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Biogeosciences Discussions

Interactive comment on "Direct contribution of phytoplankton-sized particles to optical backscattering in the open ocean" *by* G. Dall'Olmo et al.

M. Twardowski (Referee)

mtwardo@wetlabs2.com

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General Comments

This is the first published work I am aware of that experimentally assesses the contribution of backscattering in different size fractions for the open ocean. Since the particulate sources of backscattering in the ocean are poorly known (Stramski et al. 2004), this is an interesting and valuable study. The most significant conclusion in this respect was probably that the contribution of the colloidal fraction (<0.2 um) to particulate backscattering was very small. Interestingly, the submicron fraction still accounted for ~40% and ~100% of the bulk backscattering in the mesotrophic and olig-

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Interactive Discussion

Discussion Paper

otrophic ocean, respectively, showing the importance of the 0.2 to 1.0 um subfraction, especially in oligotrophic waters.

This is a well-written, thorough paper built around an apparently high quality data set. This paper should be published. The component that gave me some concern was the modeling based on Mie theory. I questioned many aspects:

1) Why were fitted Junge-distributions used in the simulations instead of the directly measured PSDs? Fitting a power law will remove noise, but it will also remove any fine structure in the distributions; there are potentially better ways to remove noise while retaining PSD structure. For the fractionated PSD data plotted in Fig. 10, some of those distributions will not be described well by a single slope. For the purposes of extrapolation into size regions where there is no data, I can understand the fit, you do not have a choice, but it is better to use measured data where available.

2) Why was the imaginary n set at zero? A more reasonable value would have been 0.001 or at least 0.0005. If the real refractive index is nonzero, i.e., the particle scatters light, then its imaginary n must also not be zero (e.g. Bohren and Huffman 1983). The difference in Mie results from varying imaginary n at different small values usually has little impact, but the difference in results when setting the imaginary n at zero versus a small value can be significant in some cases. Even a small imag n can dampen refractive oscillations in Mie phase functions so they are more characteristic of phase functions for more realistic particles.

3) Why was Dmax varied? Dmax should be set at a sufficiently high value to include the effective particle size range sampled (at least 100um). The argument could be made that a Dmax is chosen based on a low frequency threshold in measured abundance at that particular D, but then setting Dmax at any higher D will provide the same result anyway because of the negligible influence of the larger particles. Setting Dmax at these low values (e.g., 6um) seems justified only if multiple filters were used in series to provide effective Dmax cutoffs. I see no justification for artificially truncating

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a measured PSD, effectively dismissing particles that will potentially contribute to the bulk optical properties.

4) How were n and Dmax iteratively solved so that the results matched a measured cp value? There is clearly no 1 solution here and the results will be very sensitive to both. Something was assumed.

5) Why were the n and Dmax results from matching a cp value then used for bbp? Why not proceed with bbp independently in the same manner as with cp (although not sure what that really was)? These optical properties have different sensitivities to these input variables, e.g., bbp is more sensitive to n than cp. Why not vary n and Dmax until the bbp/cp value matches the measurement, i.e., use all the information you have? Incidently, looking at Mobley et al. 2002, a Junge slope of 3.5 intersects the Mobley et al. dashed regression in Fig. 2 right at a bbp/bp of 0.01 - very close to the values measured here - and corresponds to a bulk refractive index of 1.1. The Mobley et al. algorithm is based on the Fournier-Forand phase function model and has no inherent specificities to particle shape. Note this bulk refractive index is close to what you would expect from Mie theory if bbp was addressed independently of the constraints developed from the cp fitting. So which is correct? There is an underlying problem in the current approach in that there is an implicit assumption that Mie theory applied to a natural nonspherical population should without question work for cp from first principles. While there may be differing shades of gray between Mie theory's application to cp vs bbp, this is clearly not a given. From personal experience, which I know is shared by Emmanuel from our correspondences, testing closure between measured PSDs and measured cp usually does not add up.

6) Finally, I questioned the overall purpose of the Mie theory analyses. It does not seem to add much to the central conclusions and may create a confusing diversion; at least it did for me. One expressed purpose was essentially to test the efficacy of using Mie theory to obtain bbp from first principles, but can you really do that with much certitude with a size distribution of limited size constraints, while guestimating n

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distributions, imag n distributions, Dmax, and having to assume Mie theory is reliable for cp for natural particle populations? I question whether this data set could be used well for this purpose.

So I would suggest leaving the Mie modeling out. I can see some modeling being retained if the approach was changed and the purpose was clear. Putting aside the Mie modeling for a moment, the empirical data set is wonderful and clearly shows conscientious attention to planning, detail and accuracy. These measurements are not easy to make. The authors are to be commended for such fine work.

Specific Comments

Title: I do not think "phytoplankton-sized particles" is especially meaningful. I understand there is a strong underlying desire here to link bbp to phytoplankton biomass, but neither phytoplankton generally nor their biomass specifically were characterized in any way that I can see except their chlorophyll content.

Well written Introduction.

I like the use of the dye in assessing bb_wall.

p. 300: For the most correct beta water values, now see:

Xiaodong Zhang and Lianbo Hu, "Estimating scattering of pure water from density fluctuation of the refractive index," Opt. Express 17, 1671-1678 (2009).

The Morel (1968) derived correction of 1+S/37*0.3 for salts still needs to then be applied.

Verification of the WET Labs bead calibrations after the cruise to both verify their values and assess any drift is important in these relatively clear waters.

p. 304: This is somewhat tangential, but regarding chi_p factors, I can say from recent work in our lab that the Boss and Pegau value and Berthon et al.'s value around 1.1 looks accurate at 117 deg. Sullivan et al.'s value looks accurate as well - so let me

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explain. The issue is that the ECO sensors have an angular weighting function that is broader than just 117; in fact I recalculated these weighting functions for the ECO recently (Ron Z computed the original ones) and found much (much) broader functions than the ones we have been using for calibrations. Weighting a proper chi_p function in the backward direction with the more correct ECO weighting brings the chi_p value at 117 down by about 10% on average, maybe a little more. We only know this now because of the more complete VSF measurements from the MASCOT sensor. This is why Sullivan et al. found a value of 0.90 for the ECO, but that value was also affected somewhat (a few percent) by the estimates of bbp from the 3-angle ECOVSF that we were calling reality at the time. Bringing down your values ˜10% will not affect your results much; in fact all the relative bbp results will be the same. I have no problem with the values being left as they are, as we are still working on the issue and getting a paper ready for submission (if interested, we can send a preliminary draft when ready). But it may be worth commenting that although there may be upwards of 10% bias uncertainty in the bbp estimates from an ECO at this time (which is a number used in several previous publications to describe the estimated level of uncertainty in bb measurements), the key sources of bias error cancel in any ratiometric analysis of the subfraction data, so that these results in particular should have much better accuracies.

p. 310, 3 lines from bottom: should 1.5um be 2.5um? Also, note in this discussion that many soft biological particles squeeze through filter pad pore sizes smaller than their ESD. This is common.

p. 314: While the fractionation results from mesotrophic stations show that 40-50% of bulk backscattering on average was found in the $<$3um fraction, why not also discuss the results from the oligotrophic fractionation experiments 2 and 3? These experiments seem to show that nearly 100% of the bulk backscattering was found in the $<$1um fraction as well as obviously the subfractions from larger pore sized filters. Since the open ocean is dominated by oligotrophic conditions and the expressed interest here is in developing a global understanding of the sources of oceanic backscattering, I would expect these experiments to be discussed in more detail, especially considering the comparison to Stramski and Kiefer's Mie results, which I believe were specifically intended for only the oligotrophic ocean. It is stated here that the fractionation results show more backscattering in larger particle size classes than predicted from Stramski and Kiefer's Mie theory results - for experiments 2 and 3 in oligotrophic waters, which are most applicable, this statement appears to be untrue, or at least not unequivocally supported by the data. Furthermore, since there was no filter used in the experiments with a pore size of exactly 1.2 um (the 50% cutoff for S+K's results), the statement is weakly supported by the data from mesotrophic regions as well. From my viewing of the results, it really does not look like S+K were too far off in this respect. The result that essentially all the bulk backscattering in the oligotrophic samples appears to arise from the relatively narrow 0.2 to 1.0 um size class (i.e. prokaryotes) is absolutely fascinating.

p. 320: In the first bulleted conclusion, it would probably be good for clarity's sake to insert something like ", when considered with previous findings that bulk cp may be used effectively to track phytoplankton biomass," after "suggesting that..."

Fig. 1: if the black triangle sites were labeled 1-2-3 it would be a help to the reader in figuring out which fractionation experiments took place where.

Very nice work!

Best Wishes, Mike

Mike Twardowski WET Labs 70 Dean Knauss Dr Narragansett, RI 02882 401-783-1787 mtwardo@wetlabs2.com

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