Review of " Estimating Particulate Organic Matter Flux from In-Situ Optics: A Framework for Correcting for Suspended Particles and Incorporating Depth-Dependent Degradation" by Moradi and co-authors.

The authors have resubmitted their analysis of field observations of sinking particle fluxes and its modeling from particle size distribution (PSD) observations from in situ imagery from near the Cape Verde Islands. The paper now introduces three distinct methods for modeling sinking particle fluxes from in situ particle imagery. These are 1) removing suspended particles from the in situ imagery PSD using gel trap PSD determinations so that only sinking particles are considered in estimating fluxes, 2) accounting for changes in carbon content in sinking particles as function of depth/time and 3) considering changes in nitrogen to carbon ratio vs. depth due to the preferential remineralization of PON. The method results in assessments of the sinking particle PSD separately from suspended and POC and PON fluxes. The resubmitted paper is much improved, both in its focus and execution. This is greatly appreciated. I also appreciate their inclusion of uncertainty analyses in this work. It was missing before. However, there remain issues that need to be resolved. Detailed presentation of my larger issues are followed by some specific comments.

General Issues.

As I understand it, Method 1 attempts to correct for the non-sinking particles seen by the UVP before sinking fluxes are calculated by removing the non-sinking fraction from the UVP PSD. Paired gel trap and UVP PSD are used to fit log-log relationships vs diameter (D) for each member of the ensemble of paired observations available. The UVP PSD is then mapped onto the gel trap PSD using by minimizing the square errors of the areas of a trapezoid under each PSD vs. D curve summed over the ensemble. I see no physical rationale of why this is done using the areas of the trapezoid. I think that this would bias the corrections towards focusing on the smaller D values (which is counter to what one thinks about suspended and sinking PSD slopes). The authors must provide some rationale of why this is the right way to do it. Why can't the slopes and offset factors be simply modeled from each other using a linear regression approach? It seems to me that there may be an environmental parameter or two, like the UVP PSD slope or total particle volume, that can be used to better constrain the correction. Was this attempted? One of my concerns is that the details of the method presented may be specific to the particular characteristics of the data set and environmental characteristics and may be difficult to replicate in other settings.

Once the corrections (maybe more correctly termed "calibrations") are made, then the UVP data can be used to estimate fields of sinking particle PSD. This would be a very useful thing to know. It is a shame that the authors do not take advantage of this and address what fraction of the total particle pool is sinking particles in section 3.1.2 (lines 429-435). The comparison of the coefficients presented is not very intuitive. This seems like a missed opportunity.

The second method uses measurements of particle O2 consumption rates to address the loss of sinking particle carbon content vs. depth. They show that they retrieve similar values for POC fluxes whether they model the carbon losses as a zeroth or first order terms. Again, zeroth order kinetics implies that the rate of carbon loss is independent of the mass of carbon present, which simply is not physical. Given the uncertainty in all of the data used to model the POC fluxes, the differences between the two methods are small. That said, I would drop the zeroth order case as it adds unnecessary complexity to the paper (and is fundamentally flawed). Regardless, they adjust constants to make the method "work" using trap measured POC fluxes.

The third method models PON losses relative to POC as function of particle age (depth / sinking speed) using a hyperbolic function. This makes sense to me. It is logically calibrated using the trap PON fluxes. No issue here, assuming that the POC loss rates make sense.

The uncertainty calculations are appreciated as mentioned before. However, given the large uncertainties in the inputs (sinking speed spectra [fig 3a], particle O2 respiration rates [fig 3d], results of the UVP to gel trap PSD correction [fig 4gh], etc.), the error bars for the flux profiles are tiny (fig 6). Further the mismatches with the flux and C/N ratio profiles (fig 6) lie outside of the 95% confidence intervals more than 5% of the time. Something does not seem right here.

Another issue with the paper is that it seems that the methods presented here were created for a particular experimental setting and the various parameters estimated would not necessarily work for another setting. This means that the many measurements - bulk and gel sediment trap fluxes, in situ particle profiling, analysis of gel trap imagery, particle-specific remineralization rate determinations, etc. - need to made again for another setting and the various coefficients and uncertainty bounds need to be recalculated. This would make it extremely difficult to recreate for another setting, especially by another group of investigators. Clearly, we are a long way from applying these methods on without the detailed experimental work. But it seems that a discussion of the paths forward to achieve this would make some sense. What are the most important things to estimate, how best to automate some of this, etc., etc.? As it stands, the methods introduced would be hard to adopt widely.

A few additional comments follow.

The abstract needs to state that the methods introduced improve the estimates of sinking particle fluxes from the experimentally optimized conventional method. You do not want to bury the lead...

No link or reference is given for the Gel-PISA gel analysis software that is used to determine the PSD of sinking particles. The authors need to provide a link to a github repository (or similar) for the software so the phyton scripts are available. That seems important enable any open science to happen with this approach.

Figure 7 compares flux calculations made with two different profiles with differing amounts of large particles in the mesopelagic. It is super hard to see the differences in the particle sizes from the number density spectra profiles shown and some better metric is needed. Even a profile of the abundance of large particles greater than some size would make this argument easier to follow.