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

Anonymous Referee #2

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This manuscript describes an autonomous, optically-based method for the estimation of particle flux with a high spatial and temporal resolution. Following a clearly written introduction, the authors provide a very detailed section on the methods, structured into four sub-sections. Results are presented from three important oligotrophic areas where time-series stations with flux data are available (BATS, OFP and HOT). It is a great pleasure to read this well-structured and excellent contribution which is well-suited for a publication in BG. The abstract contains all important points, the title is adequate, the presented figures and tables are carefully selected, all necessary and properly prepared. The conclusions drawn are thoroughly assessed and interesting implications and next steps are finally presented in a very balanced way. The authors provide a straightforward and very critical evaluation of their method using 'optical traps'. It would be interesting to know whether this instrumentation can be applied

to mesotrophic/eutrophic ocean areas as well. What are the limitations there? Reading this paper, I fully agree with the authors' final statement that 'autonomous floats bearing bio-optical sensors have the potential to transform our understanding of the ocean carbon cycle'.

There is nothing fundamentally to criticize in this manuscript, so I can only provide a few comments and suggestions.

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). Do the authors observe a correlation between the number of 'spikes' and/or 'jumps' to water depths which could probably point to zooplankton activities? High resolution in situ photographing of particles might help to solve this problem. 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.

A weakness of the applied method is, at first glance, the different cleaning procedures (rinsing and upward movement of the float) of the transmissometer windows of the different settings of the floats. But, obviously, there is no such effect observed, as outlined by the authors (page 1237 starting line 6).

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?

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?

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.

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

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