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

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Author Comments in response to Referee #2

This manuscript presents extensive work evaluating Eulerian and Lagrangian time and length scales of velocity and chlorophyll, as well as discussion about how they correlate. The proper interpretation of drifting phytoplankton observed in a Eulerian fashion is a longstanding paradigm in ocean ecology. However, estimates of Lagrangian phytoplankton statistics and comparisons with Eulerian counterparts are rare. This study represents a significant contribution towards best understanding how to interpret phytoplankton/chlorophyll measured in both Eulerian and Lagrangian platforms. The authors are very thorough in their analysis and description of the results. Nonetheless, I have a few comments to be addressed prior to recommending publication.

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

Major comment: There are several data limitations that guide methodological decisions in an analysis of this type (e.g., the broad spatial averaging, chlorophyll averaging in the MLD). While some of the issues arising from these are mentioned briefly throughout the text, I would prefer to see a dedicated discussion section with the limitations and caveats.

The averaging of scales (or compositing of ACFs) over [5º x 5º] space bins is meant to enhance the quality of the estimates by averaging over a region that is relatively spatially homogenous. Other authors doing a similar analysis of velocity in this region used [10º x 10º] space bins and found this adequate to describe spatial variability in Lagrangian scales (Lumpkin et al., 2002). We chose to use the same [5º x 5º] space bins as Glover et al. (2018), who calculated variograms of satellite ocean color in each bin much like we compute ACFs and found these bins good to resolve spatial variability. We will better motivate this in the text.

As for the depth reduction of chlorophyll, we had indeed used a simple average over the mixed layer since other authors had done this and demonstrated good agreement with satellite ocean color when describing seasonal variability in the region (Yang et al., 2020). At the suggestion of Referee #1, we computed an alternate depth-reduced chlorophyll series from the floats that is meant to better approximate what the satellites see. Please refer to our Author Comment to Referee #1 for full details, but briefly, we utilize the fact that 90% of the satellite-measured chlorophyll signal in the open ocean comes from a depth of 1/Kd490, and it is exponentially-weighted (Gordon and McCluney, 1975). We estimate Kd_{490} from the floats following Morel et al. (2007) (their equation 8),

$$Kd_{490} = 0.0166 + 0.0773[\mathrm{Chl}]^{0.6715}$$

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

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

The series is then log transformed and filtered as before. After rerunning all scales (please refer to set of figures in Author Comment to Referee #1), the results are not appreciably different. We will include a description of this comparison in the text.

As for the choice of ACF parameters in Table 1, please refer to our response to your comment below.

We plan to consolidate all of the above matters (spatial averaging, depth averaging, ACF parameters) into a subsection of the Discussions, as you suggest.

Specific comments:

I find that, while technically correct, talking about Lagrangian-Eulerian “statistics” in the title and throughout the text can be misleading. Why not refer to the specific statistics that are included in the analysis? i.e., Lagrangian-Eulerian time and length scales.

We felt that use of the word “statistics” made for a more compact title, with the meaning becoming clear after reading the abstract. But we do not object to changing the title to: “Lagrangian-Eulerian time and length scales of mesoscale ocean chlorophyll from Bio-Argo floats and satellites”.

The notation of upper case L for both Lagrangian and length-scale can be a bit confusing. I suggest using upper and lower case or a different notation to improve readability.

We agree about the confusion. We thought about using upper and lower case letters but this can become problematic since a lower case “l” can look like a number 1 or capital “I” or something else. We propose to maintain “T” and “L” for scales and replace subscripts “L” and “E” with either “l” and “e” or “LAG” and “EUL” for Lagrangian and Eulerian, respectively, depending on which of the two looks best.

Equation 5. Terminology becomes confusing here too when calling the nominators Lagrangian, Eulerian and Spatial (chlorophyll) scales. Is there a different name that could be more appropriate and less confusing? This is essentially a change in chlorophyll, correct?

These are effectively standard deviations of chlorophyll computed in different frames: from Lagrangian time series (subscript “*LAG*” or “*l*”) from Eulerian time series (subscript “*EUL*” or "*e*"), or from spatial maps (subscript "spatial"). Though less than satisfactory, we cannot think of a better notation to express this point. However, we could add to the text the literal definition of each term as supplied here in our Author's Comment document.

Table 1 is also confusing. Why are ACF bins different? Why are time windows for Eulerian and Lagrangian different? Does that have any effect on the comparison? (I think it would if you were calculating other statistics). Where does the 27.8km ACF bin for Eulerian length scale come from? I probably missed it.

As indicated in your Major Comment above, we plan to better motivate these choices and consolidate them into a subsection of Discussions that will cover all methodological choices. Briefly, we address your specific questions here.

Regarding temporal ACF bin sizes: Ideally, one would use a bin size that matches the sampling interval of the time series because this is the smallest lag that can be resolved. For this reason, the ACFs based on satellite altimetry, satellite ocean color, or drifters use a bin size of 1 day. The floats have a variable profiling interval. While they sometimes profile with a frequency of about 1 per day, they generally profile less frequently and we found a bin size of 5 days to be a reasonable choice (with smaller bin sizes, many segments would offer no pairs). As we state, the two metbio* float segments are given special attention because they profile more frequently, and for that reason we were able to use a finer bin size of 1 day. As a general statement, choosing a larger bin size for the ACF causes structure (curvature) of the ACF to be poorly resolved at short lag and biases time scales large. This point is brought up in section 4.2.

Regarding temporal segment lengths for ACF analysis: The Lagrangian segments should be kept as short as possible because as a platform moves it may encounter different environmental (physical or otherwise) conditions, and we found 120 days was a reasonable length of time. For Eulerian segments, this is not an issue, and, since seasonal variability is removed, length of the segment is generally unimportant. Given that, we used 365-366 day segments for chlorophyll out of convenience since the data were stored as yearly files.

Regarding spatial ACF bin size: Related to our point about temporal ACF bin sizes, it would be best to use a bin size equal to the data spacing. We chose 27.8 km as that approximately corresponds to the 0.25^{o} resolution of the data in the latitudinal direction. Obviously, pairs spaced zonally may have a separation less than that distance and would fall into the first bin.

Figure 1d. orange profiles: QPI<5km; blue: all others (i.e., not total)

You are correct. The total height of the bars represents the total number of profiles, but the blue region represents only the portion with QPI > 5 km. We will fix this.

Line 180. Please specify the convention for flagging. It is my understanding that BGCArgo flagging may have changed through the years and between institutions. (I've used Sprofs where 3 means bad).

We will provide a brief description here and include reference to the relevant Argo user manual for more information.

How does the GlobColour product compare to other products? Why is this one selected over others? (OCCCI, for instance). I suggest including a brief sentence.

We did not compare how different satellite products affect the scales that are calculated. We chose GlobColour because it is blended from all available satellites and is therefore probably a most complete product in terms of space-time coverage without interpolation. Further, the study of Zhang et al. (2019) demonstrated that GlobColour data projected onto surface drifter tracks resolve realistic Lagrangian behavior in terms of (sub)mesoscale dynamics, so we conclude that their space-time information is biophysically accurate. We plan to include this information in the manuscript, in the Methods section where we introduce the data. As for why we chose to use a 25 km product, that requires a more nuanced discussion and we refer you to please see our Author Comment to Referee #1. We propose to include that discussion in a revised Discussions section.

Section 3.3 could be simplified. Two sets of chlorophyll anomalies are estimated: 1. Anomalies with respect to a 31-day smoothing filter, and 2. Anomalies with respect to the climatology. I would suggest stating something like that to start, and then continue with the details.

This is a reasonable suggestion, and we can modify the opening sentence of section 3.3 accordingly.

The climatology is based on the same 31-day filter + a boxcar function? This is not exactly what comes to my mind when "climatology" or "repeating annual cycle" is mentioned.

We apologize for the confusion here. This is an admittedly technical point so we left the details in the Appendix B, but perhaps we need to clarify the main text. Essentially, the "smoothed" subtrahend is from a 3-D convolution with a filter kernel that is a 2-D Gaussian in space and a 31-day Hamming window in time. The "climatology" subtrahend comes from first stacking the arrays by day of year in a 4th dimension so that the convolution is with a 4-D kernel that is a 2-D Gaussian in space, a 31-day Hamming window in day-of-year (like a Julian day, not absolute calendar date), and a boxcar (equal weights) across years. That way, as we say in the Appendix B, "[for] example, January 1 of every year is regarded as having the same time coordinate". The end result is a set of maps for each day-of-year, hence making it a repeating annual cycle. We can move the illustrative sentence (reproduced here) to the main text for clarity.

I don't like the use of the satellite-based "subtrahend" to estimate chlorophyll anomalies from the MLD-averaged chlorophyll from the float. How does the MLD average compare to the satellite? I think some type of bias correction may be needed. You mention that the subtrahend is regressed against float data. Do you mean you corrected a bias? That should be included in a supplement.

It is not possible to construct a "climatology" subtrahend from the floats because there is not enough interannual coverage of floats over the spatial footprint of the horizontal component of the filter at any given time step. For this reason, we need to turn to climatological fields constructed from the satellite data. To illustrate how the subtrahends look (and how float and satellite data compare), we included Figure B1. We think that figure illustrates that the satellite-constructed "climatology" subtrahend is reasonable to compare with the float data. As you point out, the regression effectively serves as a bias correction so that the mean and range of the subtrahend (once it is projected onto the floats) is comparable to the mean and range of the float-measured chlorophyll. The details of the procedure are described in the existing Appendix B. If it is helpful, regression coefficients can be included.

Line 240. Why aren't Eulerian and Lagrangian segments equal?

This is a matter of convenience. The Lagrangian segments should be kept as short as possible because as a platform moves it may encounter different environmental (physical or otherwise) conditions, and we found 120 days was a reasonable length of time. However, this is not important for Eulerian segments, especially since low frequency (such as seasonal) variability has been removed. Since the Eulerian data are stored in annual files, it was easiest to work with year-long segments.

Section 3.4 could be simplified as well. If I understand correctly, you tested two approaches to estimate spatially averaged scales. In lines 272-275 you mention you use one or the other. When and why you use each one should be clearer.

This is basically correct. When possible, we apply both methods (e.g., compare Figures 6 and D1), but only equation 8 is an option for any scales derived from ocean color due to the large number of gaps. In simplest terms: "All scales are derived by averaging in space (from integrating Eq. (7) and averaging), except any scales involving satellite ocean color, where large numbers of gaps require computing scales from space-composited ACFs (from integrating Eq. (8))." We can open the discussion on lines 272-281 with the preceding simple sentence and then eliminate much of the redundant (and less clear) text that follows.

Line 292. Picks?

Sorry: "picks" should read "scales".

Lines 319-320: "If we take …" this sentence is confusing.

We apologize for the confusion here. Our intention is to draw some contrast between float profiles where the QPI is "small" and "large". While the distribution in Figure 2 is continuous and there is no real threshold, we noted that there is a mode of profiles between zero and 5 km, so we chose this threshold for display purposes. As we mention in the text, 5 km is a good compromise between having a large amount of profiles and having a QPI that is small, so it serves as a reasonable threshold between a "small" and "large" QPI for the purposes of display in Figures 2-3. Other than for display purposes in those figures, though, QPI is only used for weighting averaged scales and there is no use of a threshold in Figures 4-9 or their interpretations. We can update the text with the information supplied in this Author's Comment document to clarify where the 5 km threshold comes from and when and why it is used.

I probably missed this. Are the results in figures 5 to 9 based on all profiles or only QPI<5km?

We apologize for the confusion here. Figures 5 to 9 display results based on all float profiles. We can update the captions to convey this. The filled circles treat all float segments equally in the averages whereas the crosses weight by segment-median QPI^{-2} so that segments with smaller QPI count more.

References:

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