Biogeosciences Discuss., 4, S1791–S1795, 2007
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Biogeosciences 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.

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Received and published: 3 November 2007

Review of: Volume distribution for particles between 3.5 to 2000 956;m in the upper 200m regionof the South Pacific Gyre, by L. Stemmann, D. Eloire, A. Sciandra, G. A. Jackson, L. Guidi, M. Picheral, and G. Gorsky.

Reviewer: Emmanuel Boss, University of Maine.

This paper provides a description and analysis of size distribution measured by two instruments for data collected during the BIOSOPE cruise. Results show that the volume of large particles can equal the volume of the smaller particles. The proportion of material in large particle changed across trophic conditions and as function of depth. Large

and small particles seem decoupled with the small fraction exhibiting diel periodicity not observed with the large particles.

The paper is well written and its scope fits well within BG.

I have some comments that I feel need to be addressed before publication in BG:

1. The authors mention Coulter data collected in the cruise (p. 3389). Why wasn't it added to the analysis (at the least to provide uncertainties across methods).

2. Besides the descriptive aspect of the findings one would like to see the findings put in the context of previous studies. E.g. is it novel that large particles volume can equal that of small particles?

3. 'Until recently, only the retrieval and analysis of the smallest size fraction (d<100 um) was feasible'; Is that true? Papers by Jackson, Hill and others show much wider size distributions (using cameras, LISST etc'). If this statement is meant to be specific to surface open ocean regions it may be correct.

4. The principle of operation of the HIAC is not clear. Is it the shadow area that is measured on a screen? Please provide more details or reference a paper that does. In addition describe the calibration procedure for size (e.g. beads of known sizes) and concentration (e.g. beads of known concentration). Also, please describe the bias you would expect for non-spherical particles and porous (not opaque aggregates).

5. You chose d1=5um without any justification. This assumes all the particles in your ensemble were created from a single primary particle. Is there any reason whatsoever to believe it to be the case? It is OK to use it as a simplifying assumption (as we do not know how to deal with it otherwise) but that has to be stated.

6. The fractal dimensions of aggregates are known to vary with size, e.g. Khalifa and Hill, 2006, Maggi, 2007 and references within. How do you justify using a single one (2.3 or 3) to describe the whole PSD.

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7. It is claimed that 'Both the HIAC and UVP techniques undersample particles at the lower end of their size ranges, producing anomalous peaks in size spectra' - please provide a reference or an explanation why it is so. How do you know at what bins it becomes good?

8. How did you fit the model power-law model to the PSD data? Did you fit the log-log data with a straight line using standard regressions, effectively assuming that the uncertainties are smaller at larger sizes (which is clearly not true) or did you actually used the uncertainty as weights when performing a non-linear fit? Any fitting chosen has to be justified as it does affect the obtained k. Given the importance of the interpretation of this parameter in the paper it is very important that the regression be done very carefully taking the uncertainties into account. In addition some description of how well the power-law fits the data is warranted. Even better, uncertainties in k (e.g. using bootstrap or replicate samples) will allow one to evaluate when PSDs are significantly different in their shape.

9. It may be useful to display sample data of the mysterious unknown particles described in the bottom of 3388.

10. 'These diel variations were not observed in particles detected by the UVP. The fact that this pattern of variability was not observed for the pool of large particles suggests that the dynamics of small and large particles are disconnected at the diel scale';. – I have doubts in this explanation. Grazing and growth are tightly coupled in the oceans. It is hard to explain why the two populations are likely to be decoupled on those time scales. A possible interpretation may be that while growth and division are light synchronized grazing, sinking and aggregation occur at all times at relatively fixed rates (except for diel migration of large zooplankton).

11. P. 3390 'For example, aperture impedance particle counters, such as the Coulter Counter, measure a particle property that corresponds approximately to the volume of the solid mass composing a porous particle, while imaging instruments frequently

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measure the cross sectional area of the porous aggregate'; - Handling of particles also cause changes in properties (e.g. disaggregation) which is less likely to be occurring in-situ.

12. P. 3390 'Multiple measurements made on different phytoplankton cultures with the HIAC showed very good agreement between the biovolume calculated assuming that the algae are spheres with the reported diameters and the particulate carbon concentration measured separately, suggesting that the light blockage is proportional to the mass content of the algae. Therefore, we consider the particle diameter reported by the HIAC to be an estimate of the conserved diameter, dc.'

a. light blockage is a non-exact description of the physical principle used by the HIAC to obtain information on size. Could you provide a more accurate description? b. Why do you generalize from the cultures to the field? Is all the carbon in the field from phytoplankton?

13. P. 3390 'The total mass of the HIAC particles should be similar to the total mass obtained from GF/F filtration but not the UVP particles because the sampling volume for the GFF filtration was too low to sample the large particles adequately.' how is the between large and not-so-large calculated?

14. P. 3393 'The particle size distribution from few 956;m to few mm cannot always be fitted with a unique power relationship' – isn't this consistent with many previous studies, e.g. the works of Kitchen, McCave and others?

15. I suggest to explore the sensitivity of your conclusion to the value of d1 as well as the fractal dimension model adopted.

Minor comments: 1. Diffusion is used where the appropriate English term is scattering. This risk to confuse many of your readers who are not French speaking (e.g. p. 3382). Similarly, please use 'size-bins' or 'bins'; rather than 'sections' (e.g. p. 3384);.

2. Equ. 1 will be simpler to understand if units were consistent, e.g.

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n=$n_0(d/d_0)^k where n_0 is the number concentration of particles of size d_0$.

3. $d_d in P.3382 is the 'bin size' (a better description than diameter increment)$.

4. The relation between dc and df has been addressed in earlier works than Jackson (1990) in oceanography, e.g. McCave (1984).

5. 'The value of 3 is the upper limit and occurs when porosity is constant with diameter.' (p. 3383) - this value occurs only when particles are solid, e.g porosity=0.

6. Please provide equ. (3) also in its discrete form as you applied it to your data, with a minimum and maximum diameter etc'.

7. The overlap between the two methods seem not existence as that data from the HIAC was deemed too noisy yet the overlap is discussed (p. 3386, Fig. 3).

8. P. 3388 - please provide the source of 'personal data';. E.g. if this data is based on nets, can avoidance be a factor? Please provide the methodology used to assess the 'personal data'.

9. P. 3388 - The difference between the two methods is 1000-32000 and 5500 cells/L, a factor of 6 in either direction or quite similar. Is this potential difference significant? What does it tell us about the uncertainties in the methods?

I am often wrong. If you feel that I am off the mark in some or all of my comments or simply need clarification feel free to contact me and I will be happy to change them.

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