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Discussions

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

Anonymous Referee #3

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Bishop and coworkers describe a novel technological development (carbon Flux Explorer, CFE) that promises to revolutionize research on the biogeochemical cycling of carbon within the ocean thermocline, while also offering new insights into the factors that control the spatial and temporal patterns of the flux of particulate organic carbon (POC) exported from the euphotic zone. This publication is timely in that the CFE will be a valuable asset to the emerging EXPORTS program (Siegel et al., 2016. Prediction of the Export and Fate of Global Ocean Net Primary Production: The EXPORTS Science Plan. Frontiers in Marine Science, 3, doi 10.3389/fmars.2016.00022).

Fluxes and transformation of carbon in the ocean thermocline have long been a topic of discussion and debate, and the need for more quantitative characterization of these fluxes, and of the processes that regulate them, has been recommended in a number of community planning docu-

ments (e.g., OCTET http://www.somas.stonybrook.edu/1999/10/18/octet/ and OCCC http://www.us-ocb.org/documents/occc_is_2004.pdf). Motivating factors include both the desire to understand the supply of nutrition that fuels the mesopelagic ecosystem and the need to characterize the environmental factors within the thermocline that control the fraction of carbon exported from the euphotic zone that ultimately reaches abyssal depths before being regenerated, where it may be sequestered from the atmosphere for centuries to millennia. The CFE will enable investigators to characterize and quantify these POC fluxes with unprecedented accuracy and resolution, both spatial and temporal.

The manuscript is well written, and it could be published with only modest revision, as already described by Referees 1 and 2. However, in the spirit of exploring multiple working hypotheses, I would ask the authors to consider an alternative interpretation of their principal scientific finding described in the paper.

Bishop et al. interpret the high flux of POC in January, a time of low surface biomass and low POC concentration, to reflect the rapid loss of POC by grazing and export. By contrast, they interpret the opposite end-member condition of high surface biomass and low export, in May, to reflect the much greater efficiency of biological recycling (consumption and regeneration) of POC in surface waters. This interpretation is plausible, and I don't necessary disagree, but I wonder if the authors can rule out the following alternative interpretation.

Specifically, could the contrasting conditions observed in January and May reflect variable storage of POC in surface waters which, in turn, is regulated by physical aggregation and sedimentation? As noted in the text (p.2, line 2 and p. 10 line 11) one generally thinks that turbulence increases particle coagulation by increasing the rate of particle-particle encounter. While this may be true for aggregation of small particles, turbulence may lead to fragmentation of fragile large aggregates. Indeed, this may explain the absence of large aggregates in the samples collected by the BUOY-OSR. Furthermore, although the weather conditions were characterized as "calm" for all deployments of the CFE, conditions were the most quiescent in January. Therefore, is it possible that ultra-quiescent conditions facilitate the physical aggregation of POC into particles large enough (marine snow) to be exported with much greater efficiency than for the fragmented pieces of marine snow? I have no evidence to suggest that this alternative hypothesis is preferable to the one offered by the authors. Rather, I simply suggest that the authors consider physical aggregation as an alternative hypothesis to account for the unexpected inverse relationship between surface ocean POC inventory and the flux of exported POC collected by the CFE.

Also, the authors speculate that larger size classes of organisms dominated the grazing during January. Can this be verified using collections of historical data available from some of the programs that have been monitoring the region for decades, such as CalCOFI, the California Current System LTER, or the Central and Northern California Ocean Observing System?

DETAILS and EDITORIAL COMMENTS in their order of appearance:

p. 2 line 13 "nearby" as one word

p. 5 line 5 delete "were"

p. 5 line 21 insert "with" between linearly and PIC.

p. 6 line 10 delete "a" after estimate.

p. 6 lines 18-20: Here the authors stress, appropriately, that the conversion to POC flux is based on very little observational evidence. I suggest that the authors add a new section to the Discussion with recommendations for future studies that would reduce the uncertainty in this conversion factor.

p. 6 line 21 delete the comma after "above".

p. 8 lines 9-12: Here the authors describe the unexpected finding that in some cases the PIC/POC ratio decreases with depth. This is unexpected because the paradigm

is that POC is regenerated much more rapidly than PIC. The authors attribute the PIC/POC decrease with depth to temporal variability of the PIC/POC production ratio. Could other (potentially more interesting biogeochemistry) factors be involved?

p. 8 line 23 insert "in January 2013" between collected and by.

p. 8 line 30 "lower" is misspelled.

p. 10 lines 20-26: Plot the Martin curve on Figure 6C to provide readers with a visual illustration of the difference between Martin's export attenuation (b value) and the b values derived in this study.

p. 11 line 9 change "high" to "higher"

Fig 1 caption: insert "place" after "deployments took"

Fig 4 caption: explain the small circles, similar to the explanation offered in the caption of Fig. 5.

Fig 9: placing a white vertical bar between the two images will make it easier for readers to compare the figure with the caption.

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