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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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Below we provide a response to Reviewer 1 in Italics.

¶Following the abstract and introduction, the methods were described in great detail. Sometimes I feel that this is more a methodological manuscript. This can be seen in the high number of figures describing the methods. Even in the result and discussion sections, methodological aspects can be found (e.g. results: chapter 3.3., third paragraph, chapter 3.4...). I wonder whether and how the methods described here are different from the ones presented in earlier papers, e.g. in Bishop et al., 2004 ('Robotic observations ...). This should be clarified and then, the method sections could be shortened and the focus concentrated on the gathered data. If the methods differ, the changes/improvements could be briefly outlined. Some figures could also be moved to

the Appendix.

—– Sorry for the confusion. The papers, Bishop et al. 2004, 2009... "Robotic Observations..." describe our first robot, the Carbon Explorer. The CE was comprised of a SOLO float with interfaced Transmissometer and scattering sensors. The primary mission of the CE was to measure particle concentration profiles (beam attenuation coefficient and turbidity), we also used the transmissometer to register particle accumulation on the upward looking optic as the float drifted at depth, prior to profiling we cleaned the upward looking optic and measured transmission again, thus enabling us to derive the Carbon Flux Index by difference. This is well documented in the referenced publications.

The current paper describes an entirely new robot, the Carbon Flux Explorer, it was necessary to describe in detail our methodology for the Optical Sedimentation Recorder (OSR) as it was deployed both on the CFE and as a heavier than water package tethered below surface floatation. This is the BUOY-OSR system.

The operational details of both CFE and BUOY-OSR systems had to be documented in detail as this is the first paper and because the findings from the two systems were so different. Also described are key environmental data (ADCP results in particular) used for identifying the cause of the differences between the CFE and BUOY-OSR observations. We think that it is therefore appropriate to retain methodology as is and to not relocate figures to the appendix.

¶My major concern, however, is the comparison between optically- and trap-derived (surface tethered and moored traps) fluxes. Both approaches are fundamentally different and comparisons are limited by seasonal, interannual and regional variability.

(1) It would be helpful to have more information on the locations of the different types of sediment traps used for comparison (e.g. in Fig. 1) and the exact seasons/years and the water depths. In chapter 4.1. the CFE fluxes from the Santa Cruz Basin were compared to fluxes from the nearby Santa Barbara and San Pedro Basins which were derived from different water depths and different sediment trap types.

(2) Flux data from Martin et al. (1987) were captured from more open ocean sites in the far north of the Pacific in different years rather than from a more local basin. To facilitate reading and to follow the presented argumentation and evaluate this comparison more critically, the authors should show a table with optical and sediment trap fluxes but with detailed information on locations, seasons, years, water depths, surface or bottom-tethered arrays with traps, type of traps and openings.

—– (1) and (2)We will add to figure 1 – locations of the two studies compared (see attached graphic which shows a larger view of the southern California bight) and modify the text to describe the periods of observations and methods used in more detail. The methodology for the comparative studies is well documented in Thunell 1998 (Parflux Trap 50 mab at 540 m; August 1993- August 1996 at 2 week sample resolution), and Collins et al. 2011 (McLane Mark V sediment traps Jan. 2004 through Dec. 2007, weekly resolution; 550m and 800 m. Shallow traps (PITs - 24 hour deployments, various months 2005-2008). We believe that adding a detailed tabulation of already published data is not needed.

¶(3) Further, the authors cannot explain the large differences (10-20 times) of fluxes in a satisfactory way. It is hard to believe that the CFE's optical sedimentation recorded (OSR) with 1 cm opening was able to better sample large marine snow aggregates of several mm size (which are rather rare in the water column) than a cylindrical tube of about a decimeter or larger in size of a free-drifting (surface tethered) sediment trap. I would expect this to be the other way round. It is hard to believe that the baffles of sedimentation tubes are so small to destroy fragile marine snow aggregates of a few mm in size (page 11, uppermost chapter). By the way, the opening of the tube of the OSR (1 cm) is not larger than the cm-sized trap baffle openings (page 11 lines 6-9).

—– The reviewer stated "Obviously, the traps [BUOY-OSR] under sample larger marine snow aggregates of the size class larger than 1mm". The OSR on both CFE and Buoy-OSR systems is identical. There may have been some confusion.

The analysis of particle size distributions in the images and the analysis of ADCP results indicate that the large aggregates are likely not destroyed by the BUOY-OSR baffles, but simply bounce off the baffles back into the flow; during the bounce, some fragments of the aggregates get into the trap. We think the text is quite clear on this.

We agree that PIT traps have almost the same baffle opening as used with the OSR. PITs have been deployed similarly to our BUOY-OSR. We would expect that similar biases would be found. Stukel's work with PIT traps appears to confirm this. During the rewrite of the article we will look to clarify the text.

¶Sediment traps have their limitations as well, depending for instance on the type of array (surface vs bottom tethered). In particular, bottom-fixed moorings with shallow water sediment traps seem to be critical to record fluxes in the upper few hundred meters of the water column. Shallow traps may not provide an accurate measure of particle fluxes and differ by a factor of 3-10 (Buesseler 1991).

—–The 3-10 range of Buesseler 1991 is based on the 234Th method combined with uncertainties of assumptions regarding the C:Th ratio of particulates.

¶Part of the discrepancies discussed here may be attributed to these uncertainties in trap-derived fluxes (e.g. page 9, chapter 4.1. of discussion).

—– We deployed identical OSRs on a Lagrangian platform and surface-tethered. We feel that the manifestation of hydrodynamically forced undersampling of mm sized aggregates by the BUOY-OSR is very clear.

In addition, we do discuss factors leading to uncertainty of our POC flux estimates in the methods. These are the best estimates we can make. Bishop et al. 1978 paper is the most closely applicable. Please see our response to Reviewer 2 in the separate reply.

¶Particle fluxes from sediment traps (NBST, surface tethered, moored) on the other hand, measure carbon fluxes more directly and apply less assumptions than the optical methods. There are less than a factor of two differences of fluxes between NBST and surface tethered traps as the authors mention at the end of page 10.

—– *The finding of "factor of two differences" of NBST and surface tethered PIT traps has been based on work performed in oligotropic waters near Bermuda. While Stanley et al., show that POC numbers are about a factor of two different, the PIC/POC ratios vary by a factor of four.*

Comparisons have not been done in the coastal environment.

Also the flume studies (summarized in Buesseler et al. 2007) on efficiency of traps experiencing horizontal currents utilized clay and silt sized particle suspensions. While the trap size was scaled down in models used, the baffle openings were cm sized in the models with baffles. Consequently, the scale of the particle size to baffle opening was small. In our case, the aggregate size is the same magnitude as the baffle opening.

The flume studies also pointed out the importance of controlling trap tilt. Hence our effort to achieve near zero tilt on the OSR systems deployed tethered to the surface.

The present study took place in a coastal environment where large aggregates are abundant, as we mention >97 percent of the flux was carried by aggregates > 1.5 mm in size in January. Thus the factor of 20 difference found in Jan. 2013 is inescapable. Size analysis suggests a minimum factor of 3 bias in the other seasons.

Knauer, Martin, and Bruland (1979) say, "Each tube had an inside diameter of 7.39 cm and was equipped with a baffle system (Soutar et al., 1977) that consisted of 16 smaller tubes (length 7.6cm). The top ends of the baffle tubes had been milled to a wall thickness of 0.06 mm to minimize surface area (about 5 percent of the cylinder mouth area which is 43 cm2). We assume that materials hitting these edges fall into the collectors and contribute to the total flux. GARDNER (1977) has shown that open cylinders with a length-to-width ratio of approximately 2 or greater will yield representative fluxes. With our use of a baffle system, an adequate length-to-width ratio (8.4) and density gradients (see below [in their paper]), we assume that our traps sample the vertical flux" of particulate matter with reasonable accuracy. We also have 210Pb data (see below) supporting our assumptions. However, like other investigators attempting to measure vertical fluxes, we presently have no way of definitely knowing whether our supposition is correct."

What Knauer et al. (1979) did not anticipate is the possibility of large aggregates bouncing back into the current flowing horizontally over the trap.

We feel that the discussion is more than adequate on explaining why and how the differences between BUOY-OSR and CFE arise.

¶Summarizing this, I suggest to be more careful with this kind of comparison and the conclusions. The problems associated with this comparison of fluxes (optical vs traps) should be clearly mentioned in the discussion.

—– *We feel that we have been as careful as we can in the discussion. We had hardly anticipated the result. We will look to see if the text can be clarified further*

¶Minor issues

(A) page 1 , line 28:... by grazers or [add] settle down as larger marine snow particles.

—– *will correct*

(B) The optical methods to estimate carbon fluxes which are described here need several assumptions, e.g. conversation factors (chapter 2.3.). What are the errors of the individual methods and the potential cumulative errors? Is there any estimation/ quantification? Something written in earlier papers?

—– *we have done all that can be done regarding a discussion of uncertainties in the derivation of our attenuance – POC conversion factor. Further work on calibration at sea is scheduled for August 2016.*

- page 5, chapter 2.2.4. Do the authors only hypothesize that attenuance is the best proxy for POC? What is the basis for this assumption, please clarify

—– *The logic is explained in the text. Transmissometer beam attenuation coefficient is a well documented and best proxy for POC concentration; it is superior to scattering based methods which are subject more to refractive index effects. By analogy we hypothesize that Volume Attenuance flux (as opposed to darkfield counts) is a proxy for POC flux.*

(C) page 8, chapter 3.3., sometimes hard to read due to many abbreviations.

—– *we will look at this*

(D) Fig. 1, show surroundings of the Santa Cruz Basin to provide more info on the general setting of the study site and the other sites used for comparison

Will do. See attached figure as an example of added detail for fig 1.

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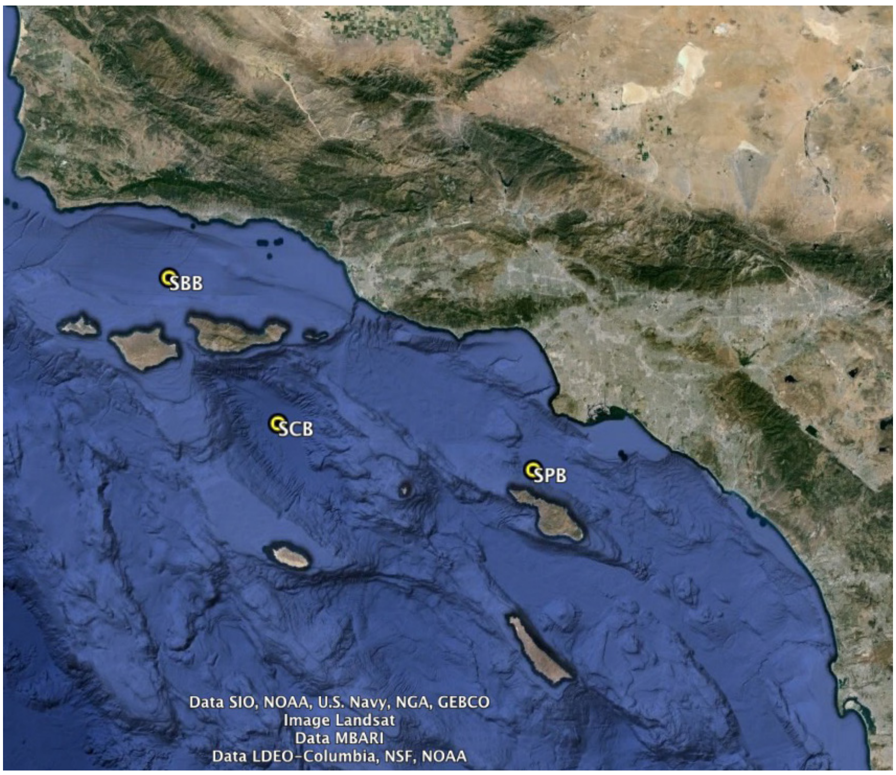

Fig. 1. Added figure element for Fig 1.