Interactive comment on "Carbon Export and Fate Beneath a Dynamic Upwelled Filament off the California Coast" *by* Hannah L. Bourne et al.

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The FOLLOWING pages include the original comments from Giorgio Dall'Olmo's over view document. We have appended queries from the marked up manuscript. *Our Replies will be in blue italicized text. We will add figures as appropriate. First and foremost we thank the reviewer for their thoughtful and comprehensive review; furthermore, we appreciate his willingness to engage in an email dialog to clarify review points; we reproduce that from that dialog as appropriate. Major Responses will be identified by [**Rxx**].*

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This manuscripts presents results from an intensive process study that was conducted on a filament off the California coast. The main dataset object of the study was collected by carbon flux explorers (CFEs) that are autonomous Lagrangian drifting platforms equipped with instruments that collect and image sinking particles over a size range spanning from 30 um to 1 cm. This autonomous dataset is complemented by ship-based transects of water column physical and biogeochemical properties and water currents. The objective of the study is to describe the fluxes of different particles and investigate what processes control carbon flux variability with depth. More specifically, the author invested a lot of time in trying to understand the reasons why these fluxes from those reported in the classic study by Martin (Martin et al., 1987). Results show that different particle characteristics and water column features could be invoked to explain the observed flux variability at different sites and that the exponents of power law fits to the flux data were different from the average one reported by Martin.

Overall I think this manuscript presents a unique dataset that contributes to understand- ing the complexity of carbon fluxes in coastal upwelling regions. My main concern with this work is that uncertainties have not been estimated. I fear that once uncertainties will be properly estimated, some of the results and conclusions could change. For example, some of the slopes of fits to the data in Fig 16 (presenting the main results) could not be significant.

I consider adding the uncertainty estimation as a major revision, because it would require new calculations and an in-depth description of which input sources of uncertainty have been identified and how they were estimated. To avoid making the text to heavy to read, this detailed description could be added as a supplementary material.

I also have concerns with how the results have been presented. I would make an effort to synthesise the results more: there are 20 figures in the main text and most of them containing multiple plots. The text could also potentially be shortened (e.g., section 3.1 may be summarised in a table)

We'll work on condensing the wording for section 3.1 Below is a draft for the requested table..

Loc	Mean MLD_{24} (m)	MLD range (m)	Zeu (SAT) (m)	Zeu (PAR) (m)	Zeu (PAR) range	Mean 0–20 m NO_3	σ_θ @ euphotic base	Mean 0-20 m Sal	Mean 0-20 m c_p	Stock 0–50 m POC	Stock 0–50 m NO_3	Stock 0–50 m NO_3 s.d.
1	19	13-25	21	19	13-25	7.76	25.5	33.748	0.943	685.8	625.3	59.2
2a	26	18-36	29	25	25±3	8.02	25.5	33.637	0.763	557.5	616.3	18.6
2b	26	18-36	29	25	25±3	7.82	25.5	33.636	0.454	410.2	521.9	26.3
4	9	5-14	-	51	51±6	3.15	25.0	33.595	0.159	111.1	371.5	17.8
3	27	11-69	77	49	49±7	1.89	25.8	33.160	0.088	103.9	123.7	18.5

Stocks are in mmol m^{-2}. c_p – units m^{-1};

Please find several specific comments in the attached pdf file. I hope you'll find this review helpful.

Best regards, Giorgio Dall'Olmo (gdal@pml.ac.uk)

[R1] *Nov 18. Dear Giorgio,*

Thanks for the review comments. One comment you made is to suggest a need for major revision and reanalysis data with uncertainties. The statistics for the fits are found in Table 1 [I meant Tables 2 and 3]. I also feel that significant groundwork for the CFE has already been laid with Bishop et al. (2016) and Bourne et al. (2019), both published in Biogeosciences. In this study, we have compared results directly with PIT traps. I feel that a major revision is not necessary. Readers interested in further assessment of the data have the full imagery and data sets available through BCO-DMO.

Jim Bishop

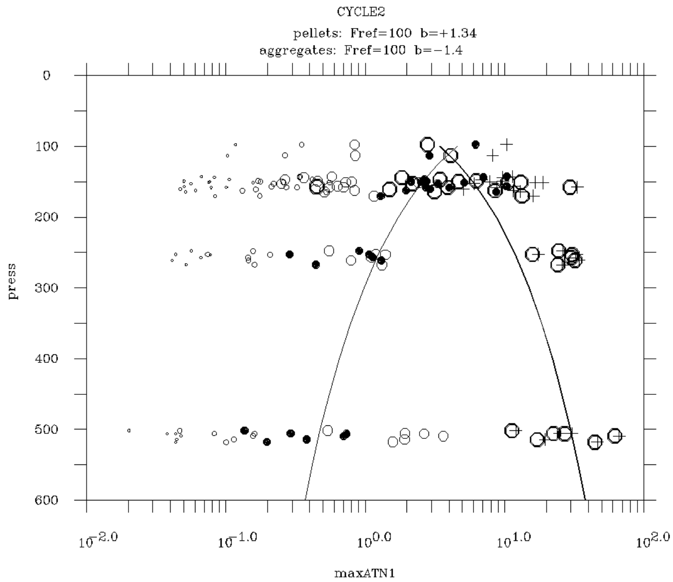
Nov 23. Thanks for your message.

The uncertainties reported in Table 1 [Tables 2 and 3] do not take into account the uncertainties in each of the points used in the regressions, which I expect to be relatively large. Therefore I would expect the uncertainties of your slopes to be significantly underestimated.

I hope this helps.

[R2] *Nov. 23. Dear Giorgio,*

The accuracy and replication of the attenuance data is high. We have shown in Figure 6(f) that choices of attenuance threshold does little to change the total attenuance of an image. Fig 5(a) illustrates replication of the timeseries of attenuance values for data at L2. The apparent noise in Fig 15 is due to the fact that the individual points are snap shots of ~5 hours of time. As the attached plot (CYCLE2bydiveATN.png) shows, the data for individual dives are consistent with pooled results. This is why the data are pooled in the fig 16 plots. The other reason for pooling is for comparison with PIT trap results which represent averages over the same time scale as pooled CFE data.



The choice of Attenuance threshold does influence particle size distributions (SDs) (see Fig 6(b-e). This is why we adopted the hybrid approach for image analysis (use of a low threshold that readily captures whole large aggregates while identifying obvious clusters of similar particles (ovoid pellets) that are treated as one). Fig 7 B shows that two CFE-s replicate in time the SD variations. Figure A1-4 illustrates SDs determined by the fast nearest neighbor and hybrid algorithms, it demonstrates that SDs from both methods replicate well at L1, L3 and L4.

Jim Bishop

Dec 4. Hi Jim,

apologies for the delay.

I am glad that your method for image analysis is reproducible.

However, what I am saying goes beyond that: if you only count (e.g.) 9 particles of a given type in a given image, then the uncertainty in the number of particles should be sqrt(9), given the expect Poisson distribution for the occurrence of particles in a given image.

This uncertainty is crucial in your analysis and should be propagated through your calculations of flux and flux attenuation because the rarest particles (and thus the most uncertain) are those that contribute most of the flux.

I hope this clarifies what I wanted to say.

[R3] Dec 4. 2020
Dear Giorgio,

First we want to thank you for being the first and prompt review.

I understand your point of (sqrt(n)) and Hannah and I will think about this seriously. We are working on responses over the weekend and your comments will be thoughtfully addressed. Briefly, the question regarding statistics of rare particles is why we analyzed and pooled imagery from 4 image cycles for each 6 hour dive and then further pooled dives at depths and further pooled particles into broader size categories and cumulative distributions.

Jim

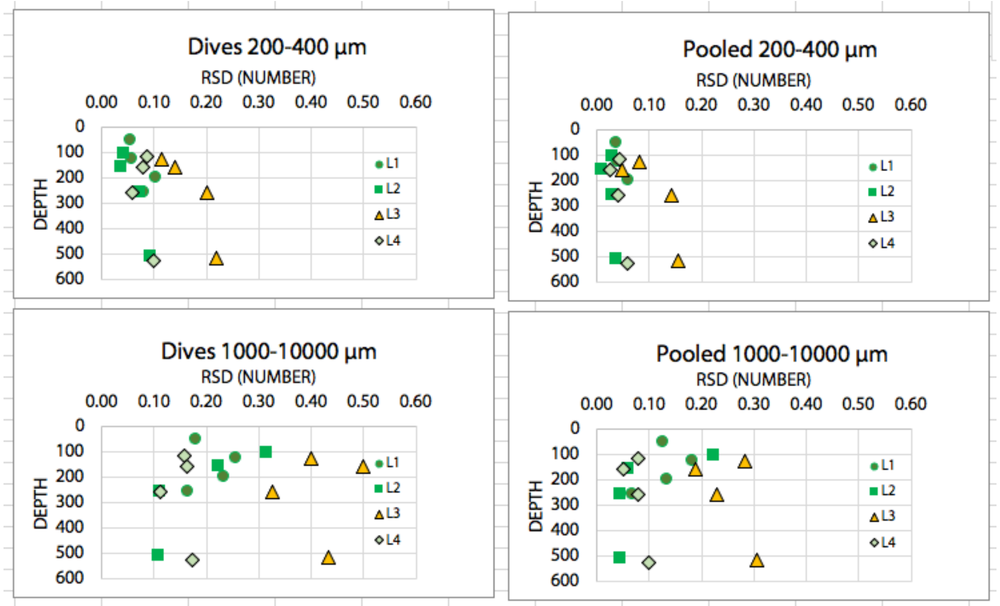
We'll add clarifying statements regarding the pooling process.

[R4] Dec 7 2020.
Dear Giorgio,

A quick note on errors relative to numbers of particles. I spent the day working on your square root of n problem. You are correct in saying that small particles are more precisely observed; however, the results show only by a factor of ~2 difference and these uncertainties are small considering the range of variability we observed. The reason for the relatively small (factor of 2) numeric relative standard deviation (RSD) difference is that we binned results in quasi logarithmic spaced intervals. [No *additional calculations were needed and number frequencies are derived from the primary data sets on line at BCO-DMO.]*

For the case of individual dives (e.g. Fig 15 data), the numeric RSD for most locations for the 200-400 µm size particles is below 10% except at L3 where fluxes are low (20%); for larger than 1000 µm particles, the RSDs are mostly below 20% except in shallow waters and at the low flux regime L3.

When results are pooled for all dives. The numeric uncertainties are less.
For the 200-400 µm particles, RSDs are <7% except at L3 (<15%). For >1000 particles RSDs are <10% in deeper samples and always <30% (including L3).
See graph below. I'll be glad to add this to the Appendix or supplemental materials.



Example of relative standard deviation error based on numeric frequency of particles in size catagories. RSD calculated as SQRT(n)/n. The results show that numerical sources of error are small compared to the range of attenuance flux values seen in the water column.

Is this helping answer your question? The noise in the plots is mostly determined by the natural processes ocurring in the water column and is not an obvious artifact of counting statistics (except possibly in deep L3 data).

Goodness of fit.
A second exercise today was to look at the goodness of fit to a martin function. p and F values can be tabulated and I don't think it changes our interpretations.

When R^2 values are low, it simply means that there is no depth trend. In the case of L2, R^2 values are high, because there is a significant relationship with depth. This can be seen in Tables 2 and 3. I've done similar regressions for individual dive results (Fig 15) and the outcome is the same. The L2 fits are significant.

Sorry to bombard you with this but your questions are important and we want to be able to answer them well.

Hope all is well. Jim Bishop

Specific Comments.

P3. L72. I would zoom in or enlarge the map, which is hard to read.

Will do. The original figure is sharp and the editorial assistant says that it can be on a page in Landscape orientation.

P5. L124. three target depths were 150, 250 and 500 m.
(highlighted along with L128)
P5. L128. only CFE-1 made flux observations at L3 and L4 at 250 and 500 m.

We will change 2nd statement to "only CFE-1 made flux observations at L3 and L4 deeper than 150 m.

P5. L147. 10,070 mATN-cm^2 : mmol POC (R^2 =0.86, and 100,500 mATN-cm^2 : mmol PN (R^2 =0.87).
1.03 mATN-cm^2 cm^{-2} d^{-1} per mmol C m^{-2} d^{-1} (R^2 =0.87).
Provide uncertainties on slopes.
[R5]*Results are reported by Bourne et al. (2019), The data used in regressions is provided in supplemental materials of that paper. Bourne et al. documented errors in POC and mATN, Given that the relative standard deviations (RSDs) of mATNflux data were ¼ of those for POCflux the regression was performed with mATNflux as the X-axis variable.*

*function of POCflux = slope*mATNflux +intercept*

slope	slerr	icept	icepterr	y_err	r2	n
0.96525	0.18605	-1.0707	2.9212	3.7835	0.89671	12
The errors of slope and intercept are 95% confidence intervals. p = 0.000003						

The errors of slope and intercept are 95% confidence intervals. The standard deviation of slope is ~10%. We will state this in the text.

For particle loading, the fit statistics are:

	slope	slerr	icept	icepterr	y_err	r2	n
VA vs. POC	9.63E-05	2.06E-05	-2.13E-03	10.8E-03	0.137E-01	0.876	12
VA vs PN	9.53E-06	1.70E-06	-1.11E-04	8.08E-04	0.121E-02	0.883	15
The errors of slope and intercept are 95% confidence intervals. p values for both regressions is <0.0001							

P6. L151. computationally fast code.

We will change 'fast' to 'efficient'

P6. L165. We choose to set the definition of a "particle" as having 4 contiguous pixels above threshold in order to provide compatibility with interpretation of darkfield imagery (now in progress), where color is important.
What is the size of one pixel?

13 µm (Bishop et al., 2016). We will add this to the text.

P6 L177. the method failed in the case of touching ovoid fecal pellets. Highlighted. No comment.

Will look at statement and clarify if needed.

P8. L211. The aim in this paper is to describe the number and attenuance fluxes of different sized particles and their changes down the water column during the CCE-LTER process study. Highlight no comment.

No change planned.

P8. L213. Figure 8 compares profiles.
it would be good to have a table with a more quantitative comparison.
[R6] *Here are the data that we'll include as a Supplement. The total attenuance of the sample shifts only 3.5% as threshold is changed from 0.06 to 0.02 ATN units. Note: these results are not corrected for the merging of particles as the threshold is lowered or for the fragmentation of large particles as the threshold is raised.*

Threshold analysis data shown in Figure 8.

THRESHOLD (ATN)	PARTICLE NUMBER						ATTENUANCE COUNTS (in thousands)					
	Total	>1000	400-1000	200-400	100-200	30-100	Total	>1000	400-1000	200-400	100-200	30-100
0.20	577	4	37	136	162	238	8786	1087	4064	2601	933	101
0.12	577	12	33	139	150	243	9680	3835	2917	2269	591	69
0.06	519	9	29	140	124	395	10345	5503	2620	1905	280	37
0.04	561	10	27	128	82	314	10527	6478	2224	1652	142	31
0.02	862	10	31	103	86	632	10709	7882	1667	1080	57	22

P8. L224. never-the-less, Highlighted.
No Change

P8. L235. Typo. Transmissomission
Will Fix

P9. L243. UVP data from individual CTD profiles averaged over 5 m intervals represents particles present in ~180 L and did not reliably sample the larger rare particles. Highlighted.
No Change

P9: L245. Typo: yielded
P9: L246. Typo: systematics, is
P9: L280. Typo. L1 was closest Morro Bay
Will Fix all.

P9: L263. density increase of 0.05 relative to surface. PLEASE ADD UNITS.
Will do.

P10. L283. By 200m depth, the salinity and density of all CTD casts converge (Fig. 12). the max depth in Fig 12 is 150 m, so Fig 12 cannot support this sentence.
We will remove the figure reference. The data for L4 and L2 data do superimpose.

P10. L293. Strikeout. CFE-3 was lost due to a shark attack on June 20.
Removed

P11. L312. Grammar. A reasonable assumption is that the properties of surface water (here defined as upper 20 m) at L2 and L4 is a result of binary
Will Fix

P11. L317. mix progressively. Mixed?
Mixed

P11 L321. sinking at a hypothetical rate of 100 m d^{-1} from. Highlighted.
Will clarify why we chose this number.

P11 L324. CFE positions followed a near linear trajectory in time.
Do the CFE displacements agree with the ADCP data?
Yes, This is shown in Figure 9

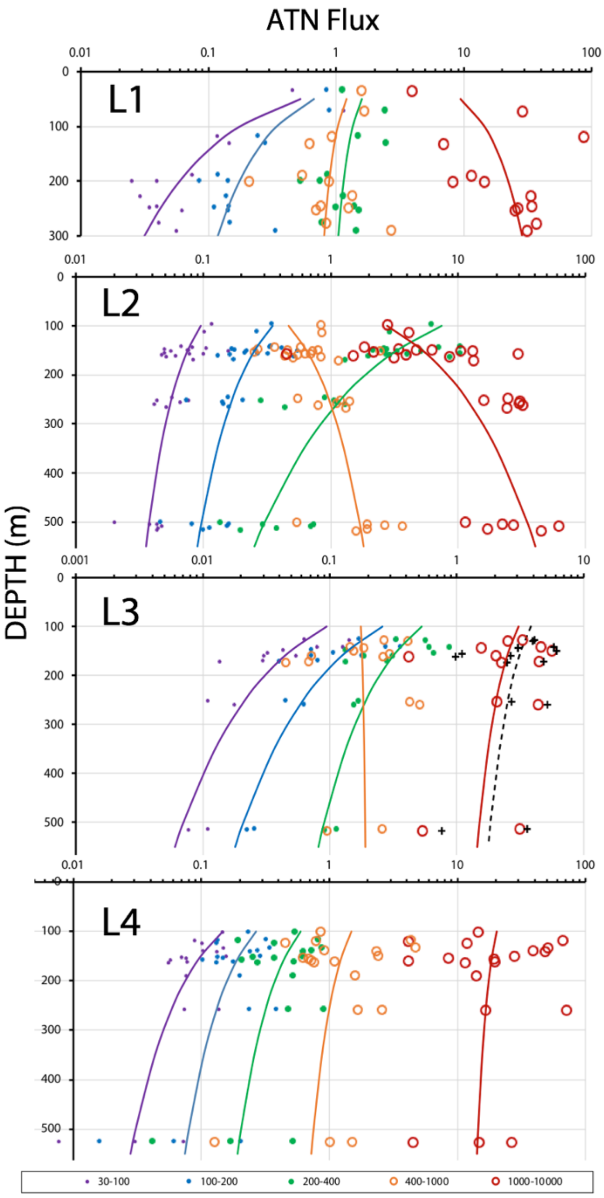
P12 L336. Typo. At L4, Particles settling at 100 m

Will Fix

P13.L364. Figure 15.

very noisy data: I would like to see different plots for each deployment.

[R7] *The reason we did is to show orders of magnitude changes among locations.*
Below is a draft replacement for figure 15 along the lines that you request. What is interesting is the consistent decrease with depth of flux carried by the smaller particles. The largest aggregate category still shows evidence of an increase at L1, a clear increase with depth at L2 and then modest decreases at L3 and L4.



We will replace Tables 2 and 3 with the regression results for all dives. See revised tables below.
We added p values which test if the regressions have a slope that is zero. 14 of 24 attenuance regressions, including all cases at L2, yielded significant non-zero slopes at the 95% confidence level. Similarly 17 of 24 regressions on number flux yielded significant non-zero slopes. The smallest size fractions at all locations have significant non-zero slopes. The regressions for larger sized particles typically have high p values and low R2 values which together indicate that depth has little significance to the regression. Thus our main conclusions are not significantly changed. We will introduce wording in the text that reflects the statistical results.

Table 2. Martin Curve Fits to Attenuance Flux (All Dives)

Location	Zref	size bin	Martin Curve 'b'	b-Error	Intercept	intercept Error	SE_y	R2	n	p
1	50	30-100	-1.57	0.51	-0.280	0.297	0.263	0.735	13	**0.0002**
1	50	100-200	-0.97	0.38	-0.175	0.224	0.199	0.648	13	**0.0009**
1	50	200-400	-0.24	0.37	0.201	0.216	0.191	0.106	13	0.2771
1	50	400-1000	-0.23	0.54	0.081	0.317	0.281	0.048	13	0.4728
1	50	>1000	0.62	0.66	0.974	0.386	0.342	0.204	13	0.1214
1	50	Total	0.37	0.59	1.176	0.344	0.305	0.105	13	0.2791
2	100	30-100	-0.58	0.19	-1.020	0.074	0.130	0.505	29	**<0.0001**
2	100	100-200	-0.80	0.26	-0.455	0.101	0.177	0.510	29	**<0.0001**
2	100	200-400	-1.98	0.41	0.871	0.162	0.283	0.717	29	**<0.0001**
2	100	400-1000	0.80	0.34	-0.330	0.134	0.234	0.373	29	**0.0004**
2	100	>1000	1.57	0.58	0.444	0.232	0.395	0.452	28	**<0.0001**
2	100	Total	0.85	0.31	0.925	0.122	0.214	0.451	28	**0.0001**
3	100	30-100	-1.61	0.59	-1.026	0.206	0.245	0.657	14	**0.0004**
3	100	100-200	-1.57	0.59	-0.586	0.205	0.244	0.646	14	**0.0005**
3	100	200-400	-1.10	0.58	-0.277	0.200	0.238	0.485	14	**0.0056**
3	100	400-1000	0.05	0.83	-0.755	0.286	0.341	0.001	14	0.9198
3	100	>1000	-0.44	0.85	0.486	0.304	0.347	0.071	13	0.3797
3	100	Total	-0.45	0.70	0.583	0.242	0.288	0.099	14	0.1653
4	100	30-100	-0.97	0.42	-0.833	0.145	0.229	0.492	19	**0.0008**
4	100	100-200	-0.75	0.50	-0.573	0.174	0.276	0.283	19	**0.0190**
4	100	200-400	-0.66	0.54	-0.225	0.187	0.296	0.213	19	**0.0466**
4	100	400-1000	-0.42	0.66	0.172	0.230	0.364	0.068	19	0.2827
4	100	>1000	-0.21	0.71	1.307	0.246	0.390	0.015	19	0.6180
4	100	Total	-0.24	0.68	1.376	0.238	0.377	0.021	19	0.5538

Notes: errors are 95% confidence intervals p denotes the probability that slope is zero. **Bold**: <0.05

Table 3. Martin Curve Fits to Number Flux (All Dives)

Location	Zref	size bin	Martin Curve 'b'	b-Error	Intercept	intercept Error	SE_y	R2	n	p
1	50	30-100	-1.57	0.47	6.486	0.278	0.247	0.759	13	**0.0001**
1	50	100-200	-1.26	0.42	5.632	0.245	0.217	0.723	13	**0.0002**
1	50	200-400	-0.73	0.38	5.099	0.224	0.199	0.511	13	**0.0060**
1	50	400-1000	-0.85	0.43	4.386	0.252	0.224	0.531	13	**0.0047**
1	50	>1000	0.14	0.45	3.834	0.267	0.236	0.028	13	0.5826
1	50	Total	-1.43	0.44	6.560	0.261	0.231	0.750	13	**0.0001**
2	100	30-100	-0.55	0.18	5.761	0.072	0.125	0.494	29	**<0.0001**
2	100	100-200	-0.53	0.24	5.068	0.096	0.168	0.341	29	**0.0009**
2	100	200-400	-1.69	0.41	5.440	0.162	0.283	0.646	29	**<0.0001**
2	100	400-1000	0.31	0.30	3.833	0.120	0.211	0.102	29	0.0907
2	100	>1000	1.26	0.46	3.414	0.183	0.312	0.462	28	**<0.0001**
2	100	Total	-0.67	0.17	5.993	0.066	0.115	0.638	28	**<0.0001**
3	100	30-100	-1.30	0.47	5.537	0.162	0.193	0.669	14	**0.0003**
3	100	100-200	-2.17	0.71	5.269	0.247	0.293	0.708	14	**0.0002**
3	100	200-400	-1.32	0.53	4.441	0.184	0.219	0.618	14	**0.0009**
3	100	400-1000	-0.49	0.61	3.480	0.212	0.252	0.144	14	0.1802
3	100	>1000	-0.31	0.60	3.235	0.215	0.245	0.071	13	0.3799
3	100	Total	-1.47	0.51	5.748	0.177	0.211	0.683	13	**0.0004**
4	100	30-100	-0.91	0.42	5.915	0.145	0.230	0.459	19	**0.0014**
4	100	100-200	-0.78	0.53	5.190	0.183	0.291	0.277	19	**0.0206**
4	100	200-400	-0.69	0.60	4.698	0.209	0.332	0.188	19	0.0638
4	100	400-1000	-0.45	0.71	4.189	0.248	0.394	0.064	19	0.2946
4	100	>1000	-0.29	0.56	4.018	0.195	0.309	0.045	19	0.3829
4	100	Total	-0.86	0.45	6.024	0.155	0.246	0.395	19	**0.0004**

Notes: errors are 95% confidence intervals. p denotes the probability that slope is zero. **Bold**: <0.05

P13 L365. Typos. through the data use Marin b factors derived fron
Will Fix

P13.L365. linear least squares fits to the log_{10} transforms of the data.
Given how noisy the data are, I do not think this is the appropriate method to fit these data. I would recommend trying a non-linear fitting technique that takes into account the uncertainties of the data (both on the x and y axes). A **bootstrapping technique** could help you here.

[R8] *First, we are evaluating the hypothesis that the Martin formula can be used to fit the data. The Martin formula requires a fit to the relationship shown in Equation 2: $log_{10}(F) = b*log_{10}(z/z_{Ref}) + Log_{10}(FRef)$. Z/Zref is precisely known. So its choice as X parameter is valid. Our results show that the function performs well for particle classes that clearly have their origin the euphotic layer – although 'b' factors are often different from Martin. Specifically, the ovoid copepod fecal pellets follow this formula well. We've provided fit results (including errors) for pooled results in Tables 2 and 3 above in **R**7. [see also **R4**]*

Crucially, you should present realistic estimates of the uncertainties (e.g., 95% confidence intervals) of all the parameters you estimate using these fits. It will then be fundamental to consider these uncertainties in any discussion focused on the estimated parameters.

*In [**R4**], we present uncertainties due to counting statistics. We have also tabulated the effect of choice of threshold on the image analysis statistics that were shown in Fig. 8. [**R6**]. Sample attenuance increases by only ~3.5% from threshold choices of 0.06 and 0.02 ATN units. There is little source of error from the choice of 0.04 ATN units as a threshold.*

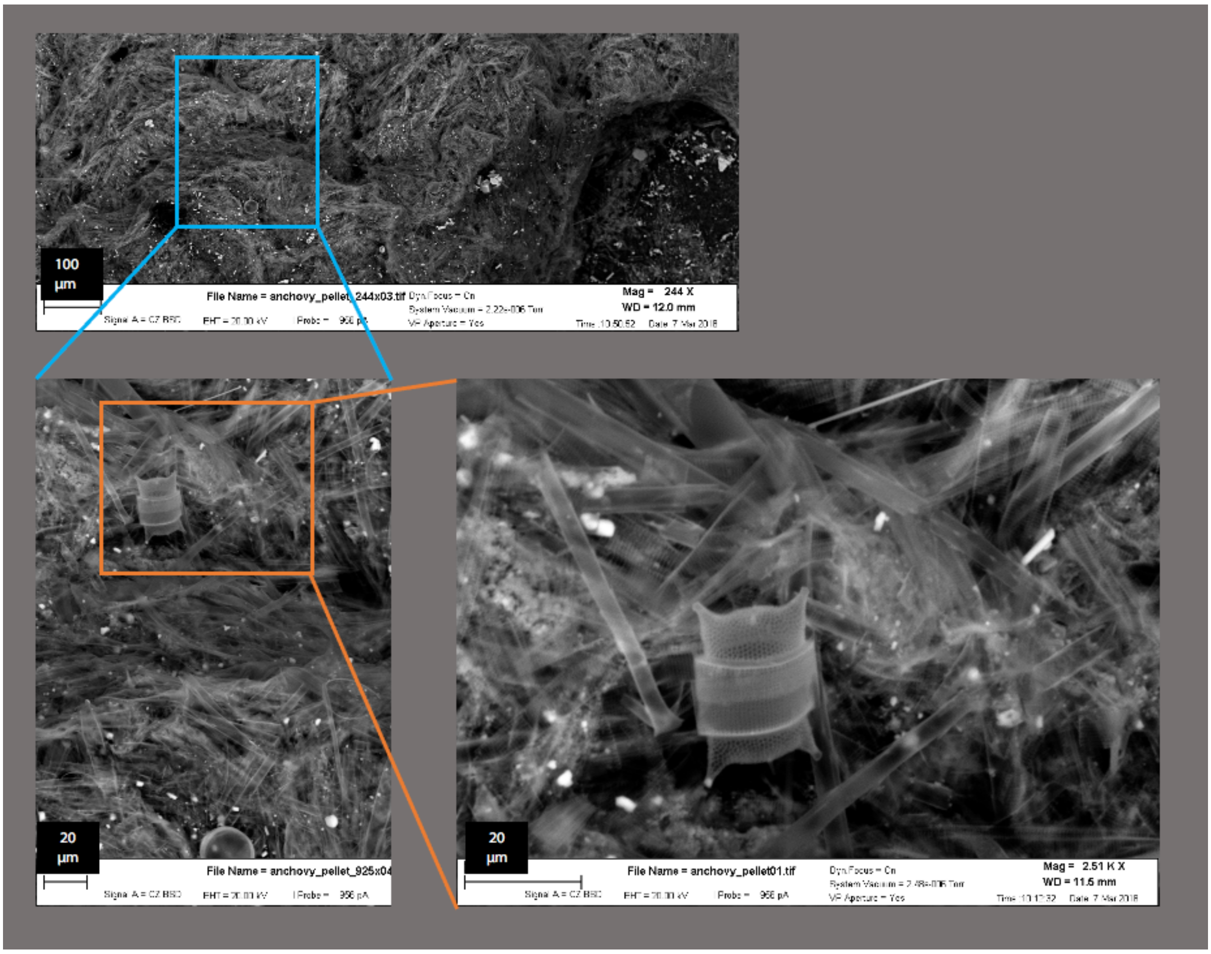
P13. L373. Fig 16.
This figure is quite important, but really hard to read. I would recommend making it larger and potentially using colours to help the reader distinguish the most important pieces of information. I could also help if you "boxed" each of the stations.
Do you really need to present 3 plots for station? It makes it really hard to compare results across stations.

We will make the figures more readable. The original figures are sharp.. Editorial says that we can address this issue. Need to decide on fate of Figure 16.
\
P13. L378. Export.
what are the uncertainties associated with these export estimates? The majority of the fluxes are due to few very rare and very large particles. It would be extremely important to propagate uncertainties (starting from the Poisson-distributed numbers of particles) from the images to the flux estimates.
These uncertainties should then feed in the fits of the Martin b exponents, which should also be reported with their associated uncertainties. Given the large uncertainties, I suspect that these exponents may not be significant.

We have responded above to this concern. The uncertainty due to frequency of occurrence of particles is small compared to natural variability.

P13. L380. anchovies were primarily grazing on diatoms.
This is an important piece of information: could you back it up with a reference?

[R9] *This point was also raised by Reviewer 2. Reference: Bourne 2018. We'll add Hannah's SEM imagery (below) to the Appendix. Fish are normally considered grazers of zooplankton and thus not considered an important factor in export. Anchovies in this case were feeding on diatoms. Thus their contribution to export is as high as any other primary grazer.*



Appendix figure: 244, 925, and 2500X magnification images of one of the anchovy fecal pellets captured by a CFE-Cal at location L1. The image shows that the material in the pellet is dominated by diatom frustules.

P13. L383. Typo. (Collier and Edmond, 1984); Anchovy…
P14. L389,390 Typos. eveny … sample stage At this
Will fix.

P14. L395 Fig. 16. no carbon estimates are presented in Fig 16.

The text says that the ratio of mATN flux:POC flux is 1.The fit statistics are shown in [R5]. We will state this in the caption.

P14. L399. Interestingly, none of the locations showed a strong decrease of flux with depth as one….
Fig 14 shows that large particles/aggregates occurred in relatively low numbers in CFE photos.
The probability of a particle appearing in the images should follow a Poisson distribution. Thus low numbers of particles should be associated with relatively large uncertainties. Because the large particles/aggregates contribute the majority of the flux, it would be crucial to propagate *all* the uncertainties from the particle detection to the estimation of the flux attenuation.

I suspect this exercise will demonstrate that the slopes of flux vs depth may not be significant at all stations and therefore could potentially change some of the conclusions of this work. This is especially important because the depth ranges over which the flux attenuation is estimated are relatively narrow, which makes estimates of the slopes more uncertain.

*We've responded above in [**R4**]. In terms of biology, the depth range covered by our sampling includes most of the living community.*

P14. L400. Fluxes at L1 and L2 show little change or increase with depth while at L3 and L4, fluxes decrease slowly with depth with 'b' factors - 0.4 and - 0.3,
we need uncertainties to understand how much we can believe these changes.

Uncertainties of fit are in Tables 2 and 3. We will modify discussion. An important point is that a low R2 value means that there is no significant dependence of flux on depth.

P14. L402. Total POC Flux increased with depth at L2.
please provide uncertainties supporting this statement
We will clarify our discussion. In the case of L2, all of the fits are significant (Table 2).

P14. L406. Fits using the fast method
To help the reader, it may be best to refer to this as Method 1
Will do.

P14. L415. Typo. progressively droped with
Will fix.

P15. L418. were detected in the 300m to 450 m.
Units should be separated from values. This comment applies to many instances throughout the manuscript.
Will fix everywhere.

P15. L428. Typo. biologically medicated uptake
Will fix.

P16. L446. such counterclockwise motion, consistent with ADCP data (Fig. 4). Water on the shelf
it would be nice if Fig 4 matched Fig 3: i.e. 4 subplots at different times.
Not sure how this can be done as the binning intervals for satellite (8 days) and altimetry (5 days) were different. That said, we tried to superimpose chlorophyll satellite imagery and deployment tracks PIT and drifter, and positions for CTDs and CFEs for each of the 4 locations to provide spatial context (Figs 5, A1-1, A1-2, A1-3.

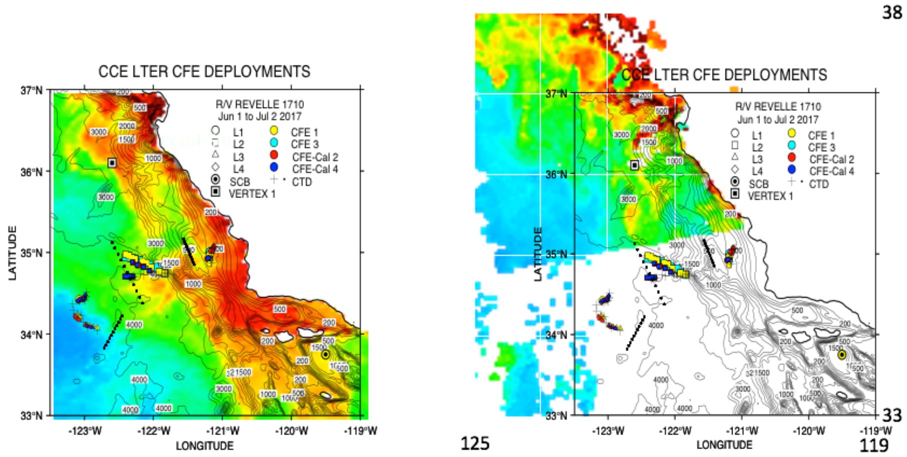
P16, L455. The Siegel et al. (2014) climatological flux for June in our region is shown in Fig. 15.
I am not sure this is a fair comparison. In such a dynamic system and with your data focusing on one specific filament?

The paragraph ends with… "The point of this comparison is that filaments make a disproportionately large contribution to carbon transfer to deeper waters and that such filaments need to be included in models. Deutsch et al. (2020) describe new eddy resolving simulations of biogeochemical processes in the California Current regime which can be informed by the work described here." We wanted to use Siegel et al. (2014) as a point of discussion.

Typo: Fig 15 does not contain any estimate from Siegel et al. Should be Fig 17.
Will fix.

P16. L472. classic Martin et al. (1987) study. Martin's.
Was this in a filament? You've just made the point that these filaments are special places with respect to C export, so I would expect you to compare your results to similar situations.
P16. L472. The comparison with VERTEX results is justified since Point Sur has been identified as an area of frequent filament development
I am not an expert in filaments, but I would expect filaments to be highly variable in time and space. Therefore I am not sure this is really justified.

Martin et al. reported high productivity and active upwelling conditions. CZCS imagery from June 1984 does show evidence for a filament near Vertex I. We don't have data on deployment and recovery dates and times from Martin. We consulted available satellite imagery and feel that our statement is justified.



June 2020

CZCS June 1984

Composite of June 1-June 8, Jun 18-25 1984

153-160 & 169-176

Data June 1984 show evidence for filament activity near Pt Sur and the VERTEX location. We also show June 2020 which shows similar high chlorophyll conditions in the vicinity of VERTEX 1.

P17. L496. … L3, Martin-extrapolated fluxes were lower.

Table 4. Martin Curve Fit parameters for PIT trap data.

	Martin Curve 'b'	b-Error	Intercept	intercept Error	SE_y	R2	n	p
L1	-0.3516	0.2265	1.4094	0.0749	0.066	0.710	3	0.3622
L2	-0.8529	0.2115	1.2869	0.0531	0.062	0.943	3	**0.0260**
L3	-2.1188	0.2739	1.1709	0.0448	0.054	0.984	3	0.0812
L4	-0.3069	0.0912	1.4510	0.0184	0.022	0.920	3	0.1825

Notes: errors are 95% confidence intervals. p denotes the probability that slope is zero. **Bold**: <0.05

Only L2 had a classic Martin curve. All other locations are significantly different than -0.86. We will modify the PIT trap and CFE Total flux comparison in Fig. 16 by plotting Dive specific ATN fluxes and fit vs. Trap fluxes and fit.

P17. L501. leads to more efficient transfer of POC through the water column. More.

is it "more efficient transport through the water column" or that you might have sampled a nepheloid layer at L1 and its transport at L2?

We rule out nepheloid layer transport here.

P18. L509 We explore reasons why the flux profile from the coastal station VERTEX1 (Martin et al., 1989, Fig. 1), which follows the classic curve, differs from results of this study.

This discussion must include an assessment of the [uncertainties].

We have addressed uncertainties above. Will clarify in discussion.

P18. L517. We add to this the observation that, large particles sampled by in-situ filtration show little shift in organic carbon percentages from the base of the euphotic zone to 500 m.

It would be interesting to understand how the relative contributions of different particles changed as a function of depth. This is important because different particles may have different POC:ATN relationships.

Bourne et al. (2019) discuss the POC:ATN relationship and uncertainties in detail. The notable result is that the data from 150 m do not appear to be influenced by the kinds of particle present. We state clearly that the calibration study was restricted to the upper 150 m. We are not sure what more can be added.

P18. L519. A caveat for the following discussion is that the Attenuance:POC flux relationship and its assumed constancy with depth is not a factor in the interpretations that follow.
You should probably then add this source of uncertainty as well to your flux uncertainty estimation.
We state that the classes of particles at 250 and 500 m are similar to those seen at 150 m. (Bourne et al., 2019), for this reason, we would expect similar POC:ATN relationships. There is no way to quantify these uncertainties further.

It is important to reiterate that attenuance is precisely and accurately determined and reproducible across instruments. Attenuance is used as an optical proxy for POC. As more calibration data are obtained, then our understanding of the optical proxy for POC will improve. For example, the optical attenuance results from Bishop et al. (2016), remain valid as they are physical units. With Bourne et al. (2019), we've reevaluated the POC fluxes downward by a factor of 3.
P18. L521. Figure 18 depicts four mechanisms.
please expand the caption to make the figure self explanatory. Once more: this must include an assessment of your uncertainties.
Proposed new caption.

> *Figure 18. Four mechanisms that can lead to non-classical particle flux profiles. (A) Temporal Delay (Giering et al., 2016); (B) Vertical Migrators (Turner, 2015, Bishop et al., 2016); (C) Physical Subduction (Omand et al., 2015, Stukel et al., 2018); and (D) Lateral Advection (Alonso-Gonzalez et al., 2009, Pak et al., 1980, McPhee-Shaw et al., 2004, Chase et al., 2007).*

We have addressed the uncertainties above.
P18. L536. Fig 5.
at 500 m I can see a decreasing trend with time…
Yes, this means that the apparent increase of aggregate flux at depth is stronger.
The 250 m data show an upward trend.

P20. L574. Some larvaceans create and discard up to 26 feeding webs a day.
Reference?
Riki Sato, Yuji Tanaka, Takashi Ishimaru, House Production by Oikopleura dioica (Tunicata, Appendicularia) Under Laboratory Conditions, Journal of Plankton Research, Volume 23, Issue 4, April 2001, Pages 415–423, https://doi.org/10.1093/plankt/23.4.415

P21. L607. flux, their activities may not fully explain the depth increasing flux profiles observed at L2.
Before making this conclusion, I would estimate the uncertainties in your calculations. I suspect these uncertainties are rather large and may modify your conclusion.
Uncertainties are discussed above. The L2 regression data are robust.

P21. L625. Typo. Fig. 19.d. Should be 18d.
Will Fix.
P22. L656. the nepheloid layer at L1 is dominated by smaller particles and not >1000 µm sized aggregates. Highlighted.
No change.
P23. L677. Highlighted. At L2, 200-400 µm sized olive colored ovoid pellets contributed on average. 50% - Ref 2 also picked up on this…
Will clarify where the results come from.

P23 L678. Highlighted. >1000 µm sized amorphous aggregates dominated flux at depths greater than 150 m.
P23. L683. Interestingly, flux profiles for particle classes smaller than 400 µm, always had negative b factors which were more closely in agreement with the classic Martin fit.
I wonder how much this is due to the large numbers of, and thus smaller uncertainties associated with, smaller particles.
We have responded to sqrt(n) point above. This does not change the conclusions. See discussion above.
P24. L698. some evidence that westward moving currents laterally transported waters with POC from the continental shelf,
this statement seems to contradict the conclusion in lines 658-9
There may have been nepheloid layer particle transport from L1 offshore; however, no connection was evident in CTD transects or in CTD profiles during the intensive occupations at each study locale.
P24. L713. Typo. Unlike, sample collect…*Will Fix.*