Lagrangian-Eulerian time and length scales of mesoscale ocean chlorophyll from Bio-Argo floats and satellites

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Format of this document:
Black = Referee Comments; Blue = Author Comments and description of how manuscript has changed.

Author Comments in response to Referee #1

Thank you for your response to my comments and questions. I think there is a nice study hiding in the manuscript, but that there is still some work to find it.

Thank you for your careful review of our revised manuscript.

The study addresses important questions and suggests an interesting framework for comparing Lagrangian and Eulerian scales, but while I respect the intention by the authors to only include mesoscales, I think this constraint has to be communicated more clearly. I originally assumed that the main story was to assess if Argo floats can be assumed Lagrangian when sampling Chl, but such analysis would need to include all scales that can be observed. I now realize that this assessment is of lower priority and that you mainly focus on understanding how Eulerian and Lagrangian timescales compare over mesoscales. This focus is of course valid, but the abstract, introduction ,and conclusions should be rewritten to deemphasize the question about the utility of Argo floats and if they can be considered Lagrangian. Also, please be careful when providing estimates of timescales of decorrelation since these are calculated for a simplified world without sub-mesoscale processes.

That is a correct assessment of our intention: we primarily aim to understand how Lagrangian Chl scales relate to Eulerian Chl scales, and how the velocity field provides the link. Judgment of floats' suitability in Lagrangian analysis is a secondary aim. We have revised the text to de-emphasize an assessment of the suitability of profiling floats in Lagrangian analysis through the following changes: (1) We removed the last two paragraphs of the conclusions; (2) We removed the last sentence of the abstract and replaced it with a comment on the importance of stirring for setting Lagrangian scales, which is a conclusion related to our primary aim. Nevertheless, because we use floats as a tool, an assessment of their behavior is necessary, as is some introductory description of how they sample. Therefore, we haven't changed the introduction or other sections.

Secondly, our approach revolves around calculation and interpretation of scales computed from data. However, the abstract, introduction, and conclusions (even the title) indicate that they are computed from mesoscale-resolving (or filtered) data. Therefore, we prefer to refer to scales as simply "integral" or "decorrelation" scales. Discussions (Sect. 4.2-4.6) and Conclusions (Sect. 5) are clearly framed in terms of mesoscale processes and clearly discuss implications of analyzing mesoscale-resolving data (or equivalently, data that do not resolve the submesoscale).

I am still quite concerned about your definition of material derivatives and the consequence it has on your results. Eq 1, as it is stated now, is correct when describing the material derivative of a field which is fixed in space, for example the temperature gradient in a small lake (https://en.wikipedia.org/wiki/Material_derivative) or a stationary velocity field as used by Middleton (1985). I don't think it's correct for Chl in the open ocean which will be advected together with the Lagrangian reference point though. Here, the material derivative in a Lagrangian frame is
$\frac{D Chl}{D t} = \frac{\delta Chl}{\delta t}$
And in a Eulerian frame
$\frac{D Chl}{D t} = \frac{\delta Chl}{\delta t} + \boldsymbol{u} \nabla Chl$
Please see for example eqs 1 and 2 in Chenillat 2015 (https://www.frontiersin.org/articles/10.3389/fenvs.2015.00043/full) or section 1.2.2 in https://www.usc.es/export9/sites/webinstitucional/en/investigacion/grupos/gfnl/documents/thesis/tesis_Florian.pdf. The paragraph on lines 85-97 is a bit confusing due to this. I read it as starting with talking about Chl, making a statement based on the material derivative of velocity in the middle, switching back to to talk about Chl, and finishing with a relationship based on Lagrangian and Eulerian observations of velocities. I am a bit reluctant to take the rest of the section at face value, especially equations 4 and 5, due to this. It might be that you can expand the findings by Middleton (1985) to a moving tracer, which is different from their assumptions of stationarity, but it would have to be carefully proven.

Thank you for suggesting the Chenillat et al. (2015) reference. We have perused the reference and thought carefully about your comments. Our equation (1) makes no assumption about a steady Chl field: we assume that it is fully evolving and advected by the velocity field (and also subject to sources, sinks, and diffusion). Also, Middleton (1985) does not assume the velocity field is steady, it is only assumed statistically stationary. Our equation (1) is the standard material derivative in any text, and it is also the exact equation (1) of Chenillat et al. (2015), though they refer to it as an advection-diffusion equation. When relating frames, the equation (2) in Chennilat et al. (2015) groups the Eulerian (EUL) and advection (ADV) terms from equation (1) into what they call an "evolution equation along a moving fluid parcel" (note the different notation in the derivative of their equation (2), now using the *d* instead of partial ∂), the same as our term "LAG" in our equation (1).

The use of our manuscript's equation (1) is standard, and our attribution of the three terms from left to right as "LAG", "EUL", and "ADV" is also standard. For example, please refer to Jönsson et al. (2011) and their equation (1), which is also the same as our equation (1). In their manuscript, the left-hand side term (same as our LAG) is treated as a time derivative along a trajectory, the fixed-in-space partial time derivative term (same as our EUL) is treated as a time derivative at a fixed location, and the advection term (same as our ADV) is taken as the difference of the two. This is entirely consistent with our approach, the only difference being we apply a statistical scaling of the terms (our equation (5)) in our study instead of quantifying the terms with numerical data as those authors did. Both studies consider a fully evolving tracer field. Therefore, we firmly believe that the mathematical formulation we employ is sound.

Regarding your comments about lines 85-97, here we explain why we first scale equation (1) with velocity scales, and then with tracer scales. The analyses of Philip (1967) and Middleton (1985) assume a velocity field that is statistically stationary and show that the ratio of velocity time scales T_l/T_e is a function of the ratio of velocity fluctuations u' to an evolution speed of the velocity field, $c^* = L_e/T_e$. Their relationships are determined from velocity autocorrelation functions and contain no information about tracer concentrations. Therefore, the dispersion (with coefficient *K*) implied from the velocity scales – which is related to T_l by $K = T_l\left(u'\right)^2$ – is a particle dispersion, representing effects of chaotic advection on the movement of water parcels. Tracers like Chl are not particles and are subject to a transport that also includes diffusion (and sources) in addition to advection. By scaling equation (1) with Chl scales (as done in equations (4)-(5)), our intention is to see how velocity fluctuations u' relative to translation of the Chl field (at speed $c^*_{\mathrm{Chl}} = L_{e,\mathrm{Chl}}/T_{e,\mathrm{Chl}}$) influence the values of $T_{l,\mathrm{Chl}}$ and $T_{e,\mathrm{Chl}}$. This approach uses scales that have the effects of transport and non-conservative terms built in. Our goal is to gain insight into how $T_{l,\mathrm{Chl}}$ varies, and what processes control it. We do not know ahead of time if there will be such a relationship as our equation (4), but given the earlier studies that conclude mesoscale Chl anomalies can largely be explained by stirring (Denman and Abbott, 1988; Glover et al., 2018), we suspect it is worth evaluating. We have reworded parts of Sect. 2 to make our logic clearer, following the discussion above.

Finally, we hope that a clarification of our physical interpretation of the primary results will alleviate your concerns about our framework and convince you that the Chl field is fully evolving. We suspect that you might be objecting to our claim that advection can be important for Lagrangian decorrelation. Indeed, the flow field is advecting the Chl so it might be surprising for the Chl concentration along a trajectory to be influenced by advection as opposed to non-conservative terms from the right-hand side of equation (1). Since LAG = EUL + ADV = S + DIFF, even though the “real” drivers of Chl decorrelation along a trajectory must be sources / sinks (S) or effects of turbulent diffusion (DIFF), mathematically, those terms project onto the EUL and ADV terms.

To help clarify this point, we have made changes to the text to explain the origin of Lagrangian decorrelation, suggesting mesoscale stirring is a major driver. The biggest change is an update to Sect. 4.5. In earlier versions of the manuscript, this section simply presented the empirical relation (our Eqns. (4) and (12), displayed in Figure 6) to interpret our results but now we use it as an opportunity to tie all results together. We first update the section with a qualitative interpretation of the functional form and its parameters (as you asked for in a later comment) and we better motivate the idea that mesoscale stirring generates Chl anomalies and their Lagrangian scales. This is done in part by appealing to the mixing length arguments of Glover et al. (2018), who construct an additional scale (their L_{tracer}) equal to the distance a mesoscale eddy could stir a water parcel containing Chl anomalies (their equation (2)) assuming that all Chl anomalies are generated by stirring a mean gradient. They show that in our study region of the North Atlantic, L_{tracer} and $L_{e,\mathrm{Chl}}$ are statistically equivalent (see their Figure 7). This relation implies that the statistical decorrelation length of Chl, $L_{e,\mathrm{Chl}}$, is likely set by mesoscale stirring of the Chl field, consistent with our finding that $L_{e,\mathrm{Chl}} \approx L_e$, which is the velocity decorrelation scale and the diameter of typical mesoscale eddies. The "geometry" of mesoscale stirring relates the frames (Euclidean statistical separation $L_{e,\mathrm{Chl}}$ and trajectory distance $L_{l,\mathrm{Chl}}$) in the limit of large turbulent velocity u' by setting q_{Chl} .We suggest it is useful to think of ADV as a local stirring of the mean Chl field as opposed to advection of anomalies over long distances (though we show below in this response document that the two views are equivalent). That is why it matters in its relation to the translation of the Chl field, given by $c^*_{\mathrm{Chl}} = L_{e,\mathrm{Chl}} / T_{e,\mathrm{Chl}}$. In addition, it is best to view u' as turbulent velocity fluctuations. When the observer is a true surface Lagrangian observer (and the velocity field is unfiltered with all velocity scales resolved), u' is properly captured by the platform's movements, but for an observer like an Argo float, effects of stirring are underestimated. That means the ADV term is underestimated as we had said originally in the manuscript.

This leads us to address what processes cause the decorrelation of Chl along a trajectory. We make an important update to equation (1) to include a term DIFF, which encompasses effects of turbulent diffusion due to unresolved advection, which nominally is due to the fact that water parcel trajectories differ from infinitesimally small tracer particle trajectories but is even more important by our focus on mesoscale variance since a range of scales of advection are not resolved. Then, we continue our discussion in Sect. 4.5 where we introduce a scaling DIFF (new equations 13-14) and consider the ratio of the LAG and DIFF terms (β = LAG/DIFF), quantifying how much of the Lagrangian tendency is caused by turbulent diffusion (or unresolved advection; equation 15). We can show that when turbulent velocity fluctuations are relatively important ($u' > c^*_{\mathrm{Chl}}$), LAG is largely explained by DIFF, a finding consistent with our interpretation of stirring playing a leading role in setting Lagrangian statistics. Likewise, through an inequality on β we are able to infer that sources (S in equation 1) must be increasingly important when turbulent velocity fluctuations are relatively small ($u' < c^*_{\mathrm{Chl}}$). Noting this, in Sect. 4.4, we de-emphasize an attribution of biological sources and sinks S in driving LAG, noting that in general DIFF could also be important. Finally, throughout we relax language that advection "causes" Lagrangian decorrelation.

As an aside, though we do not include this in the manuscript to avoid the complications of introducing an additional length scale, if we take the Glover et al. (2018) definition $L_{\mathrm{tracer}} = \langle \mathrm{Chl} \rangle_{\mathrm{space}} / \nabla \overline{\mathrm{Chl}}$ (their equation 2) and take their finding that $L_{e,\mathrm{Chl}} \propto L_{\mathrm{tracer}}$ over our study domain (their Figure 7), then our scaling $\mathrm{ADV} = u' \langle \mathrm{Chl} \rangle_{\mathrm{space}} / L_{e,\mathrm{Chl}}$ (our equation 5) becomes $\mathrm{ADV} = u' \nabla \overline{\mathrm{Chl}}$. This supports our interpretation of ADV as local stirring of a mean gradient.

Finally, I still think that the organization and tone of the MS miss the intended audience. For example, I would have liked a more verbose discussion about the formalism for relating Eulerian and Lagrangian timescales described in Middleton (1985) and why it can be used for contrasting them. I'm also missing a more descriptive explanation of the different metrics that being used. What does for example u'/c_{Chl}^*, α_{Chl}, or q_{Chl} tell us? It can be found by reading the text and references carefully, but a reader might give up before figuring it out. Just a table listing all parameters and a short description for each of them would be very helpful. The description of ACF is very thorough but it's not easy to figure out what is specific with your approach without going through the section in detail. Finally, it would be good to add references to the equations that aren't original to this MS.

There is now additional detail in Sect. (2) about the interpretation of Middleton (1985) as a “particle dispersion”, motivating our use of tracer time and length scales, but just as the methods section is already technical, we don’t think a discussion of the formalisms leading to equation (3) would benefit our audience. The parameters u', c^*_{Chl}, and α_{Chl} are defined in Sect. 2, and q_{Chl} is defined in Sect. 4.5. All of those parameters are given a qualitative interpretation in Sect. 4.5, where we clarify in detail the physical processes leading to the $T_{l,\mathrm{Chl}}/T_{e,\mathrm{Chl}}$.

Finally, we have checked equations for attribution. Equation (1) is standard, but given its central role we have added citations to Chennilat et al. (2015), Jönsson et al. (2011), d’Ovidio et al. (2013), and van Sebille et al. (2018). Equation (3) is attributed to Middleton (1985). Equation (6a) is attributed to Morel et al. (2007). We add references to Glover et al. (2011, 2018) for equations (C1)-(C2). All other equations either follow from the above or are standard definitions.

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. As mentioned by the authors, there is a lack of studies [at all scales of variability] comparing estimates of Lagrangian and Eulerian phytoplankton statistics, including temporal and spatial correlation scales at all scales. In this sense, this study represents a significant contribution towards best understanding phytoplankton/chlorophyll measured in both Eulerian and Lagrangian platforms. Throughout the revisions the authors have made an effort to improve the readability of a technically loaded manuscript. I recommend this manuscript for publication after the following details are considered:

Thank you for your careful evaluation of our revised manuscript.

1. Lines 77-81. I see these lines are responding to reviewer 1’s comments, but this sentence is very long, and the key points may be missed. Please consider rewording and breaking it up into smaller sentences.

The sentence has been split and rewritten for clarity.

2. Line 220. Typo: “is used”.

We adjusted this sentence by removing “used to calculate integral scales”, since that point is obvious. That simplification seems to be the best way to fix the sentence.

3. Lines 588 – 590, regarding temporal segment lengths for ACF analysis. Please consider rewording. While I agree that given the range of temporal scales of phytoplankton (1 – 15 days), the segment length of Eulerian time series is probably not an issue, the initial date may have an impact in the result. If I understand correctly, this initial date is variable in the in situ data, but

the same for every year in the satellite data. I’m not certain either what the effect would be in the spatial scale estimate. Also consider that methods to remove seasonal variability are not perfect, and some signal may still remain. I respect your methodology, but I don’t think you are showing evidence to definitely say that the different length segments for L and E estimates are “not an issue” and “generally unimportant”. I would be more cautious in this statement. In my own experience, length of the time series did matter in the comparison of other Eulerian and Lagrangian chlorophyll statistics. The storage format of the is not a strong reason for this methodological choice, so I suggest removing that last part of the sentence.

Thank you for your concern and for sharing your experience. We have rewritten the sentence in line with your suggestions.

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