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Discussions

Interactive comment on "Volume distribution for particles between 3.5 to 2000 μm in the upper 200 m region of the South Pacific Gyre" *by* L. Stemmann et al.

Anonymous Referee #2

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General Comments: This paper presents some very nice and interesting data on the size distribution of particles at two contrasting stations in the South Pacific. There are two main results in this paper: 1) particle number spectra in the South Pacific, and 2) the small but not the large particle population exhibit diel behaviour. More of this type of high time and depth resolution data is what is needed to move forward with understanding the dynamics of the highly variable particle field, and this paper represents a nice contribution to the field. The diel result stands on its own and has very interesting implications for understanding particle dynamics. The interpretation of the number spectra results and subsequent conclusions need some more attention before publication, however, particularly in how the number spectra are transformed to volume/mass/POC spectra/concentration profiles. The results of these transformations hinge critically on the fractal dimension used, and not enough justification is given for the values they chose.

Specific Comments: Discussion Section 4.3: the sensitivity of the mass/volume spectra to the value of the fractal dimension (eg. Fig 9) highlights the limitation of the particle-counting approach to biogeochemistry. Unless we know how to transform particle number spectra into mass or volume units, the usefulness of knowing particle size spectra is lessened. It is therefore of utmost importance to constrain the fractal dimension as well as possible. Using POC measured from Niskin bottles is a good start for calibrating HIAC calculations. (In fact, the flow of the logic in section 4.3 could be improved; instead of starting with the assumption that HIAC measures a conserved diameter, pose it as a question, and use the POC data to help constrain it). What other geochemical data are available from the cruise that could be used as an independent cross-check on particle mass calculated from the number spectra? It would be even better if measured dry weight data are available, since the dry weight to POC conversion is very variable (the authors use 20%, but their cited reference–Alldredge 1998–finds that this number ranges from 20-40% in the Santa Barbara channel, and I expect it could be very different in an ultra oligotrophic environment). The methods section stated that an in-situ pump was deployed. These can sample hundreds to thousands of liters, certainly adequate to collect enough large aggregates for POC and dry weight analysis. Is there anything available that could help constrain the fractal dimension for the UVP particles? Rather than derive a fractal dimension themselves, the authors use Dˆ3=2.3 from Guidi et al, in review, which was determined on the same cruise. However, even using Dˆ3=2.3 seems to greatly overestimate POC (5-10 fold, by my reading) from HIAC compared to the GF/F filtrations from Niskins (Fig 10). In fact, the predicted POC should probably be higher since it is possible that POC accounts for a larger fraction of dry weight in these oligotrophic regions. Doesn’t this imply that the fractal dimension for HIAC-sized particles should be even lower than 2.3? A more thorough justification of using Dˆ3=2.3 is certainly needed, particularly in light of

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the discrepancy between the HIAC calculations of POC and GF/F POC.

p. 3392, lines 16-29 and Conclusions: given that the HIAC-calculated POC using Dˆ3=2.3 is so different than the GF/F POC, and the UVP-calculated POC are uncalibrated, I am not convinced of the validity of using Dˆ3=2.3 across the board to compare the disproportion of mass between the two particle pools, and am particularly not convinced by statements in the conclusion that hinge on this, such as "we show that the number spectra can provide realistic estimates of particle mass...", and "the mass of large particles can equal the mass in the smaller particles...". This type of statement would be better supported by actual geochemical measurements of size-fractionated particles, something that is easy enough to do using in-situ pumps and doesn't require the time and depth resolution of the techniques used here. A more appropriate conclusion would be to dwell on the statements the authors already make (Conclusions, lines 8-11), that knowing the geometric properties of particles is crucial in order to take advantage of these types of data, and further discuss the range in the particle mass concentrations as a result of different geometric properties.

Introduction: The overall impact of this paper could be increased by putting the number spectra data into better context, particularly in the introduction. As biogeoscientists, we care about the particle number spectrum because it provides insight into particle dynamics and thus into carbon transport to depth, not because we might be able to calculate the mass of the Loch Ness monster. Plenty of other papers make these links (many referenced and/or even written by some of the authors, plus those by Alldredge et al, among others), but the introduction as written makes it feel like a mathematical curiosity rather than convincing us that it's an important parameter for understanding the marine system. The promise of in-situ camera data is to allow sampling at time and depth resolutions that are not possible for discrete geochemical analyses. Further, constraining the particle number spectra would allow the extrapolation of results to size ranges that are usually not easily sampled. These are points that need to be made explicit in the introduction.

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Section 2.3: the fractal dimension ends up being a very sensitive parameter for transforming from number spectra to mass/volume spectra (cf. figures 8,9). It is worth putting in an equation showing how it is used in calculations (how d_c, d_f, and Dˆ3 are related).

p. 3387, line 18: It's interesting that the DCM does not coincide with the HIAC particle max. This (the fact that the DCM is deeper than the Prochlorococcus/HIAC peak at 100m) would be a good point to clarify, otherwise the logic in lines 20-24 is confusing.

p. 3388, lines 11-12, 15: how were the aggregate and mesozooplankton observations made?

p. 3388, last paragraph: the unidentified objects are worth reporting, particularly since this is thought to explain the shallower slope for the number spectra at MAR, but the discussion could be shortened considerably as it fits into a curiosity category more than anything else.

Technical Corrections: p. 3389, line 17: "steeper"

p. 3391, line 5: comma after D3=2.3; "compared to"

Figure 8 is never referred to in the text! I think it belongs with p. 3391, line 19. The legend has a different variable convention than the figure caption.

Interactive comment on Biogeosciences Discuss., 4, 3377, 2007.