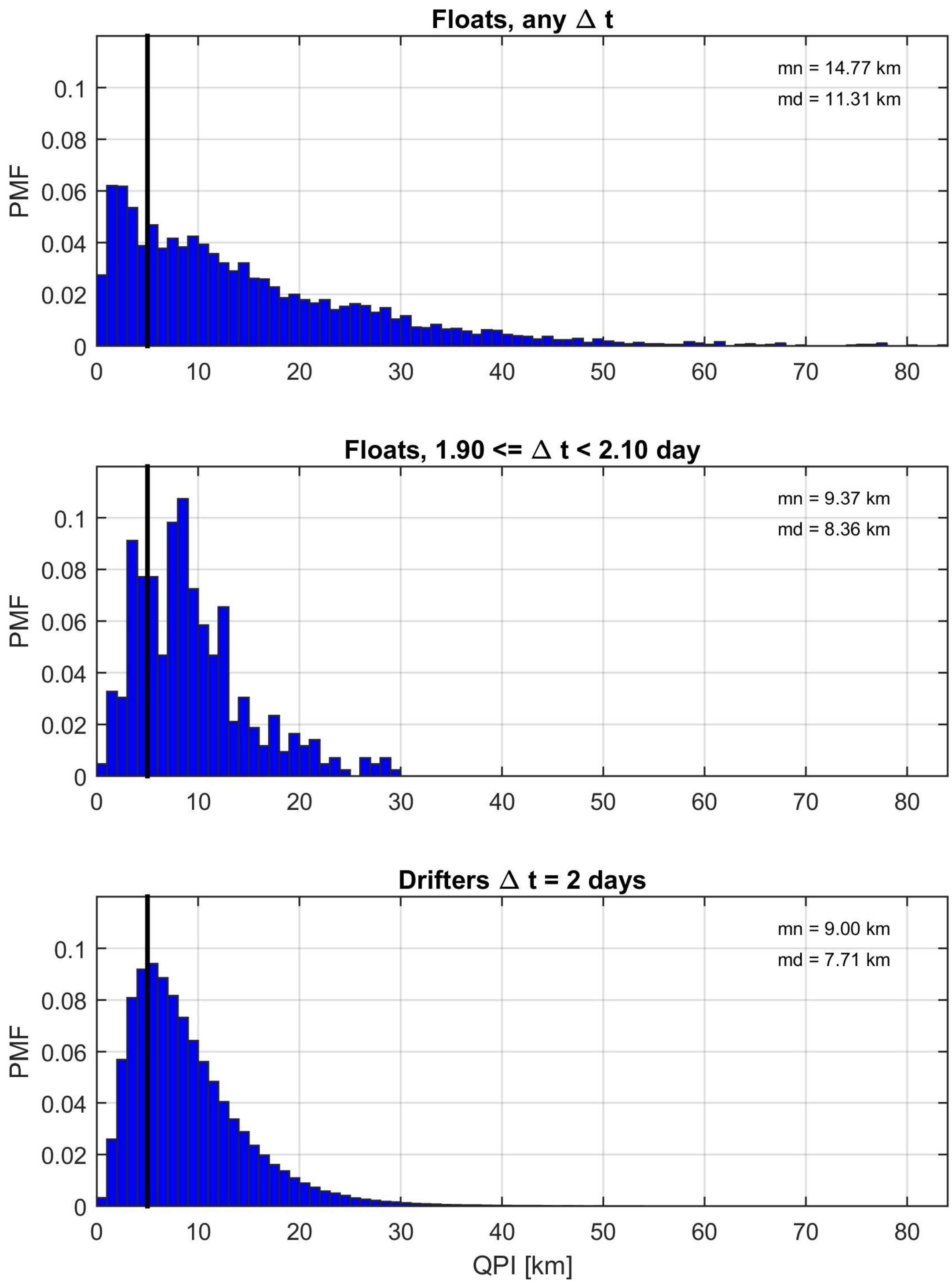

Figure R1: Probability mass functions (PMF) of QPI for all floats (top panel), floats with $\Delta t \approx 2$ days (middle panel), and drifters with $\Delta t = 2$ days (bottom). Vertical lines represent 5 km.

We computed the QPI for all of our drifter returns that fall in the study domain of [30N, 65N, 300E, 340E] and [2003-01-01, 2016-12-31] and used our daily subsampling, so that $\Delta t = 2$ days since trajectories are compared at [t_{i-1}, t_i, t_{i+1}]. The distributions of drifter QPI are shown in Figure R1 (third panel). We found two things surprising. Firstly, the median over all drifters is larger than expected at about 8 km. Secondly, the distribution of float measured QPI when restricting to profiles with $\Delta t \approx 2$ days is very similar (Figure R1, second panel), with only a slightly larger median. Inspecting QPI as a function of latitude (averaged in 5º bins) for the two platforms reveals that, while the distributions are similar when including all samples, the latitudinal variations are different. Each platform has a maximum near the Gulf Stream (40-45ºN) and a minimum at 50-55ºN or 55-60ºN, but the QPI is more variable for the floats, with a maximum larger than that for drifters and a minimum that is smaller (even though, presumably, floats are less Lagrangian with respect to the surface flow) (Figure R2). As we know that floats tend to lag the surface flow, the energetic and sheared currents of the Gulf Stream may exaggerate this difference, causing the very large QPI there. On the other hand, deeper mixed layers and more sluggish currents at higher latitudes may cause a relatively smaller QPI for the floats at higher latitudes. Another possibility is that Ekman transports become important farther north away from the Gulf Stream, and the deeper floats are sheltered from this flow, instead primarily feeling the geostrophic flow and yielding a relatively smaller QPI compared to drifters, who feel the total current. However, given the relatively stable distribution of drifter QPI with latitude, we suspect lack of including an Ekman term amounts to a small difference, in line with the findings of Della Penna et al. (2015). Instead, deviations for the drifters are probably primarily due to sub-map-grid scale processes or altimetric geostrophic currents generally underestimating surface flow due to the finite differences being computed from a product that has been mapped from the actual altimetry swaths (Ascani et al., 2013; Sudre and Morrow, 2008).

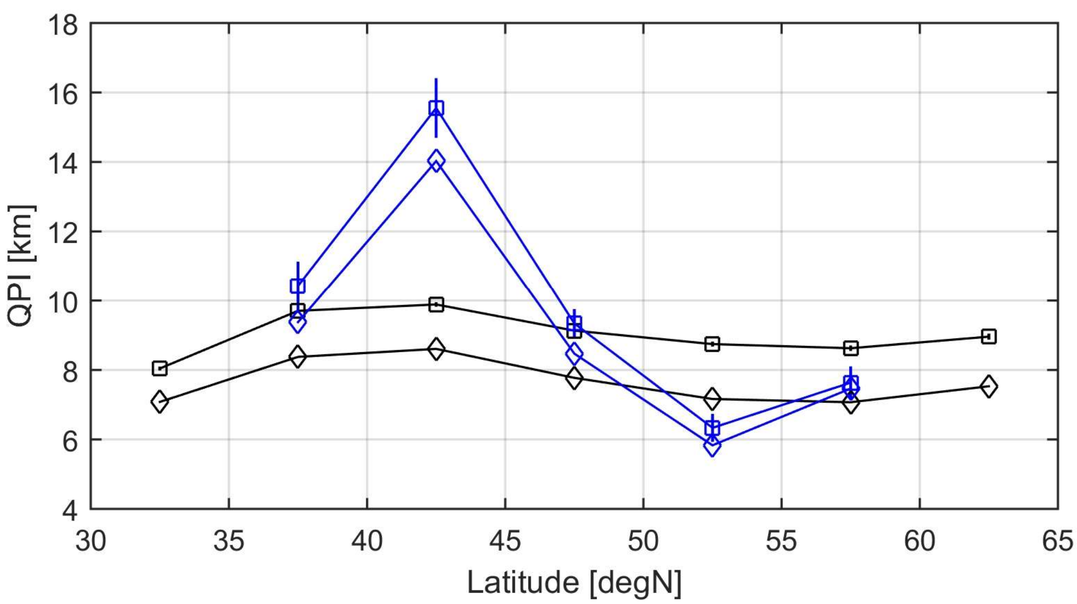

Figure R2: QPI as a function of latitude for drifters (black) and floats with $\Delta t \approx 2$ days (blue) in 5º latitude bins. Squares with vertical line are means ±1 standard error. Diamonds are medians.

All that said, we still think that the QPI as we calculate it (deviations from a surface geostrophic trajectory) is reasonable for our study mainly because, as indicated (though we will clarify), we have set out to conduct a study of mesoscale Eulerian-Lagrangian time and length scales. For example, the velocity scale analysis (Figure 5) is based on Eulerian scales from satellite altimetric geostrophic currents and drifter velocity time series filtered to remove super-inertial variability. If we take that geostrophic altimetric fields are a reasonable approximation for the mesoscale flow, then it is reasonable to us to emphasize float segments whose trajectories are similar to trajectories subject to that flow.

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.

We have computed an alternate depth-reduced chlorophyll series from the floats that is meant to better approximate what the satellites see. About 90% of the satellite-measured chlorophyll signal in the open ocean comes from a depth of $1/Kd_{490}$, and it is exponentially weighted in the vertical (Gordon and McCluney, 1975). Our new approach is to conduct a weighted average over one attenuation depth. First, we estimate Kd_{490} from the floats following Morel et al. (2007) (their equation 8),

$$Kd_{490} = 0.0166 + 0.0773[\mathrm{Chl}]^{0.6715}, \qquad \text{(R1.1)}$$

where we take [Chl] as the mixed-layer average chlorophyll. Then, we take a weighted vertical average at each time step as

$$\mathrm{Chl}_{\mathrm{float}}(t) = \frac{\sum\limits_{z=1/Kd_{490}}^{z=\mathrm{surface}} \mathrm{Chl}(z,t)\exp\left(-2Kd_{490}(t)z\right)}{\sum\limits_{z=1/Kd_{490}}^{z=\mathrm{surface}} \exp\left(-2Kd_{490}(t)z\right)}. \qquad \text{(R1.2)}$$

We use a weighted sum instead of integral because some profiles have missing data near the surface. The series is then log transformed and filtered as before. In general, the two time series (R1.2 and the MLD-average used in the original manuscript) are very similar with some discrepancies at daily to subdaily fluctuations; however, these are generally removed with the subdaily filter (compare Figure B1 to attached revised Figure R7). We then reran all scales and provide here a complete set of figures (equivalents to Figures 6-9, B1, D1). The results are not appreciably different. We would be fine with using the new method of depth reduction.

As for using float-measured data instead of projecting satellite data onto float trajectories, we see great value in using in-situ observations. Firstly, it is not possible to do this analysis with satellite data projected onto the floats, given the limited number of floats available. As you can see in Figure B1 (squares), there are many gaps when projecting satellite ocean color onto a float trajectory, even when using a nearest neighbor approach as is done in that figure, which is more generous than the preferred bilinear interpolation used for drifters in the paper. This issue is not prohibitive when working with the drifters because there is a tremendous number of them, so even though individual segments are sparse and offer few pairs at a given lag, a composite ACF can be constructed from many sparse segments. Secondly, we feel it is instructive to demonstrate what can be learned from a near-continuous, in-situ time series, as this represents as complete a dataset as is possible and is what most float users will work with.

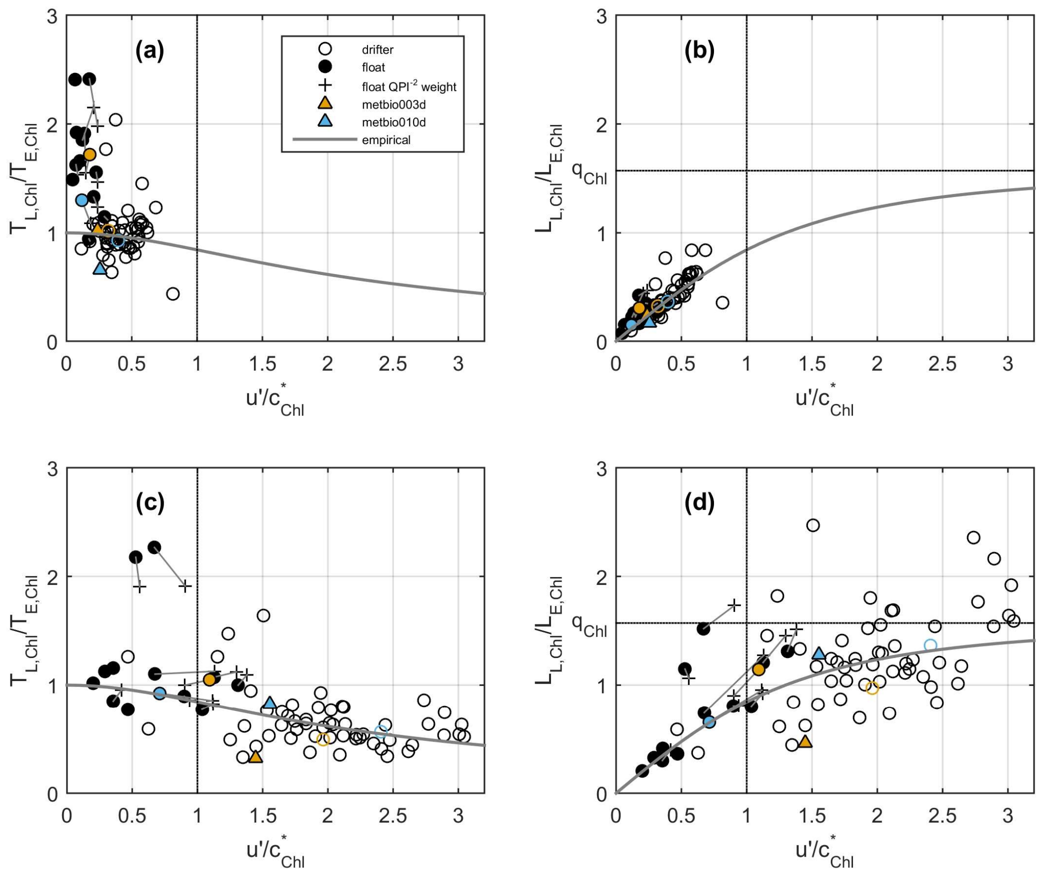

Figure R3: Revised Figure 6 from manuscript using new definition of float chlorophyll series.

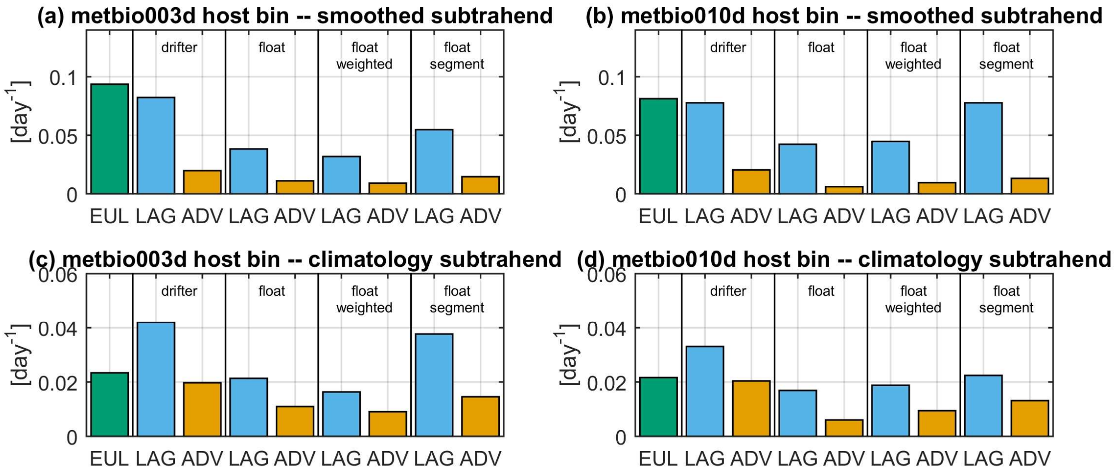

Figure R4: Revised Figure 7 from manuscript using new definition of float chlorophyll series.

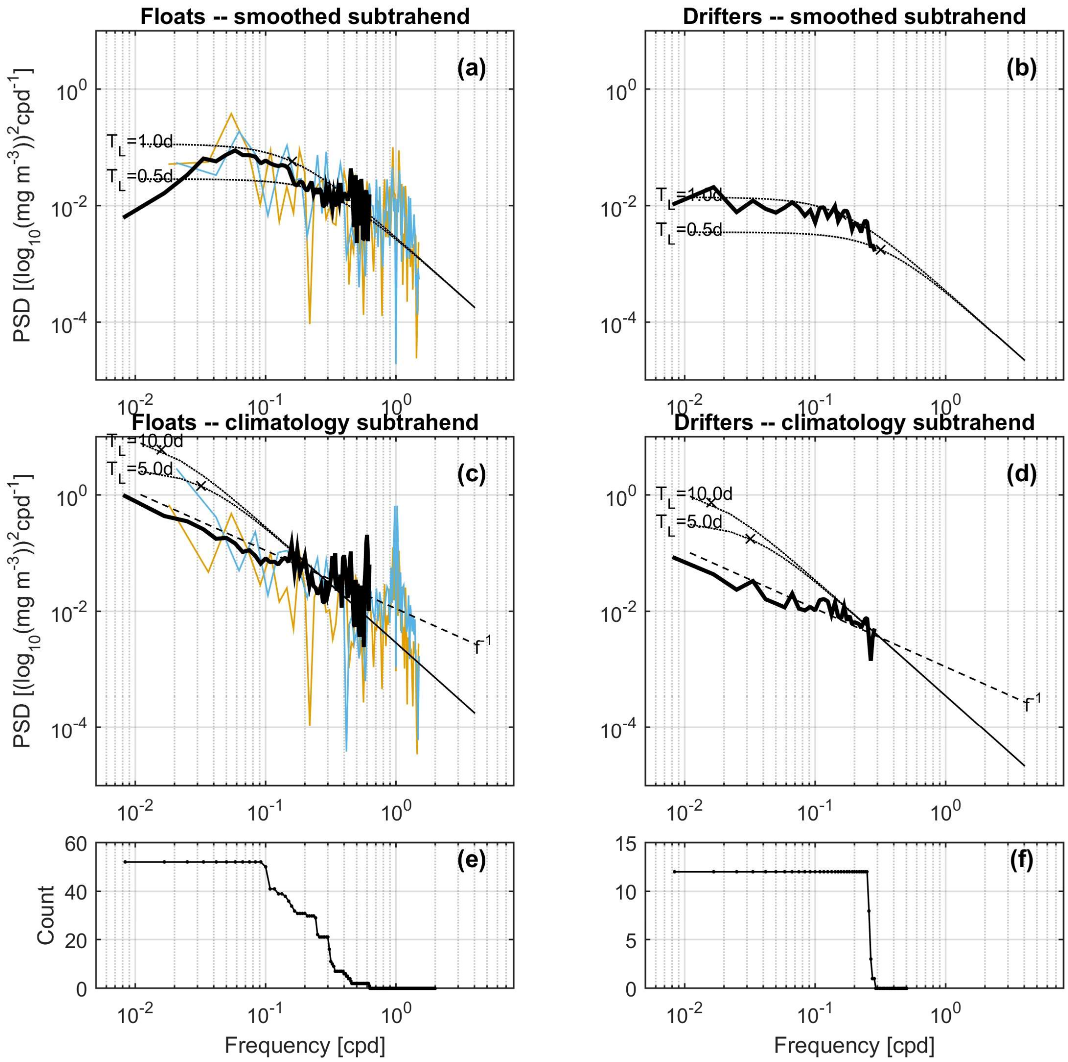

Figure R5: Revised Figure 8 from manuscript using new definition of float chlorophyll series.

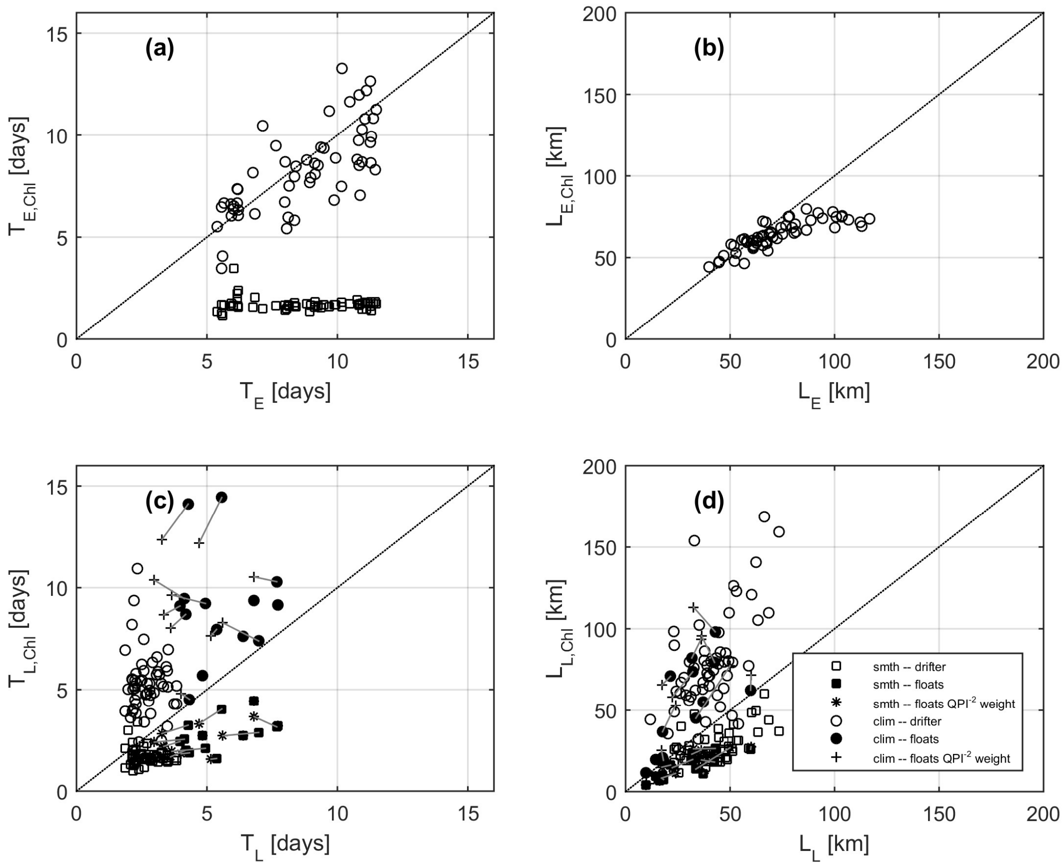

Figure R6: Revised Figure 9 from manuscript using new definition of float chlorophyll series.

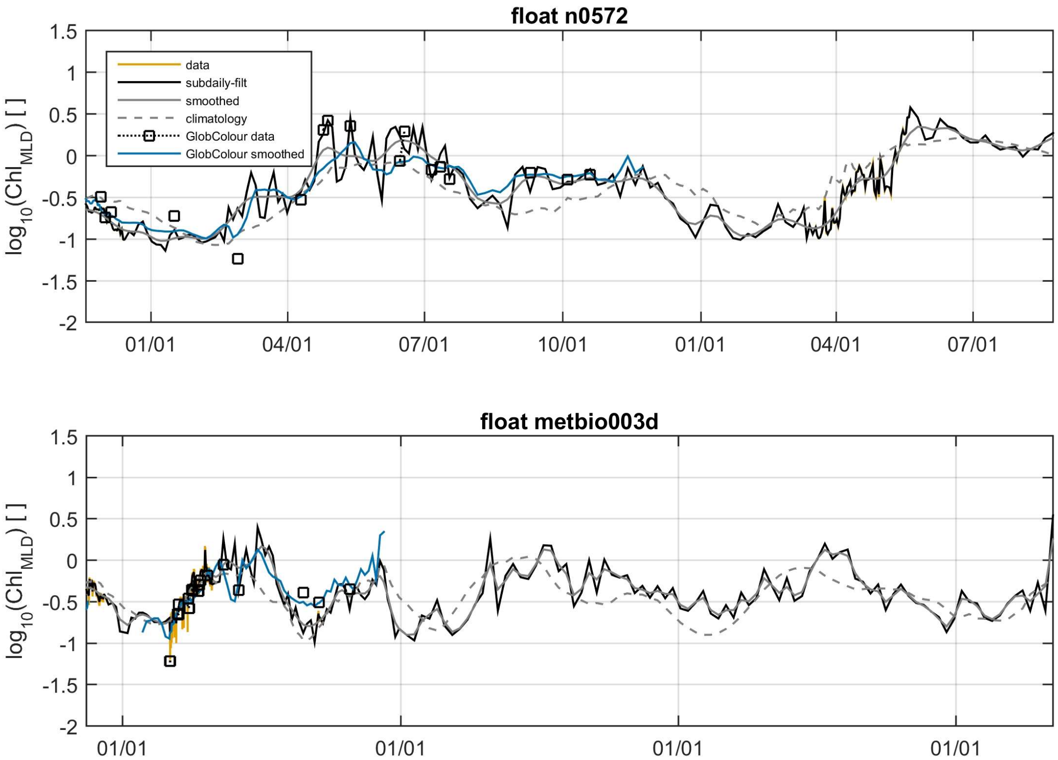

Figure R7: Revised Figure B1 from manuscript using new definition of float chlorophyll series.

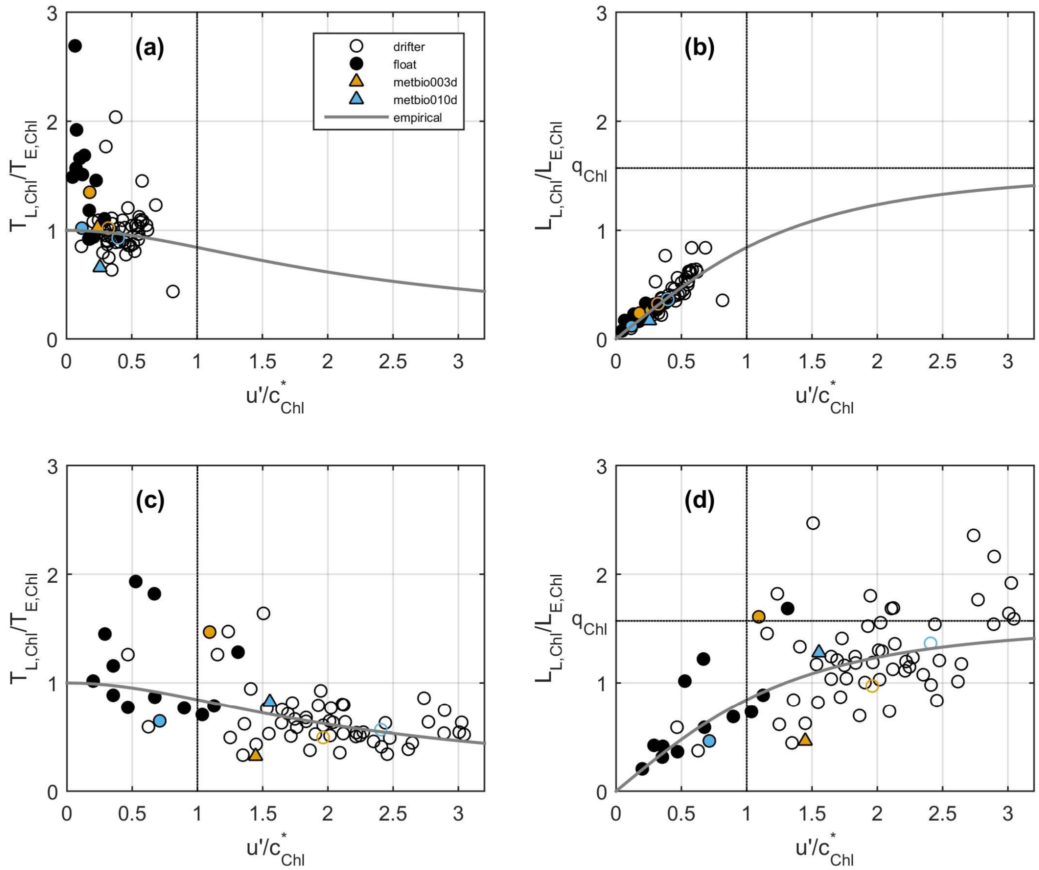

Figure R8: Revised Figure D1 from manuscript using new definition of float chlorophyll series.

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.

This is a good suggestion for such a technical paper. Already, in our initial submission we've made great effort to move non-essential information to appendices (there are already five). In our reading, though there is a deep theoretical exposition in Section 2.1 and a lot of methodological detail in Sections 3.3-3.5, we believe that the information retained in the main text is essential for evaluation of the paper. However, we agree that the exposition can be improved. Referee #2 made some good suggestions on how to enhance the clarity of (and reduce length of) sections 3.3 and 3.4. We believe that by addressing those issues and by more clearly motivating our analyses, readability will improve.

References:

Ascani, F., Richards, K. J., Firing, E., Grant, S., Johnson, K. S., Jia, Y., Lukas, R., and Karl, D. M.: Physical and biological controls of nitrate concentrations in the upper subtropical North Pacific Ocean, Deep Sea Res. Part II Top. Stud. Oceanogr., 93, 119–134, https://doi.org/10.1016/j.dsr2.2013.01.034, 2013.

Della Penna, A., De Monte, S., Kestenare, E., Guinet, C., and d'Ovidio, F.: Quasi-planktonic behavior of foraging top marine predators, Sci. Rep., 5, 18063, https://doi.org/10.1038/srep18063, 2015.

Eveleth, R., Glover, D. M., Long, M. C., Lima, I. D., Chase, A. P., and Doney, S. C.: Assessing the Skill of a High-Resolution Marine Biophysical Model Using Geostatistical Analysis of Mesoscale Ocean Chlorophyll Variability From Field Observations and Remote Sensing, Front. Mar. Sci., 8, 1–10, https://doi.org/10.3389/fmars.2021.612764, 2021.

Gordon, H. and McCluney, W.: Estimation of the depth of sunlight penetration in the sea for remote sensing, Appl. Opt., 14, 413–416, https://doi.org/10.1364/AO.14.000413, 1975.

Morel, A., Hout, Y., Gentili, B., Werdell, P. J., Hooker, S. B., and Franz, B. A.: Examining the consistency of products derived from various ocean color sensors in open ocean (Case 1) waters in the perspective of a multi-sensor approach, Remote Sens. Environ., 111, 69–88, https://doi.org/10.1016/j.rse.2007.03.012, 2007.

Sudre, J. and Morrow, R. A.: Global surface currents: a high-resolution product for investigating ocean dynamics, Ocean Dyn., 58, https://doi.org/10.1007/s10236-008-0134-9, 2008.