General comments

This paper adopts the use of a 1-D mechanistic model of particle dynamics, originally developed for the ocean mixed layer, to examine the role of fragmentation on export flux and transfer efficiency from ~100-1000 m. This mesopelagic version of the SISSOMA model assumes that aggregation is negligible below the mixed layer and updates the fragmentation term to be representative of particle-associated copepods (PACs). The experiments reveal the isolated and combined impacts of remineralization and fragmentation on POC flux with depth, particle size distribution with depth, and the contribution of individual processes to POC flux attenuation. When compared against backscatter-derived estimates from BGC-Argo float observations from the GLOBESINK database, these results suggest that the initial decline in particle size observed in the top 200 m cannot be reproduced without simulated fragmentation. The model experiments are idealized such that they can disentangle the processes of sinking, remineralization, and fragmentation; they are not intended to represent every process, thus do not perfectly match the observations. The authors do an excellent job of discussing the model limitations and hypothesizing on mechanisms/factors not represented and how that would affect flux attenuation and particle size distribution (especially L483-485). The results are strengthened by a sensitivity analysis of the model parameters and structural assumptions that generate similar results. The results provided a strong argument for including fragmentation in Earth system models and ocean biogeochemistry models used to study the carbon cycle.

The paper addresses scientific questions regarding particulate organic carbon flux attenuation in the ocean, which are highly relevant to the global carbon cycle and within the scope of Biogeosciences. The scientific methods and assumptions are valid, and the assumptions are clear. However, the description of experiments and calculations are not sufficiently complete and precise to allow their reproduction by fellow scientists. Additional mathematical formulae need to be defined. The results are sufficient to support the interpretations and conclusions presented; however, the results could be expanded for completeness. Furthermore, description of the results illustrated in figures is imprecise or incorrect. Below are my specific comments on how to improve the manuscript. With these adjustments, I think the paper will be a valuable contribution to the field of biogeosciences.

Specific comments

1. Methods need more detail.

1a. “Psi” L121: Here you call it the fraction ingested, but in the Supplement you call it the preference for that particle based on its size. These are two separate things. Preference, if it exists, should be a multiplier on POC concentration (POC_i,j) in Eq 3. L305: I would argue that psi is a metric of fragmentation strength, not the ratio of PAC to POC. S2.1: Again, I would not call the PAC:POC ratio “fragmentation strength.” I think psi is fragmentation strength. S2.1: Again, psi is not a preference, this is how much consumed.

1b. L124-127: Did you calculate this ratio from the Mayor outputs? It is not an explicit result presented in that paper (i.e. no figure or table, no mention in text). Alternatively, UVP data could be used to estimate ratios. Also, what are the units of POC concentration? Is it number or mass? If it is number, then fragmentation would make more particles, thereby increasing concentration and PAC abundance, whereas mass would have the opposite effect.

1c. Eq 3: Is not a Holling Type III. More parameters need to be specified? What are they?

Disk form: $ingest(POC) = \frac{a \cdot POC^b}{1 + a \cdot h \cdot POC^b}$.

Michaelis-Menton form: $ingest(POC) = \frac{POC^b}{k_{1/2} + POC^b}$.

Ivlev: $ingest(POC) = 1 - e^{-a \cdot POC^b}$.

From the MEDUSA documentation (note I could not find this information in your manuscript nor the Mayor et al. 2020 citation), it seems you use the Michaelis-Menton form. And when there are multiple prey types like you have, grazing is somewhat like your $\frac{POC_{i,j}}{POC_T}$, but more precisely follows the MEDUSA formulation like this:

$$Gm_X = \frac{g_m \cdot p_{mX} \cdot X^2 \cdot Zm}{k_m^2 + F_m} \quad (54)$$

where X is Pn, Pd, Zμ or D.

$$F_m = \left(p_{mPn} \cdot Pn^2\right) + \left(p_{mPd} \cdot Pd^2\right) + \left(p_{mZ\mu} \cdot Z\mu^2\right) + \left(p_{mD} \cdot D^2\right) \quad (55)$$

In this case “p” is preference.

- L133-135 and S2.2: Appendix D on fragmentation in K&V said they distributed the mass equally across all smaller size bins. The size bins themselves may be logarithmic, but not how much mass gets allocated into each. Did you alter that to make it a logarithmic distribution? If you indeed did distribute it logarithmically, then please give the equation as to how. It seems like there would also be a parameter for the proportion that goes to (i-1), etc.

- L136-137: Does that mean the psi fraction is ingested and (1 - psi) is remineralized? Please describe.
- L142-143: Why did you choose this range of density?
- L262-263 (Fig 4 caption): Were these two Martin curve exponents from a Martin et al. paper? Or diff ref? Either way, a reference is needed.
- L441: Need more information in the Methods section about the PAC. For example, the PAC is an ambush feeder that is only eating a part of the particle, hypothesized that it is mining protozoans off of it.
- S1.1: Biomes were grouped by eye? No clustering algorithm or anything like that? Please state.
- S3: How is DOC included in the size calculation? What is the size of DOC?
  - S3: There is nothing about DOC in Section 2.1.
- S4: Does remineralization take mass away, but increase porosity, thus still holding onto water, like in K&V? Please state.

2. Imprecise language.

- L42-45: Total export flux estimates of 4-12 PgC/yr are an example of uncertainty, but T_eff variability in time and space is expected. This is different from uncertainty as exemplified by total export flux estimates between 4-12 PgC/yr. What is the uncertainty in T_eff at a given time and location on average? If unknown, frame as variability rather than uncertainty.
- L214-215: "In most cases" This is not easy to see/assess if accurate. Some lines suggest stabilization very shallow, others increase with depth. Hard to see if there are regional or seasonal patterns to when it decreases vs. stabilizes. Fig 3 shows this is only a pattern for 2 regions, not most.
- L216: I would put EQU in a separate category of moderate seasonal variations. > NHS, SHS, NP, but < NHHL and SO. ~ 400 m there is a 50 um difference across two seasons.
- L223-225: Replace "baseline" with "global mean" or define what the baseline is if not that.
- L342: Not clear why this is called "further sinking runs." Should be something like more even size distribution runs?
- S3: "As with the sensitivity tests in Section 3.3, higher slope values present mid-mesopelagic average ESD maxima (Fig. S15c)," should say that higher slope values have deeper ESD maxima.
- S3: "At greater and increasing slope value (Month 6 to 12; Fig. S15a) …" These are all increasing but not all greater. Months 6-8 are low values, 9-12 are high.

3. Text states something I do not see in the figures
- L222: Looks like 95, not 90.
- L299: Fig 5 red bar on left says 67%, not 56%.
  - Fig 7: Shows 56%, which is a different value for Frag with 0 slope than Fig 5.
- L394: The minimum size looks shallower for 8 (yellow) compared to 5 (cyan) to me.
- L395: "higher POC encouraging" But months 5 and 8 both have the same flux so how can you call one higher?
- L396: But why not also in months 1-2 when flux is the same amount as 11 and 12?
- L420: No, your results show <u>increasing</u> POC flux does this, not <u>higher</u> POC, still unclear why.
- S1.2: "indicate an initial decrease in particle size with depth" This is not supported by Fig S8. Particle size initially decreases only for EQU and NHS. The others all show an increase in the top 100-150 m. I would remove this line and keep the one below that is a more accurate description of the pattern "occurs within the top ~ 100 m of the water column, followed by a relatively rapid decrease through the upper mesopelagic (< 500 m depth) and stabilisation at greater depths (Fig. S8)."
- S1.2: "Exceptions are observed in the NHHL" The actual maximum is ~100 dbar, but there is a secondary/local maximum at 400-500 dbar.
- S3: "ΛL/S increase with depth in all NONE experiments" Not exactly. ΛL/S has a maximum at intermediate depth with s3 and s4.
- S3: "… the depth of the appearance of reductions in size (akin to s3FRAG+REMIN and s4FRAG+REMIN; Fig. 8c and 8e respectively) shallows" This wording is very confusing. Which panel shows this? Both size and L:S ratio maxima (and local minima just below those) are deeper with increasing slopes.

4. Results need more explanation

- Results only focus on size differences, but particles also vary with respect to excess weight. How did that shape results? What patterns emerged there?
    - L471: Isn't the excess weight essentially a metric of this? There can be two particles of the same size with different excess weights akin to a dense fecal pellet and a light, fluffy aggregate.
- Figure 6b: These results are not discussed fully. Need to discuss the patterns that appear here such as none of these look like the GLOBSINK results. Why does s3 have that huge increase in size then drop at depth? And why doesn't the same thing happen in s4?
- L354-356: Or the starting amount is higher than that tested. Is the starting particle abundance akin to an oligo-, meso-, or eutrophic system?
- L391: "followed by gradual increases" Sometimes there are 2 decreases and 2 increases (months 1, 2, 11, 12), why?
- L393: Why does the direction of flux change and not the flux amount itself make a difference? Never explained.
    - L396-398: Why/how does the sign of change in particle flux alter this amount? I would expect the same amount of flux to have the same amount of PAC, thus contribute the same amount of fragmentation. Does it have something to do with the total concentration of POC? And why does %fragmentation increase in months 4-6 when flux is decreasing and close to its minimum?
    - S3: Fig S15: I still do not understand the asymmetry in these results. The "b" panels here and in the main text are never explained.
- S3: "… suggesting size classes at stripped of mass at shallower depths when initial slope value is higher" But why do months 10-12 have bigger ratios than months 1-3 below 200m, when slopes are the same?
- S3: Figure S14: Missing some important descriptions: Note that Remin looks increasingly like None with steepening slope in terms of average size. Note that in (d & f) all except Frag+Remin look like Kiko.

5. Visualization of results: additions or improvements.

- Figure 2: I would have liked to compare the model results of Fig 3 to the observations more directly, instead of having to go back and forth between figures. Could you please add Fig 2 as a subpanel next to model results?
- L179: I'd also like to see the large:small ratio from observations against model results.
- L318-319 and other references to declining PAC biomass: Never show results for POC concentration. That would be helpful to understand changes in PAC activity with depth.

6. Size spectra slopes

- L72-73: Size spectra slopes should be negative. If you are going to use positive values, then you need to write the equation as: a * size ^ -b to show these are lines (in log space) that decrease with increasing size.
- L242-243: Suggest calculating the PSD slopes of these profiles instead of citing Clements here.
- L422 and throughout: Suggest describing slopes as steeper or flatter, rather than higher or lower, big or small.

Other

- L287-289: "In the upper mesopelagic (~ 200 m), remineralisation and fragmentation follow similar normalised flux profiles (Fig 4a.), suggesting they both attenuate flux at a similar rate." Important result to come back to in Discussion. How one can't tell the processes involved from the flux profiles alone.
  - L426-427: But simply looking at attenuation in the shallower depths did not tell you about the different processes involved. Also important to note.
- L465-466: Makes sense as the particles should become less labile with depth as the more labile parts are consumed or remineralized. Maybe note this or cite a paper that shows this.
- L477: This suggests only pellets produced by DVM zooplankton are important. But couldn't pellets produced by your PAC also change the results?
- L497: And remineralization rates that vary with temperature, oxygen, etc. Suggest adding fecal pellet production to "(DVM, feeding modes, etc.)"
- S3: Last sentence of second paragraph: "… may be a result of sinking POC that is less exposed to these particle transformation processes." Should call out the remineralization process specifically.

<u>Technical corrections</u>

- L45: "Among BCP … should be the start of the next paragraph.
- L55: Why specifically marine snow here and not all POC?
- L88-90: Only says PAC redistribute into smaller sizes, but should mention that they eat some.
- L213-214: "Average ESD" suggests the results from Fig3. I recommend stating as the range, not average.
- L219: Should read "Southern Ocean (SO)"
- L231-234: These sentences belong in the Intro and Methods, not the Results
- L326: PSD slope range should be 2-5 not 2-4.
- L327: missing word: "…PAC biomass to POC concentration <u>ratio…</u>"
- L380-388: This paragraph is Methods, not Results.
- S1.2: Units on figure are dbar, most text uses units of m.
- S2.3: "shallows," used here and elsewhere, is not a word. "shoals" is.
- S3: First sentence of second paragraph should read, "With an initial slope of 2 (Fig. S14a,b), the resulting ESD and ΛL/S vertical profiles resemble the NONE experiment with a PSD slope of 0 (Fig. S13)…"