Biogeosciences
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Discussions

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Interactive comment on "Autonomous, high-resolution observations of particle flux in the oligotrophic ocean" *by* M. L. Estapa et al.

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Received and published: 28 June 2013

Reviewer 1:

1. Fig. 9. The caption says that the magenta bars show cycle-mean flux proxy and propagated fitting error. It is not clear what shows the fitting error. Do you mean the propagated fitting error is included in the bars? It also says that the LEFT hand axis denotes depth of contoured data. I think you mean the RIGHT hand side.

The caption erroneously retains text from an earlier version of the figure in which error bars were included, but whose complexity made it difficult to read. The old text has been removed and the axis labels corrected.

2. Table 1 and the text clearly show where the five floats were deployed. However,

the text frequently refers to floats only by number, assuming the reader will remember which float was deployed where. I think it would help the reader significantly to add a letter(s) to the numbers as a reminder of where each float was deployed when discussing the floats. E.g. 1H and 2H for near Hawaii, 3SS and 4SS for Sargasso Sea and 5B for BATS.

Changed as suggested.

Reviewer 2:

1. (*In summary section*) It would be interesting to know whether this instrumentation can be applied to mesotrophic/eutrophic ocean areas as well. What are the limitations there?

Possible limitations include greater variability in particle size and composition due to lithogenic inputs (which might alter the relationship between beam attenuation increase and mass flux), greater turbulence, and in some coastal locations and time periods, limitations of the profiling float platform itself (e.g. shallow water, variable stratification). We have added text in 3.6, paragraph 2 discussing the need for better constrained POC:σ_c values for lithogenic particles if this method is to be employed in coastal or shelf areas.

2. The authors are careful with their derived 'discontinuous flux component' (page 1243, starting line 5) and argue against episodic flux events as the sole driver. However, I do not believe that 'swimmers' were responsible for these 'flux' observations. From their previous argumentations, I assumed that 'spikes' in the records were attributed to zooplankton 'swimmers' ('transient passes of active swimmers', page 1239, starting line 5) and not to the 'jumps' (which are transformed into the 'discontinuous flux'). This makes sense. More plausible to me is the movement of material on the upward- looking window to explain the 'discontinuous flux component'; this process might also explain the negative 'jumps' (dilution of material).

*We agree that some part of the discontinuous flux component could certainly be due to movement of material on the window, as discussed in section 3.2, paragraph 2. However, positive jumps were more frequent than negative jumps, which is inconsistent with **all** jumps being caused by movement of material. More likely, there is no "silver bullet" and several processes likely contribute. We also agree that the bodies of swimmers themselves are more likely to cause transient spikes than persistent upward jumps; however, swimmers could still contribute to the "discontinuous flux component" by defecating or shifting material on the window.*

Do the authors observe a correlation between the number of 'spikes' and/or 'jumps' to water depths which could probably point to zooplankton activities?

With the float at BATS, spikes occurred only at 150m, while jumps occurred at all depths. Only some profiles had spikes and jumps. The number of affected profiles was too small to make a statistically significant conclusion about the depth-dependence of spikes and jumps, however.

High resolution in situ photographing of particles might help to solve this problem.

We agree (as is stated in the text, Section 3.2, second paragraph).

On the other hand, if the 'discontinuous flux component' is attributable to episodic flux events, I would expect relationships of the signals in the various depth ranges (e.g. 150, 300, 500, 1000m) at one site, assuming rapid settling of the sinking particles.

We also agree with this statement, but in our multi-depth deployment we only had one float, which changed depths every 24 hours. This meant we could not decouple time- and depth-variability of flux, particularly for rapidly-settling particles. We added text to the middle of section 3.3 to address this. With multiple floats sampling different depths at a single site, we could begin to test this expectation.

3. Do the authors have any idea about the underlying causes of the high variability in the order of one day in all records?. One could speculate about day-and-night cycles

due to the dial migration of zooplankton. Is there any evidence for this, e.g. from drifting trap deployments from one of the study regions?

We agree that day-night cycles could be responsible for some of the observed sub-daily variability. Unfortunately in all of the oligotrophic study regions, sediment trap samples integrate over periods of several days by necessity (to get enough material for analysis). Digital sampling methods (e.g., cameras, transmissometers as used in this study) are the only way at present to examine sub-daily flux variability in oligotrophic regions.

4. The decoupling of particle fluxes at different depths (300m vs 150m) is an interesting feature? Are there indications from other studies for intense horizontal advection or suspended particle layers in this depth range?

The modeling study by Resplandy et al., 2012 predicts intense horizontal variability in particle flux. As mentioned in the response to comment 2, the observed decoupling of 150m and 300m observations could be due to the 24-hour time (& space) offset between depths.

5. For better reading, I suggest to rename the floats #1, 2, 3, 4, 5 to, for instance, H1, H2, O3, O4 and B5 (defining the regions of study) in all figures, tables and in the text.

We have renamed the floats according to the suggestion of Reviewer 1.

6. Fig. 9. Axis description 'date' is overlying the numbers (dates) in the lower panel.

This has been fixed.

Reviewer 3:

1. 1.- My main question is the influence of turbulence on the particles sitting on the lens. P 1243 l. 11 "The same low-energy turbulence that allows collection of particles by the transmissometer could also occasionally resuspend them." In addition to the occasional resuspension of particles and without a catching mechanism, wouldn't it be

possible that small particles are washed off the lens continuously (unless they stick to the particles already deposited)? P 1248 l. 28 "Although the greatest need for high time-resolution particle flux data is conveniently in the upper thermocline, where ambient turbulent velocities are likely to be higher, we need to better-establish the degree to which low turbulence limits collection of quickly-settling particles, which may constitute a significant flux fraction." I'm not that sure that higher turbulence is convenient. I would say it will influence on the trade-off between catching larger, fast-sinking particles and loosing small-slow-sinking particles washed off the lens.

This is certainly a concern that should be resolved, most directly (we think) with in situ photography of particles settled on the window. We have added a sentence to the second paragraph of section 3.6 which brings up the possibility of loss of the smallest particles, and alluded to the work of Bishop's group at UC Berkeley, where applicable photographic techniques are being developed.

2. 2.- A brief description or sketch of the transmissometer attached to the deployment line would help to explain the geometrical details that become important in the discussion about the effects of turbulence and catching efficiency. I have noticed the authors refer to Bishop 2004 and Bishop and Wood 2008 but, still, I think a brief description or sketch is needed here.

A schematic figure (now Figure 10 in the revision) of the transmissometer and float cross sections has been added to accompany this discussion. In making this figure, we double-checked drawings of the instruments and realized we made two errors in the original computation of horizontal shading of the transmissometer window: 1) the float body blocks 21%, not 18%, of the transmissometer circumference; and 2) all four transmissometer support struts shade the window (not just the outer two). In total, 47% of the transmissometer circumference is shaded rather than 32%. This does not affect the conclusions of the paper (in fact it brings float-based flux estimates slightly closer to the OFP-based estimates, within a factor of 2 after depth scaling).

3. 3.- Some data segments were discarded because they didn't satisfy the goodness-of- fit criteria. In Fig. 3 there is a short period (the 4 points after time=0) where the data collected didn't satisfy the criteria.

In Figure 3 it was points 3-6 that did not satisfy the criteria, not 1-4 (points 1-2 were discarded from every cycle to allow the float to stabilize at its target depth). We have clarified this in Figure 3.

How is the flux estimated in periods where there are non-significant segments? Are they included as zeros or discarded?

Where there are non-significant segments, no flux estimate is made.

I imagine that very low sedimentation rates wouldn't satisfy the goodness of fit criteria. If this interpretation is correct, flux rates might be overestimated if non-significant segments are not considered.

This is a valid concern. While the RMSE threshold criterion only eliminates segments where the fit of the data to the model is poor, the R^2 threshold of 0.5 could have the effect of eliminating segments with slopes close to zero. We checked whether our criteria led to flux overestimates by determining the frequency with which segments failed the R^2 test, passed the RMSE test, and had slopes between 0 and 0.0026 $m^{-1}d^{-1}$ (this upper limit chosen after examination of the distribution shown in Fig. 5). For the four long-term deployed floats, between 7-12% of hourly observations fell into segments at least 3 hours long that failed the R^2 test but passed the RMSE test. Of these segments, only rarely (0-2 times over floats' entire deployments) were slopes in the 0-0.0026 $m^{-1}d^{-1}$ range. Therefore, the conclusions based on data in this manuscript are probably unaffected. In a future iteration of the fitting process, however, a different criterion for assuring segment linearity may be desirable. We added text to the end of Section 2.4, paragraph 2 discussing this point.

4. 4.- Although it is clearly written, sometimes I was not sure I had understood the rea-

soning presented and I was missing an explicit statement on the mechanism. p. 1242 l. 20 "If the discontinuous flux component were caused by large, episodic flux events, we would expect the distribution shapes and CVs of time-averaged flux observations to match those from sediment traps." Please, state why.

Sediment traps integrate flux (including any episodic events) over periods of several days to weeks. If an "optical sediment trap" also senses these episodic events and we average its (higher time-resolution) record down to the lower sampling frequency of the traditional sediment trap, the two should show similar flux distributions. On the other hand, if the optical sediment trap is missing the episodic flux events or if its record contains artifacts that are not caused by particle flux, then the flux distribution will be different than that of the traditional sediment trap. We added text to the beginning of the first paragraph of section 3.2 to clarify this distinction.

Page 1245 line 13 "We must first assume that settled particles and any fouling material on the transmissometer window do not interact optically (note that this is not the same as geometric overlap, since particles also diffract light around their edges). Justifying this assumption, we generally observed cp(650) to increase steadily for hours (Fig. 3) and sometimes for entire 1–2 15 d drift phases, rather than the rate slowing with increasing cp(650)." Why does the observation justify the interpretation? Please, explain.

A useful analogy here is the more typical use of a transmissometer to measure suspended particles. At low concentrations, beam attenuation scales linearly with the concentration of suspended particles (according to Beer's Law). At high enough concentrations, however, photons begin to interact with more than one particle ("multiple scattering") between source and detector and the concentration:attenuation relationship flattens. We expected that a similar effect could occur were a thick-enough layer of particles to accumulate on the window. The data seem to suggest that this wasn't a problem during these deployments, however. We added text to the end of the first paragraph of section 3.5 to make this more clear.

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5. 5.- To validate a method, reproducibility or repeatability is usually required. Here, a comparison with "replicate" transmissometers would provide a very strong support to the consistency of these optical particle-flux measurements.

We strongly agree with this statement and believe such a test is the logical next step. Fieldwork will be underway shortly.

6. P 1243 l. 28 I think the authors refer to Fig 8d.

Yes, we have fixed the text

7. Fig. 9 caption: depth is shown on the right axis.

This has been fixed.