Paper Review – Forest et al. BG

This paper combines an array of measurements (ship-based, moorings and underwater profiler, and remote sensing) together with elaborate computational, statistical and modeling approaches to “assess the variability and forcing factors of vertical fluxes of particulate organic carbon on the Mackenzie shelf” (in 2009). The paper represents an extensive integrative effort to amalgamate such a diverse range of measurements and techniques and should be commanded as such. Integration of multidisciplinary data sets using multivariate statistic& models is in line with the recent work of first author A. Forest, with recent papers dealing with zooplankton grazing/carbon cycling in the Beaufort Sea (e.g. Forest et al. 2011; Forest et al. 2012).

The extensive data treatment in the current paper, using multiple data sources (e.g. annual hydrological time series, annual time series of particle sinking fluxes and short-term traps, remote sensing estimates of ice area, wind maps, underwater profiler for sinking flux estimates) and different resolution and/or approaches, makes it particularly challenging for critical review. Being a convinced advocate for the need for data integration, I strongly support the use of multidisciplinary tools to help provide insights into complex and often interrelated bio-physical processes that impact the biogeochemical cycling of carbon in the Arctic (and the global Ocean). That said, while fancy computational treatment of data may at times come at the price of losing track of the original datasets, it should never override the validity and reliability of the data. Simply put, the best modeling exercise (and following interpretations) can only be as good as its data feed. Unfortunately, this is one of the fundamental caveats of the paper. The objective here is to assess bio-physical controls on particle sinking export, therefore the variable “sinking export” is key to the overall analysis and interpretative approach. I have serious concerns with respect to the sinking flux estimates and derived estimates from the profiler and which form the core of the paper. These fundamental aspects, explained in more details below, need to be addressed in order to validate the approaches used here and solidify interpretations.

- The authors use a combination of flux measurements from short-term and long-term deployments to estimate parameters for an algorithm to derive sinking fluxes (carbon and dry weight) from underwater video profiles of particle distributions (UVP) (see Figure 8). These UVP-derived flux estimates, as well as the equation parameters, are further used for graphic/statistical analysis and interpretations. In recent years, there has been quite a few studies reporting sinking flux measurements with short-term (Juul-Pedersen et al. 2010, Sallon et al. 2011) and long-term sediment traps (O’Brien et al. 2006, Forest et al. 2008) in the same area. These studies are known to the authors and cited in the text. Yet, the authors do not use this knowledge of sinking fluxes in their study area to assess (compare) their own estimates. Focusing on short-term sediment traps only, the 50 m fluxes obtained by Juul-Pedersen et al. (2010) at 21 stations in the study area between June and October (2002-2004) range between 14.8 to 258.4 mg C m-2 d-1 (their Table 2). The 100 m fluxes measured by Sallon et al. (2010) at 12 stations in the study area in June/July 2008 range between 38.3 to 257 mg C m-2d-1 (their Table 2). In the present study, the authors report extremely low sinking fluxes of POC based on their short-term sediment trap deployments, < 15 mg m-2 d-1. These fluxes are surprising low and somewhat dubious considering results from other flux studies with short-term drifting sediment traps (calibrated with 232Th) in the same study area and other Arctic regions (e.g. very low POC sinking fluxes, ranging from 37.7 to 77 mg C m-2 d-1 are reported for Hudson Bay, Lapoussière et al. 2009).

- I advise for caution when/if using the results from the long-term sediment traps deployed as short-term drifting sediment traps. This has important implications for the analyses of the UVP5-derived fluxes vs the measured fluxes presented in Fig. 8. Low (< 15 mg C m-2 d-1) sinking fluxes in Fig. 8 are derived from the short-term deployments of long-term traps. As mentioned above, these fluxes are extremely low and should be used/interpreted with caution until the efficiency of the long-term traps in short-term deployments, according to the method used here, is demonstrated. The question to the authors then is does a significant relationship between the UVP5 and measured fluxes still exists if, as a cautionary measure, one removes data points < 15 mg C m-2 d-1?

This issue has great implications for analyses and interpretations in the paper (also see below), e.g. the result showing that estimated sinking velocities vs particle size the Beaufort Sea (this study) are at odd with other world regions could simply be related to the low flux estimates here, which drive the regression – parameterization between the UPV and fluxes, and further derivation of sinking velocities (Fig. 11).

- The very low fluxes estimated with the short-term sediment traps raise concerns with respect to the methodology. There are no details on the deployment method (free-drifting, bottom anchored) in the Mat. and Method Section. Are the short-term moorings bottom-anchored (similar to long-term moorings)? Based on the trap line description, i.e. 4 sediment traps + instruments, with the traps weighing 40 kg each (17 kg in water, info not in ms), one would assume that the line is anchored to the bottom – especially considering the sub-surface floats requirements to achieve neutral buoyancy and the depth of the shallowest trap (ca. 50 m). There is extensive literature on hydrodynamic constrains on sediment traps design/deployment and the particular case of drifting sediment traps. The authors are certainly aware of published and recommended methods for drifting traps with drifting lines, either surface-tethered or neutrally-buoyant, hydrodynamic considerations, ways to minimize potential overtrapping/undertrapping, as well as other important technical considerations such as filling the complete trap with a seawater solution of higher density than the surrounding water (the use of added brine has been discussed & challenged in recent literature). These methodologies are quite different than long-term moorings where the trap line is bottom-anchored and the trap cups are filled with a brine solution to which a preservative is added (avoiding degradation of material over the long-term). The manuscript ought to present in better details the short-term trap deployments and demonstrate that the methodology used here (using long-term traps, most likely bottom-anchored, with the main trap component not filled with filtered seawater or a brine solution) provided reliable estimates of sinking fluxes, especially given that the results obtained differ widely from studies using drifting traps in this and other Arctic regions.

- The extremely low short-term sinking flux results could also explain the very low A and b values obtained for the parameterization of the UVP-derived fluxes in this study compared to other studies (see lines 443-444). The authors note their much lower A and b scaling factors, with b twice lower, than in other studies. This also implies that the derived fractal dimension is much lower than in other studies. The authors draw an extensive interpretation from these results (lines 724 +), discussing the role of EPS in sinking fluxes, citing the study of Sallon et al. (2011). However, the authors fails to discuss actual results on flux magnitude and composition presented in Sallon et al. (2011) and which are most relevant for comparative analysis with the current study, including POC, EPS, fecal pellet fluxes, fecal pellet size-class distribution, etc. (applicable to this and other parts of the ms). These (and other fluxes) should be included in an in-depth review and analysis of the sinking fluxes obtained here and their impact on the estimates of A, b and other derived estimates. As a first step to constrain second and third-level interpretations based on UVP-derived fluxes, one needs to take a solid aim at validating them.

- A similar comment as above applies to other derived results/interpretations in the ms. It is somewhat disconcerting that the authors may have missed/ignored the red flags associated with their short-term flux data set (e.g. extremely low fluxes, extremely low derived parameter b and derived fractal dimension, uncommon settling speeds & trend, Fig. 11), in favor of their use to derive UVP flux estimates, and then to further derive 3-D maps (Fig. 9), 3-D visualizations of fluxes (see lines 499 +), and numerous other interpretations (e.g. see point above, multidimensional analyses – Figs. 14-16 ). While the exercise is technically and visually attractive, again the question of its validity undermines the scientific output and interpretation. On a few occasions, the authors indicate that their results may require to be taken with caution, e.g. "... the abrupt transition from a high-to-low POC flux regime was linked to a real shift in the particle abundance from Cape Bathurst to Banks Island – and not to an artifact of the visualization." (lines 515-517), "If true, the amazingly high vertical fluxes..." lines 785-786. Unfortunately, these brief statements hint that the authors are aware of their unusual results (we expect no less from the experienced list of authors) but opted for the "high-tech" road, with intricate analyses and fancy visual representations. Unfortunately, this approach ends up undermining the exercise and the derived interpretations and conclusions.

Other points of importance:

- The justification for a log-log relationship rather than a linear relationship for estimates of the same variable (sinking flux, Fig. 8) needs to be clarified. This is a classic approach for remote sensing estimates of chl a in the ocean since they range over a few orders of magnitude. However, for POC (and mass) sinking flux, the range of variation is much narrower, ranging typically from ca. 100 mg C m-2 d-1 or a bit less to < 1000 mg C m-2 d-1.

- Lines 774 + : “High fluxes (> 50 mg C m-2 d-1).” These are not high fluxes – In particular for this area, the authors should compare their flux values with those from other studies in the area. They could also compare with other regions where high fluxes are often observed, i.e. Barents Sea. At the other end of the spectrum, the extremely high POC fluxes of 1000 – 5000 mg C m-2 d-1 should definitely be put into perspective (and perhaps challenged?). The authors hint to the questionability of these fluxes on lines 785-786 “If true, the amazingly high vertical fluxes...”; yet, they avoid any comparison with previous fluxes in the area or in other coastal areas – which are again, necessary to validate these results.

- Fig. 10 Example of extremely low (and improbable POC fluxes). The vertical flux of POC is < 30 mg m-2 d-1 (panel c) and between 0.5 and 5.5. in panel d)!

- Table A1 is unecessary. This table is appropriate for a database (e.g. Polar Data Catalogue) rather than a paper.

- Fig. 12 Are the structures well defined or a result of the interpolation of points? Please indicate points on this figure. Also, zooplankton panel; the authors mention that they are confident that their zooplankton biomass estimates represent the zooplankton biomass “since large zooplankton represent the bulk of zooplankton in this region” – However, the small zooplankton could represent a significant component. It would therefore be appropriate to indicate “Large zooplankton biomass” and indicate the size class in the figure/ figure legend. Also appropriate to indicate that the zooplankton biomass is derived from the UPV5.

- Did the authors attempt a correlation between their zooplankton biomass estimate and actual in situ estimates. Some of the authors are zooplankton specialists and one would assume these data are available.

- Lines 812 +. The authors cite other sediment traps studies in the same area (but do not compare their sinking flux results with these studies). Their conclusion is that “none of these studies had a multi-parameter dataset with a spatial resolution fine enough to proceed to a state-of-the-art” variation partitioning analysis of vertical flux predictors, corollaries and spatial patterns.” I would argue that the current study, while having the “multi-parameter dataset” that could help address fine-scale resolution of vertical fluxes, fails to do so because due to the lack of validation of the data that are at the core of the analysis. This validation, in the form of comparisons with other datasets in the same region –especially short-term drifting traps recent studies, critical analysis of the methods employed, and analysis of error transmission during iterative mathematical procedures, is essential to support interpretations & conclusions.

- Lines 958-959 “From a pan-Arctic perspective, the southeast Beaufort Sea might not appear to be the most interesting system to study with respect to primary productivity (e.g. Ardyna et al., 2012). “interesting” really is a matter of perspective. I surmise that the authors mean that the region is of low productivity?