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Interactive comment on "Carbon-based phytoplankton size classes retrieved via ocean color estimates of the particle size distribution" *by* T. S. Kostadinov et al.

Anonymous Referee #1

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This is a very long paper (39 pages plus 11 figures, not counting references) that loses the reader in a complicated description of results based on relatively simple algorithms and a mountain of assumptions.

P 575 L8 – this statement shows old references; is this still the present view – or does this need to be qualified strongly with a specific timeframe and types of processes?

L23 – it is not just climate that can affect these patterns – they change at much shorter time scales as well, and also spatially for a number of reasons

P 576 L 6 – define green models/reference

P 578 L25 – is relaxing the best way to describe this, or "departing from"? "improving on"?

P578-579 and P 586 L 4 – how are uncertainties estimated if the time-element is not really included, specifically, the authors use monthly SeaWiFS data to compute the size-fractionated organic carbon content. Is this a linear quantity, i.e. do you get the same result if this is computed 'daily' and average to monthly fields?

P 580 L 19-20: This is confusing. Eq (3) provides cellular carbon content. Yet they then multiply by 1/3 to obtain "living phytoplankton" C. Aren't 'cells' living? I would not call them living if they were detritus. . .?? It is also unclear why the authors first claim that the method they use is better than previous methods because it is not tied to a constant CHl:C ratio. Yet they introduce another constant, of 1/3, as in: "The carbon biomass of living phytoplankton only (C, [mgm-3]) can then be estimated by multiplication by 1/3".

It seems that the authors could have reached similar results simply applying an estimated carbon per cell estimate to the estimates of concentration of cells they published earlier (Kostadinov et al 2010)?

P 586 – why were data downsampled to 1 degree, if the original images are 9 km pixels? I didn't understand the need for this – clearly this eliminates substantial pixel noise the authors may have had.

P 589 L 15 – aren't all these empirical algorithms, including the CHL algorithm, designed to match field observations? I don't understand why this limits the applicability of a dataset to be compared with the author's new results. It would seem that there is value in comparing the estimates of POC or phytoplankton C to these field observations as a validation step

L17-20 – how well are the size classes themselves validated globally, if you can't validate something simpler like POC? The patterns derived from the satellite data are described as truth and justified as conforming to common sense oceanography – but not validated against ground measurements.

Perhaps I missed some key information - I was confused on how the authors compared the satellite derived estimates to ship data, such as AMT. Did they use the monthly fields to compare to a particular AMT observation (a point on a particular date)? The regressions (done in log –log scale; Fig 8) are not very impressive. The eye would almost say there is no relationship. The simplistic validation done here against data from one cruise is not sufficient to conclude that (P 612: We demonstrate that satisfactory in-situ closure is observed between PSD and POC measurements)

The uncertainty analysis conclusions point to problems with assumptions about the index of refraction used in Mie scattering modeling – but this discussion is not really included in the paper.

In any event, this is a long paper that ends up tiring the reader. Perhaps the authors can extract the essence and submit a more digestible version.

Interactive comment on Ocean Sci. Discuss., 12, 573, 2015.