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Biogeosciences Discussions

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

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—– *and italicized text indicates author response to the review.*

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 interpretations.

¶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.

—– *The conditions encountered are well documented with both remote sensing and CTD data.*

Specific comments

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

Be et al. Armstrong et al.

¶(2) 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.

—– *The CFEs have had field deployments lasting as long as 40 days with no ill effects. We have demonstrated in the lab that they can operate at hourly frequency for 8 months. By extension, 16 months at 2 hour frequency... etc. The CFE is independently powered and thus has no impact on the profiling lifetime of the float.*

Bishop, J.K.B. (2012) Autonomous Exploration of Sedimentation Dynamics in the California Current System. Proc. 15th Biennial Challenger Conference for Marine Sciences. 3-6 Sept

2012. University of East Anglia, Norwich, UK. p36.

¶(3) 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.

—– will add this to supplemental materials).

¶(4) 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.

—– will clarify the text although the Appendix is formally part of the paper.

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

—– we will look at this. Given that the OSR is shown in Appendix 1, the aspect ratio of the baffle will be obvious

¶(6) 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.

—– We say that images are recorded on board the CFE. We will implement processing onboard the CFE at the level described here. The OSR data transmitted include engineering status and image metadata. We will clarify the text.

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

—- Will state depths were 300-400 m deeper than planned in May 2012.

¶(8) 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.

*—– We have explained our rationale for this. 2.303*log10 = ln.*

¶(9) 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?

—– Overlapping particles are additive in attenuance units as explained in the text. The only complexity is determination of particle size distributions. We provide the series of images as supplemental materials that were used in the estimation of cumulative attenuance and POC conversion factor.

¶(10) 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.

—– We will retain this discussion as it is something we have demonstrated through deployment experience.

¶(11) 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.

—– We plan to implement processing codes that detect movement. This is outside of the scope

of the current paper. The interference by swimmers was minor in Attenuance units.

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

—– *We did (Guay and Bishop, 2002). The two points about birefringence are related.*

¶(13) 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.

—– *The text is clear. We do not think an equation will simplify the text.*

¶(14) 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.

—– *Thank you for pointing out this reference. We will add discussion. Alldredge (1998) collected marine snow by scuba at depths of 10 to 20 m. If we use our 2d analyses of min and max dimension to compute ESV following Alldredge (1998) we get volumes that are 8.5 times higher. If we used their conversion of ESV to POC/aggregate and sum for particles > 1 mm in size then, the POC estimate falls 17 times lower than our estimate. Their estimate yields a POC density for marine snow equal to 0.00020 g/cm3; in our case numbers are 0.05 g/cm3. The carbon density of aggregates derived from their equations differs by a factor of 250.*

I'm not sure whether or not there is a units error in Alldredge (1998) or if the identified marine snow particles sampled in the euphotic zone are mostly empty of material. There are no images of collected marine snow in the 1988 paper. Ours are clearly loaded. Bishop et al. (1978) worked with aggregates sampled from 100-400 m. We feel that these data are more reasonable. Obviously, there is a need for further work on calibration. We will add some discussion of Alldredge (1988) to the paper.

¶(15) P. 6, L. 33-34 and Figure 1. Add a distance scale bar to Fig. 1c.

—– *yes*

You state that all satellite data points used for comparison were within a 2km radius of 33.72N, 119.5W, 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.

—– *150 km2 is the rectangular area of the study region. The actual area is smaller. This is an effort that will not change the interpretation of the data. With 3 surfacings of the CFE per day and only one satellite image per day at best, a drifting matchup is not as simple as it seems. The single point reference is adequate.*

We had provided imagery at 4 km (Modis aqua) as a supplemental document. We worked with 1 km results and report means and standard deviations. We have attached similar images from Mati Kahru (Scripps) with area of the Reference circle plotted. They are also shown at 4 km resolution. The big picture story of the imagery is that prior to the expedition, there were higher levels of Chlorophyll at the surface in the Santa Cruz Basin in general. From day to day they fluctuate. By the time the ship arrived, the Chlorophyll had decreased. Interestingly, a bloom developed in the Santa Barbara Basin by the time we had left. Animation of the imagery shows that there is little coherent structure. This is the best that we can do to document conditions.

Further analysis of patchiness is beyond the scope of this paper and our funding.

¶(16) 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.

—– *There will always be a temporal / spatial effect seen in particle profiles. We have analyzed hydrographic data for evidence of intrusions and found none. The water column is influenced by tidal currents and there is a net westward drift.*

¶(17) 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.

—– *We described conditions minimal wind and swell at the surface. In January 2013, there is no evidence for frontal features in the area of our observations. The whole water column is influenced by semi-diurnal tidal currents varying from 0-20 cm/sec. It is also influenced by internal waves.*

The difference in shear is that experienced by the BUOY-OSR vs, the Lagrangian CFE at depth. We have quantified the horizontal motions leading to the observed bias.

¶(18) 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.

—– *will review this and improve the figure.*

¶(19) 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 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.

—– *see our response to point (15) above.*

¶(20) 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.

—– *The data are presented as clearly as can be. The depth occupations of the CFE occur approximately 7 hours apart as described in the methods. The time series is not long enough to average out day/night effects; thus there may be a temporal effect. In early versions of the manuscript, Martin curves were not drawn. Reviewers requested this discussion. Reviewer 3 requests contrast with the typical curve. We will do this. We will provide a table with times and positions of CFE transmissions and BUOY-OSR positions. The figures graphically compare time series. We feel that a lot of additional discussion is unwarranted.*

¶(21) 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 um (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.

—– *I looked at the text and don't understand what the reviewer wants us to say beyond the fact that the factor of two differences are found in oligotrophic waters. The text seems clear but I will look at it again and modify if it can be improved. That said, Stanley et al. found a factor of four difference in PIC/POC ratio. So surface tethered traps are collecting a different quality of particle than NBSTs even in oligotrophic waters.*

¶(22) 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.

—– *we see no reason to add the reference for particle trajectories. This has been a point of discussion since Gardner's 1977 PhD thesis.*

¶(23) P. 11 L. 21: Replace "the single profile 234Th/238U method" with "the 234Th/238U method with a steady-state assumption", which is clearer.

—– *The text seems clear as written. Will review.*

¶(24) 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".

—– *the text seems clear as written. Will review.*

¶(25) 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.

—– *the text seems clear as written. Will review. The conditions satisfying steady state the Reviewer indicates are unlikely to occur in any coastal environment.*

¶(26) Figure 9: Please put a thin margin between these panels.

—– *will do.*

¶(27) Movies in Supplement. Do these represent multiple depths and profile cycles?

—– *yes. Imaging logic described in the methods.*

¶(28) Can you make this information clearer in the "readme" file?

—– *yes. However imaging logic is described in the methods.*

¶(29) If there are multiple cycles represented in the videos, please insert "marker" frames so it's clear where the breaks are.

—– *the video is provided as an example of CFE deployment results. They are there primarily to contrast the kinds of particles encountered by CFE in the three seasons. A second purpose to document the contrast of CFE and BUOY-OSR collections in January. We don't have funding to do any more with movie production at this time.*

¶(30) 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)

—– *Yes the ones that move around and/or disappear are swimmers. We describe interferences in the PIC records due to barnacle larvae. The effects are relatively small in the attenuance data. We mention cases where corrections have been made. We are developing codes to detect movement and remove the contributions of these relatively rare swimmers.*

Technical comments ¶(31) p. 1, L. 14, change "monitor" to "monitored"

—– *yes*

¶(32) p. 1, L. 19, Break into two sentences. Start 2nd sentence with "Multiple lines of evidence indicate :::"

—– *yes.*

¶(33) p. 1., L. 19. Remove space from "under sampling"

—– *yes.*

¶(34) p. 1, L. 20. Change "compared to" to "than the"

—– *yes.*

¶(35 36) p. 2, L. 3. "coccoliths" should be singular p. 2 L. 13, change "near by" to "nearby"

—– *yes yes.*

¶(37) P. 2 L. 13, should be "strong, recent weakening"

—– *yes.*

¶(38) P. 2 L. 18. It is unnecessary to abbreviate Eppley and Peterson 1979 because you only cite it once more.

—– *the abbreviation works as written.*

¶(39) p.2 L.34. Insert "that" before "we have developed".

—– *yes.*

¶(40) p.3 L. 4. "gain detail of the" is awkwardly-worded.

—– *will clarify.*

¶(41) p. 8 L. 5. "artifact" is misspelled.

—– *will correct.*

¶(40) P. 10 L. 17: Should refer to Figure 6, not Figure 4.

—– *will correct.*

¶References:

Alldredge, A. 1998. The carbon, nitrogen and mass content of marine snow as a func-

tion 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

——— end of response

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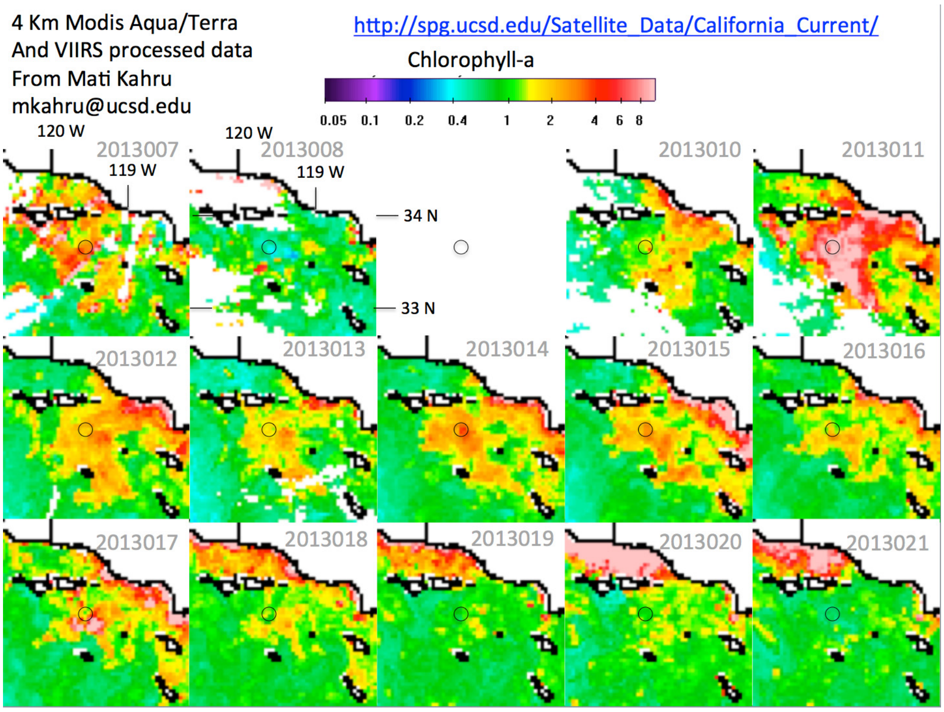

Fig. 1. Imagery from Mati Kahru (Scripps) January 2013