Biogeosciences Discuss.,
doi:10.5194/bg-2016-62-RC2, 2016

Biogeosciences Discussions
Open Access EGU

Interactive comment on "Robotic observations of high wintertime carbon export in California coastal waters" *by* J. K. B. Bishop et al.

Anonymous Referee #2

Received and published: 13 April 2016

General comments This manuscript describes the first use of a powerful new observational technology. Pending some revisions detailed below, it will make an important contribution to the literature on methods for observing the ocean's biological carbon pump. These data are the first of their kind, and if the CFE technology continues to be robust in the field, these types of measurements could revolutionize our understanding of the ocean biological pump. Aside from the new method, this paper's scientific findings include high sinking POC flux under apparent low-productivity conditions in an upwelling, coastal system, and undersampling of sinking POC by surface-tethered relative to neutrally-buoyant sediment traps. Most of the specific comments below pertain to instances where strong, rather general statements are not fully supported by the data as presented. In some cases, the data just need to be clarified or more technical detail added to the text. In other cases, more nuance should be added to the interpre-

Printer-friendly version

Discussion paper

tations. A theme through several of the comments below is that more attention should be paid to spatial variability in the measured C fluxes, particularly in comparisons of in situ observations to satellite data. In general, the necessary changes should be straightforward for the authors to address.

Specific comments

p. 2, L. 4. The specificity of the sentence about foraminifera shells, etc. suggests that perhaps a reference is necessary.

p. 2, L. 36. Can the CFE really operate for years? It would be more useful to most readers to point out the demonstrated length of deployment so far (months?) or give the nominal number of profile cycles that can generally be achieved.

Methods section. I suggest you tabulate the cruise numbers, deployment locations/times/depths, and retrieval locations/times/depths to help the reader keep track of the different observations.

Section 2.1. It may be a good idea to move the basic trap funnel and stage dimensions from Appendix A to here. Otherwise, you don't state the collection area anywhere. I noticed that another reviewer has assumed the OSR has a 1 cm diameter trap opening, which is not the case and may have led to some misunderstanding.

P. 3, L. 20. Use "thickness" instead of "length" to describe the vertical dimension of the baffle.

P. 3 L. 28-30. If any of the OSR data are transmitted, they must be pre-processed on board prior to doing so, correct? If this is something that has been implemented, a brief description of the on-board processing steps should be given here. Otherwise, please remove "OSR data" from the list of things that are transmitted during surfacing cycles.

P. 3 L. 29. State the actual dive depth rather than "considerably deeper than planned."

P. 4, L. 12-13: While it is ultimately the author's call, I suggest a change from base-10 to base-e. I believe that this is the convention for most of the optical oceanography community when describing optical properties.

P. 4, L. 20 (and related discussion in Section 2.3): How have you dealt with particles that overlap? Would stepwise subtraction lead to a possible underestimation of flux? Please address.

P. 4, L. 30: Is there a poster or meeting presentation which has shown the elimination of the stress polarization interference, which you can refer to here? If not, this sentence should be reworded to indicate that the problem is surmountable, but the discussion of later CFE builds should be removed as they are not relevant to the data presented here.

Section 2.2.3 and P. 7 L. 7-8, and supplemental videos: Presumably swimmer interference should also affect analysis of the POC flux data, not just the PIC flux. Please revise this discussion so that it applies to both proxies. The supplemental videos clearly show several instances of "disappearing/moving particles" (e.g., halfway through 1204 and maybe again at the end; also about halfway through 1301). Unlike direct sediment traps, you can actually detect and correct for the presence of swimmers with the OSR, and I think this should be discussed in more detail.

P. 5, L. 21: Please add a reference for the statement that birefringence scales linearly with PIC concentration.

P. 6, L. 6-16: Please add an equation or two summarizing the calculation you have described here in words. It will greatly clarify the procedure.

P. 6, L. 18-28: It would be illustrative to also compute fluxes using models in the literature for aggregate carbon content as a function of size. For instance, Alldredge (1998) contains useful relationships for several categories of marine snow. At the very least, you should mention the existence of such models and their relevance to interpretation of image data such as that collected by the OSR.

P. 6, L. 33-34 and Figure 1. Add a distance scale bar to Fig. 1c. You state that all satellite data points used for comparison were within a 2km radius of 33.72°N, 119.5°W, but that appears to be the center of a "150 km2" study region, and does not correspond to the actual surfacing locations of the CFE or optics cast locations. If Chl and POC were patchy, then the changes seen in the surface optical properties at a fixed point in the center of the box are unlikely to correspond with the observations on a quasi-Lagrangian platform drifting tens of kilometers from this point. Comparisons to satellite data should attempt to match up with the actual locations of the CFE tracks. Once this is done, please adjust the text accordingly.

Section 3.2: How close in time and space were the different depths measured on each deployment? These details should be made clear in the text or in Figure 1. That is, it is possible the CFEs were sampling different sinking particle pools at each of the different depths? It may not be correct to assume a single attenuation model.

p. 10 L. 11-14. You invoke certain physical conditions here in order to support the hypothesis that low-biomass conditions were caused by consumer-driven export and not by physical aggregation or advection. However, the minimal wind and current shears you describe are inconsistent with your other major finding that your surface-tethered BUOY-OSR undersampled by a factor of 20 relative to the CFE due to strong hydrodynamic effects felt by the different platforms. Please reconcile these ideas.

p. 10, L 16-17 and Figure 6: The trend is hard to pick out from Figure 6. You need to add an inset that shows the January 2013 period. Otherwise it looks like the 1-week-prior points are scattered, not necessarily decreasing, and in any case they cover up the running-mean line so it cannot be seen.

P. 10 L. 18-19. "Satellite imagery from Jan 2013 shows a patchy POC/chlorophyll distribution without obvious eddy structures or fronts near by." This statement is not possible to evaluate from the satellite chlorophyll images included in the Supplementary Information. It certainly appears as though there are potential eddy structures and fronts in

Printer-friendly version

Discussion paper

these images. However, the images do not have latitude and longitude marked, there is no color bar (is it log or linear? What are the scale limits?) nor are the CFE deployment locations marked. These images need to be clearly annotated so it is obvious that they support the claim that there were no nearby eddy structures or fronts. Otherwise it is not possible to differentiate a rapid temporal change in POC from a rapid spatial change, and this assertion should be removed.

P. 10, L. 22-23: What are the uncertainties on your derived “Martin” b values? Are they even significant? (If not, add a statement to this effect – it adds strength to your finding that the Martin curve is an inappropriate model for these data). However, you should also mention again the time and distance separations among the different depth measurements – if export was patchy, then it could be that each sampled depth is too far from the others to infer a continuous attenuation profile.

Section 4.1: The difference between surface tethered and neutrally buoyant traps may be more pronounced in the presence of large aggregates such as the ones you have observed here. The studies you cite comparing PITS and NBST traps were conducted in an oligotrophic region where in situ camera profiles showed low concentrations of particles larger than 1500 μm (McDonnell and Buesseler, 2012). Your findings in coastal California are quite striking, but there may be site-specific differences in the relative efficiencies of tethered/neutrally-buoyant traps at collecting aggregates. Please revise lines 34-36 to address the differences between the different types of environments.

P. 11, L. 5-6: Near-horizontal approach of particles to tethered traps has been described in detail by Siegel et al. 2008; I suggest you include a citation to this reference.

P. 11 L. 21: Replace “the single profile 234Th/238U method” with “the 234Th/238U method with a steady-state assumption”, which is clearer.

P. 11 L. 23-24: Similarly, replace “time series sampling” (which is less specific) with something like, “multiple reoccupations of a water parcel assuming non steady-state conditions”.

P. 11 L. 22: Replace “is not applicable” with “may not be applicable”. If it can be established that a coastal system is in steady state and advection is minimal, then the steady-state assumption can be used.

Figure 9: Please put a thin margin between these panels.

Movies in Supplement. Do these represent multiple depths and profile cycles? Can you make this information clearer in the “readme” file? If there are multiple cycles represented in the videos, please insert “marker” frames so it’s clear where the breaks are. Also, the movies occasionally show “disappearing” particles. Are these zooplankton? How are they treated in the flux estimation calculation? (see also the comment on section 2.2.3)

Technical comments

p. 1, L. 14, change “monitor” to “monitored” p. 1, L. 19, Break into two sentences. Start 2nd sentence with “Multiple lines of evidence indicate. . .” p. 1., L. 19. Remove space from “under sampling” p. 1, L. 20. Change “compared to” to “than the” p. 2, L. 3. “coccoliths” should be singular p. 2 L. 13, change “near by” to “nearby” P. 2 L. 13, should be “strong, recent weakening” P. 2 L. 18. It is unnecessary to abbreviate Eppley and Peterson 1979 because you only cite it once more. p.2 L.34. Insert “that” before “we have developed”. p.3 L. 4. “gain detail of the” is awkwardly-worded. p. 8 L. 5. “artifact” is misspelled. P. 10 L. 17: Should refer to Figure 6, not Figure 4.

References:

Alldredge, A. 1998. The carbon, nitrogen and mass content of marine snow as a function of aggregate size. Deep Sea Research Part I: Oceanographic Research Papers 45: 529–541.

McDonnell, A. M. P., and K. O. Buesseler. 2012. A new method for the estimation of sinking particle fluxes from measurements of the particle size distribution, average sinking velocity, and carbon content. Limnol. Oceanogr. Methods 10: 329–346.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-62, 2016.