

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.

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.

Line 12: “intense” should be “intensive”?

Line 19: Perhaps replace “Martin” with “the canonical Martin profile”, or something similar, so this statement is clearer to all abstract readers regardless of background.

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.

Line 146: Change “time” to “times”

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 $(\sqrt{N})/N$?) This detail must be included for the reported uncertainties to be meaningful.

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 $(\sqrt{no})/no$, where “no” is the number of particles collected during the dive (this detail, by the way, is what needs to go in the main text as per my comment above). However, the values in the column labeled “no” are not integers, so I think there is some inconsistency. Please address this and if necessary revise the counting uncertainty analysis.

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 to the discussion.

Lines 354-355. The sentence ends with a preposition.

Line 402. Data *are* (not *is*)

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.

Lines 420-437. My original comment requested more evidence to support the inference of fast sinking 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.

The new paragraph (lines 427-437) provided here in the revision really just supports the general idea 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.

Lines 521-522: Consider rephrasing so as not to start with a question stated in passive voice...

Line 524: “Confidence” should not be capitalized

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 show a 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).

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 $\sigma_{\theta}=26.2$

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 as a potential driver.

Line 657: Add a few transitional words at the start of the first sentence, ("On the other hand...")

Line 658: "The" should not be capitalized.

Line 664-665: This sentence is out of place – seems like it belongs in the prior section (4.3.3)

Last paragraph of section 4.3.4: This should be rewritten to convey more uncertainty about the drivers. 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.

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).

Line 721: Remove comma after "Unlike..."

Line 722: Change to "... (or 16 months at 2 hours)"