Review of Bourne et al. *BGD* revision

General: The re-focused writing is much more concise and straightforward to read after the current edits, this is very much appreciated. It was quite a slog, before. However, the clearer writing allowed me to pick up on a few things I missed the first time through. The two main issues that remain are 1) checking on the details of the counting uncertainty calculation, and revising if necessary; and 2) conveying a level of uncertainty in ruling out the different hypotheses for increase flux at depth (M1-M4) that is more in line with the amount of evidence that is available. There are a few minor and technical issues with the text that should be addressed as well. The figures are much improved.

We appreciate the help. We respond to major points (1) and (2) as follows.

{R1} Counting error estimates were correctly calculated. Derived number and attenuance fluxes were extrapolated using cumulative distributions to the boundaries of each of the 5 size categories. Number flux when back calculated to raw number will not yield integer values. We checked the data, the inferred number of particles is consistent with imagery and the math is fine.

{R2} The subduction of un-grazed phytoplankton or surface layer POC is not a significant process. Also there is no support of lateral transport as explaining the origin of aggregates or flux increase. Levy et al. (2012) model subduction processes in western boundary current regimes. Indeed (Bishop, Smith, and Baker, 1992) observed strong signatures of particle subduction at the Gulf Stream front. Our observations and those of Stukel et al. (2018) do not support subduction impacts in the CCE region deeper than ~100 m. We have clarified our text below.

Reference: Bishop, J.K.B., Smith, R.C. and Baker, K., (1992) Springtime distributions and variability of biogenic particulate matter in Gulf Stream warm-core ring 82B and surrounding N.W. Atlantic waters. *Deep-Sea Research*. 39 (1A) s295-s326

We respond to detailed suggestions below.

[1] Lines 9-10 (abstract): The inserted text in the first sentence is important but now the sentence is long and awkward. At least insert commas between listed items, better would be to break into two sentences. *{done}*

Line 12: "intense" should be "intensive"? *{done}*

Line 19: Perhaps replace "Martin" with "the canonical Martin profile", or something similar, sothis statement is clearer to all abstract readers regardless of background. *{done}*

[2] Line 74: Were L1, L2, L3, L4 intended to follow the evolution of a water parcel over time? Or was it just coincidence that L1, 2, and 4 coincided with the aging of the filament (and that L3 was outside of it)? Maybe a short statement describing the rationale behind site choice, if there was an overarching goal. *{lines 77-78: Clarified}*

[3] Line 146: Change "time" to "times" *{done}*

[4] Line 232-244, 517-520, and data supplement: I appreciate that this counting error analysis has now been added to the text, but there is some missing detail that precludes its evaluation. I would like to see specific, directly worded text briefly explaining how counting error was determined. Counting error estimates are dependent on the error threshold chosen to propagate (±1 particle? ±10 particles? Or (√N)/N?) This detail must be included for the reported uncertainties to be meaningful. *{lines 215-217; lines 245-255; lines 265-268; clarified}*

[5]… Furthermore, I suspect there may be a problem with how counting error was calculated. I downloaded the supplement with the dive-averaged number flux errors. From notes in the header lines it appears that the relative error is calculated as (√no)/no, where "no" is the number of particles collected during the dive (this detail, by the way, is what needs to go in themain text as per my comment above). However, the values in the column labeled "no" are notintegers, so I think there is some inconsistency. Please address this and if necessary revise the counting uncertainty analysis *{Lines 245-268: the calculation is correct}*.

[6] Section 2.4: There are statements within this section that compare estimates to other reports in the literature. These statements should be pulled out of the methods section and moved tothe discussion.

{Lines This section is simply methodology – we believe it belongs where it is}

[7] Lines 354-355. The sentence ends with a preposition*{done}*.

[8] Line 402. Data *are* (not *is*) *{done}*

[9] Lines 411-419. Most of this paragraph should be merged into the discussion – it contains interpretation and comparison to other work, not just a report of the results.

{Moved… Now Sec 4.4, Anchovy Faecal Pellets: Enhancement of Carbon and Phosphorous Transfer Efficiency. }

[10] Lines 420-437. My original comment requested more evidence to support the inference of fastsinking speeds from the focusing of ovoid particles around the stage perimeter (and the later statement that these particles were "obviously" fast-sinking). For instance, one could alternatively speculate that particles found around the perimeter are the ones most likely to stick to the funnel walls on the way down and then fall in clumps.
If there is no specific evidence supporting the speculation that clustering around the stage perimeter indicates fast sinking speed, please rephrase lines 421-423 using more tentative language than is currently there. *{we will not weaken this statement as we elaborate below}*

[11] The new paragraph (lines 427-437*)* provided here in the revision really just supports the generalidea that large, dense particles sink quickly – but this is not really a controversial idea, and in my opinion it does not require this much justification. I would recommend removing the new paragraph altogether, but if it ends up being retained, it should be moved to the Discussion. In this case, the first sentence is also phrased as a question and should be revised.

[10] and [11] Given that the reviewer has focused on this issue, we feel that the topic should be addressed fully. We have broken out this discussion under a subheading. Clarified text is below.

*Lines 473-491: **Note on particle size-distributions**. During review we were asked if the funnel of the CFE led to artificial aggregation of particles forming either the ovoid faecal pellets or >1000 µm aggregates and also if we can provide further evidence that the ovoid faecal pellets and aggregates were relatively fast sinking.*

Did the funnel design used by the CFE lead to biased size-distribution results? We have seen no evidence that the polished, electrically-neutral, and steeply-sloped titanium funnel used in the CFE played any role other than the physical focusing of fast-sinking faecal pellets at the edges of the sample stage. During post recovery inspections, we have not found any evidence of fouling of the funnel surface, even after 40 day deployments. We also note that successive images taken 20 minutes apart show that the ovoid particles arrived individually (Bishop, 2020a) and that similar pellets and pellet number fluxes were noted in PIT gel–sediment trap samples at L2 (Connors, et al., 2018). The answer to the first question is no.

Are the particles fast sinking? The model of Komar et al. (1981) when applied to an average sized ovoid pellet (ECD = 250 µm, length/width ratio = 1.5; Figs. 6g and 8b), having an excess density relative to sea water of 0.2 g cm^{-3}, sinking through 10 °C water (viscosity = 0.0144 poise) would have a sinking velocity or 350 m d^{-1}; the smallest particle in this category (ECD=200 µm) would sink 200 m d-1. For aggregates, we used the Bishop et al. (1978) modification of the broad-side sheet settling model of Lerman et al. (1975). An aggregate with an ECD of 1500 µm and net excess density of 0.087 g cm-3 would settle at 300 m d^{-1} (Bishop et al., 1978). For reasons outlined in Bourne et al. (2019) we believe the aggregate model may overestimate sinking speed, but by no more than a factor of three; so, 100 m d^{-1} is a reasonable lower limit. Henson et al. (1996) measured sinking rates for similarly sized aggregates and discarded larvacean houses to be ~ 120 m d^{-1}. Further evidence for fast sinking speeds for the particles that contribute to flux comes from time series sediment trap deployments (e.g., 175-300 m d^{-1} from Wong et al. 1999 (station PAPA), and >190 m d^{-1} from Conte et al., 2001, Bermuda time series). The answer is yes.

[12] Lines 521-522: Consider rephrasing so as not to start with a question stated in passive voice… *{done}*

[13] Line 524: "Confidence" should not be capitalized *{done}*

Line 555-560*:* I think this paragraph is meant to convey that there is mixed evidence about whether the flux profile was at steady state, but it is a little disjointed. Maybe a unifying sentence stating this explicitly would help pull together the different statements. Also, while I agree that Fig. 5a does not show a clear trend of increasing or decreasing flux, nor does it showa temporally constant flux profile. In line 559 I would change "particularly at L2" to read "although it is not constant, either" (or something like that – some rephrasing might also be necessary).

{Line 630, we say no "major" trend… we leave statement as is}

[14] Section 4.3.3. This is getting beyond my expertise, but I don't think the presence or absence of chlorophyll fluorescence at depth is sufficient, by itself, to rule out physical subduction as a factor. Also, just because a filament develops around a cyclonic eddy does not mean that there cannot be subduction at the edge (e.g., see Figure 2 in Lévy et al (2012), *GRL*, 39(14). https://doi.org/10.1029/2012GL052756). Rather, the vertical displacement of the σ_θ=26.2 isopycnal and the changing temperature and salinity structure below ~100 m from L1, to L2a, to L2b (Fig. 11a,b,c) suggests to me that there could have been physical processes at work.

Unfortunately, I don't think you have the necessary spatiotemporal resolution in your physical observations to say much one way or another about physical subduction. I suggest rewriting this paragraph to describe the possibilities, but without trying to rule out physical subduction asa potential driver.

{L1 was a site with active upwelling, upwelling was evident at L2a as evidenced by vertically displaced isolines.} Physical subduction is not supported by observations.

Lines 705-710. Modified wording: "While, subduction of ungrazed phytoplankton may also occur at fronts; there is no evidence to support a subduction process connecting the surface layer to 250 m at L2 as chlorophyll fluorescence profiles (Fig. 11) show minimal presence of phytoplankton material deeper than 100 m. (Fig. 12). Our findings are consistent with those of Stukel et al. (2018) who found in targeted study of fronts in the CCE region that subducted particles were completely remineralized by 150m. Eddy driven subduction and frontal subduction processes do not play a role in fluxes seen in our data."

[15] Line 657: Add a few transitional words at the start of the first sentence, ("On the other hand…") *{done}*

[16] Line 658: "The" should not be capitalized. *{done}*

[17] Line 664-665: This sentence is out of place – seems like it belongs in the prior section (4.3.3). *{Removed sentence}*

[18] Last paragraph of section 4.3.4: This should be rewritten to convey more uncertainty about thedrivers. Your evidence is not strong enough to rule out non-steady state fluxes or subduction, so the statement that "all other candidate mechanisms are not supported" should be removed.

Created new section 4.3.5 Recap of flux mechanisms.

We are left with a puzzle. All small particle classes smaller than 400 µm exhibit a decrease with depth at all locations consistent with a single origin within the euphotic zone and progressive particle remineralization during sinking. At the same time >1000 µm aggregate fluxes either increase with depth (L1 and L2) or slowly decrease (L3 and L4); the increased flux cannot be supplied by biologically mediated consumption and repackaging of the smaller sized sinking particles because the fluxes of <1000 µm particles are too low. Our strongest candidate mechanism to explain the >1000 µm POCATN flux profile at L1 & L2 is active transport (M2) or some related process transforming the DOC pool to POC flux at depth. Due to the strong decline in measured new production at L4, non-steady steady state export (M1) may have been an additional contributing factor at L4. The subduction (M3) and lateral transport (M4) mechanisms are disfavoured. [

19] Section 4.3.5. This is very disconnected from the rest of the paper, I suggest removing it (or at least condensing to a couple speculative statements and folding into the 4.3.4 summary paragraph). *{We have moved this section to conclusions – and rewritten}*

[20] Line 721: Remove comma after "Unlike…" *{done}*

[21] Line 722*:* Change to "…(or 16 months at 2 hours)" *{done}*