Referee #1

The authors have made extensive corrections based on the input of the two reviewers, resulting in an improved manuscript. However, I am still confused by the authors' presentation of their hypothesis and the existing literature; the lack of clarity leaves the entire manuscript still very muddled. This is either due to a continued incorrect reading of the literature on the authors' part, or a lack of clarity in the way they have presented it.

In the abstract: If, following from Giering et al., the authors are contending that two very different carbon budget regimes exist in the "upper" and "lower" mesopelagic ocean, this should be explicitly stated before anything else is presented. A great many studies have shown that respiration and other sinks can exceed carbon inputs in the mesopelagic, but there are very few that have shown the opposite… though this is not what the authors seem to be suggesting: "In particular, it has been suggested that organic-carbon supply exceeds respiration by free-living microbes and zooplankton in the upper mesopelagic." If the authors mean that a very different situation exists in the upper mesopelagic than when considering the mesopelagic as a whole, then this contrast to the existing findings should be explicitly highlighted… and then the authors should very logically proceed to put their study in that context. Since this puts the authors "in the weeds" in terms of their study's broader relevance, they should be very clear about the chain of logic. If this is correct, then the authors should define what they mean by the upper and lower mesopelagic.

Review document p. 42, line 17: I am not sure how the consistent standard errors with depth imply a lack of variability with depth in the factors driving C_spec. How does this work mathematically? Perhaps I am just missing the authors' point here, but it seems to me one could obtain the same standard error despite large changes in the relative importance of various factors, so long as the increase in the strength of one was accompanied by a precisely complementary decrease in another.

Figure 9: I am not sure that completely removing the error bars was the right decision. Now, it appears the solubilization term has no uncertainty in it! I realize the authors have explained their intent in the caption, but the figure itself is now misleading. Since the authors are attempting to present real data and the results of a thought experiment in the same figure, this has to be very clear. Perhaps the label "Solubilzation" could be changed to "Solubilzation (hypothetical)" or something similar? Also, in (b) where are the error bars on the new zooplankton respiration data? Or the particle respiration data? The errors in these could be combined using statistical methods and presented somehow. Perhaps the entire panel in (b) needs to be marked off as hypothetical?

Referee #2

***Review (2nd round) of* "Depth-resolved particle associated microbial respiration in the northeast Atlantic" *by* Belcher et al.**

GENERAL COMMENTS AND RECOMMENDATIONS

I am pleased to notice that the authors have addressed most of my general concerns. The revised version of the manuscript now presents conclusions that are consistent with the hypotheses and measurements conducted. In particular, I appreciate that the main objective of closing the carbon budget in the mesopelagic as noted initially has been replaced by solving imbalances in the upper mesopelagic POC budget.
However, some aspects of the manuscript still need to be revised. Especially, the initial design, objective, methods and results of the roller tank experiment included in this work are still highly questionable and I strongly advise the authors either to thoroughly rework this part of the study or simply remove it from the manuscript.
I am confident that the manuscript will deserve publication in Biogeosciences after the revisions detailed below have been done.

SPECIFIC COMMENTS

Note: references made are to the revised manuscript.

Abstract, p. 1, line 19-20: from this sentence it seems that the study is designed to explore an excess of POC supply rather than a missing loss by respiration. The imbalance should be presented the other way around (i.e. the estimated respiration does not balance the observed flux attenuation of POC, suggesting a missing loss).

p. 2, line 7: please add *Steinberg et al.* [2008] to these references.

p. 2, line 9-11: same problem here, please rephrase the other way around.

p. 6, line 24: correct citation is *Logan and Wilkinson* [1990].

p. 6, line 25: again Fractal is not a shape it is a geometry! A spherical particle can have a fractal structure. However a sphere in the Fractal or Euclidean geometries has different structures (but a sphere with a fractal dimension of 3 is equivalent to an Euclidean sphere).

p. 7, line 22: replace *μC individual^{-1} h^{-1}* by *μg C individual^{-1} h^{-1}* if it is what was intended.

p. 8, line 28: please change *"The low R^2 shows that there is variability around this relationship, suggesting some heterogeneity in PA composition"*, to *"The low R^2 suggests that the influence of particle size on the sinking velocity is limited and that particle composition may exert a higher influence".*

ADDITIONAL COMMENTS TO AUTHOR'S RESPONSES

GENERAL COMMENTS AND RECOMMENDATIONS

Note: references made are to the initial manuscript and first round of review

(3) *"However, during the cruise a microbiology team performed parallel MSC deployments devoted to linking aggregate carbon content with microbial abundance."*
How long will it take for these data to be produced? It might be worthy to wait for these and include it in the present manuscript.

SPECIFIC COMMENTS

p. 4, line 25: *"Classifications were done manually by A. Belcher based on particle appearance. The morphologies were distinct allowing confident classification."*
A manual classification should usually be avoided because highly subjective to the operator. You need to detail what criteria were used to decide how to sort the particles. I also find really surprising that natural particles had morphologies distinct enough so that they can be classified so easily by hand. Can you provide some kind of evidence that no mixed shape particles were observed?

p. 6, line 1: *"... we observed particle formation after two days and aggregates increased in size during the incubation period."*
Why then did you write in the initial manuscript p.10, line 21 *"..., PAr POC contents could be reduced to 2 µg C mm^{-3} over 7 days (time incubated after first signs of aggregate formation)"*? The first signs of aggregation were obtained after 2 or 7 days?!
"Additionally, a large number of the aggregates formed in the study of Iversen and Ploug (2010) fall in the range of sinking velocities that we measure on our roller tank formed aggregates (50-150 m d^{-1}), suggesting that aggregation processes are not inhibited at this speed."
Again, there is absolutely no point in comparing the sinking velocities of particles made in different roller tank experiments from different primary particles and measured at different times. The only way to identify a potential effect of tank rotation speed on aggregation kinetic would be by comparing the time of apparition of the first aggregates from two roller tank experiments using the same material incubated at the same concentrations (preferably two identical phytoplankton cultures at the same stage), but using two different rotation speeds.

p. 7 lines 16-17: be careful with the use of "significant relationship" and "significant correlation". The low *p-value* indicates that the result of the statistic test is significant. However, the low R^2 suggests an absence of correlation between PA sinking velocity and ESD. This is probably what you call "variability around the relationship".

p. 10, line 15: *"... many studies still utilize roller tank to collect particles. We therefore thought that we could make additional useful comparisons of respiration rates".*

Certainly not! Roller tank experiment are only designed to form particles artificially, not to collect them. The way you justify why you conducted this roller tank experiment is still not satisfying. Why did you want to compare respiration rates of roller-tank made particles with natural particles? (especially because you noted that these particles were not sampled at the same depth).

"...in an attempt to assess the aggregation potential in the most productive water strata"

How do you assess the 'aggregation potential'? Roller tank experiments are unfortunately useless at such a task, because they cannot be used for quantitative studies [*Jackson*, 1994].

Based on this, I am still not convinced of the interest of including this roller tank experiment in the manuscript and will let the editor decides whether it has to be removed or not.

REFERENCES CITED

Jackson, G. A. (1994), Particle trajectories in a rotating cylinder: implications for aggregation incubations, *Deep-Sea Research Part I: Oceanographic Research Papers*, *41*(3), 429--437, doi:10.1016/0967-0637(94)90089-2.

Logan, B. E., and D. B. Wilkinson (1990), Fractal geometry of marine snow and other biological aggregates, *Limnology and Oceanography*, *35*(1), 130-136, doi:10.4319/lo.1990.35.1.0130

Steinberg, D. K., B. A. V. Mooy, K. O. Buesseler, P. W. Boyd, T. Kobari, and D. M. Karl (2008), Bacterial vs. zooplankton control of sinking particle flux in the ocean's twilight zone, *Limnology and Oceanography*, *53*(4), 1327.