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Comment

Interactive comment on “Characterization of the bio-optical anomaly and diurnal variability of the particulate matter, as seen from the scattering and backscattering coefficients, in ultra-oligotrophic eddies of the Mediterranean Sea” by H. Loisel et al.

W. Gardner (Referee)

wgardner@ocean.tamu.edu

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General comments: The primary objectives of this study were: 1) to re-examine the causes of the color anomaly found in ultra-oligotrophic waters of the Mediterranean Sea by adding measurements of the particulate backscattering coefficient, *bbp*, and 2) to test whether or not diurnal cycles in *bbp* could be observed.

These are relevant questions within the scope off BG, and I believe that both of these

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objectives were well met. This is an excellent paper that carefully examines many details of the data that might escape notice if given only a casual examination. It is definitely a paper that will make a difference in studies of bio-optics and will be cited frequently.

Throughout the paper the methods and assumptions are clearly stated, results are well explained and justify their interpretations and conclusions. They note that new commercial instruments have allowed new pertinent bio-optical measurements to be made. The natural outcome of this is also to provide IOP measurements at so many wavelengths that there is an exponential increase in the number of proxy parameters that are being proposed by different researchers, making it more difficult to compare results from different papers because slightly different proxies are sometimes used. The proxies used in this paper are sound and well justified.

Specific comments: The authors point out that while the signal of beam attenuation due to particles, cp , comes predominantly from particles between 0.5 and 20 μm , most of the bbp signal has been assumed to derive from submicron particles based on Mie theory. They point out that recent fractionation experiments by others in both coastal and open ocean waters indicate that at blue wavelengths, more than half of the signal comes from particles $>3 \mu\text{m}$. The importance of making measurements of bbp is that bbp can now be assessed satisfactorily from space, and could provide new insights on biogeochemical fluxes, especially from geostationary satellites (i.e. GOCI and others being planned). It is important, however, to have field data that show how bbp varies with depth, season and diurnal cycles. The data from this paper address variations with depth (variations in the study area are generally what is expected from measurements elsewhere) and diurnal cycling (they do exist in some areas), but the authors acknowledge that further study is necessary to address seasonal variations and other issues.

The paper provides a very good review of recent work in bio-optics and remote sensing, especially the issues pertinent to the Mediterranean anomalous waters. After consid-

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ering three possible explanations for the blue-to-green reflectance anomaly in these waters, they make good arguments that the most likely cause is the presence of very fine Saharan dust, which is manifest by higher concentrations of lithogenic silica in the upper water column.

The specific particulate coefficient, cp^* ($=cp/TChl-a$), of the data was examined as done in Loisel and Morel (1998) as a means of determining changes with depth in the particle composition. This was addressed almost exclusively in vertical changes. It is worth noting that a global map of cp^* agrees with their estimates of 1.0 to 0.1 in surface waters of the Mediterranean, but also shows higher values in the open ocean oligotrophic gyres (Gardner et al., 2006, Fig. 10). Values in the gyres generally exceed 1.0 and during the Austral summer, estimated values are greater than 3 in the eastern South Pacific, the region of clearest surface waters found in the ocean (Morel et al., 2007: Morel, A., B. Gentili, H. Claustre, M. Babin, A. Bricaud, J. Ras, and F. Tieche (2007), Optical properties of the "clearest" natural waters, *Limnol. Oceanogr.*, 52(1), 217-229.)

Technical issues: The paper is well written and the language is fluent and precise with impressively few grammatical/word corrections needed given that I don't think any of the authors are native English speakers. Thank you for your attention to detail. After hand writing corrections on my pdf printout, I downloaded the text version of the paper and made minor corrections in tracking mode. While I have attached the file, I see no purpose in posting the text version with corrections. They will be of no interest to anyone but the authors because they are minor.

The title and abstract are appropriate. Lots of excellent, appropriate references were cited. In my comments above I referenced a paper by Morel et al. (2007) and in the introduction of the annotated text file I noted another early paper that calculated net primary production from diel cycles in cp – Walsh et al. (1995).

The figures are all useful and informative. In the text version in tracking mode I

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suggested a few minor changes in the some figure captions. The only suggested change is in figure 12 where I suggested that it would be best to use different symbols for the two different measurements so that if the figure was copied/printed in black and white, the reader could still distinguish between the two measurements.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/8/C2913/2011/bgd-8-C2913-2011-supplement.pdf>

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