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***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.**

Anonymous Referee #2

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Review for the paper 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 Loisel et al.

The paper presents the results of the optical measurements taken on a research cruise in the Mediterranean Sea. Results are used to examine two separate aspects. The first relates to the underlying causes of the optical “anomalies” observed in the Mediter-

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anean sea and the second the diurnal variability in the backscattering coefficient. Regarding the first aspects the authors examine using their dataset the anomaly previously observed in ocean color algorithms in the Mediterranean sea at low chlorophyll concentration and in particular reexamine the different ideas proposed to explain these anomalies. They find that the a higher than “normal” backscattering coefficient could be the cause of the anomalies, they in turn surmise that this is consistent with the hypothesis that desert dust coming from the Sahara is causing the anomalies. In the second part of their study they examine the diurnal variability observed in the backscattering coefficient and suggest ways to use it to calculates productivity in oceanic waters.

General comments

First part

The paper is well prepared and presented and the topic is definitely interesting to researchers in the field. In essence, the authors bring new measurements of backscattering to the question of the optical anomalies in the Mediterranean Sea. This data seems to indicate that higher than "normal" backscattering in the Mediterranean oligotrophic waters. Several points I feel should be addressed in this first part to make sure that the study is not biased and that the results can be trusted.

1) In oligotrophic waters the sensitivity of backscattering instruments are pushed near their limits and all correction and in particular the the dark measurements become crucial. Were dark measurement subtracted from data? If so how?

2) Comparison with the data of Huot et al. (2008) is used to assess how "abnormal" this data is. Although the spread in Huot et al. data is limited in the very low chlorophyll environment (probably due to a lack of measurements), the spread is clearly larger in more mesotrophic waters. The Mediterranean data in that sense may not be particularly high. I think the discussion has to go beyond just comparing to the trendline and need to include a discussion of the dispersion around this trendline (95% confidence interval on the fitted parameters are given in Huot et al. This might used as a guide in

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the discussion).

3) The method used to derive the backscattering coefficient is different from that used in Huot et al. (2008) could this also cause a bias. Is it possible to derive the backscattering coefficient from a single angle (125 degrees) to make sure this is not underlying the observed differences?

4) How do the backscattering obtained from the AOP inversion compare with the one derived from the ECO-VSF ? Is there any information in the spectral shape of these inverted backscattering coefficient that could support the hypothesis made on page 7885 about the shape of the backscattering coefficient ? Also why would the strongly absorptive particles required to have a higher bbp at 555 than 443 not be important in total absorption measurements (I think I know why but you need to convince the reader)?

5) Backscattering at station BOUSSOLE (Antoine et al. 2011, their figure 5) tend to be lower than Huot et al. (2008) relationship during oligotrophic periods. This needs to be addressed as it is in the opposite direction from the present observations.

6) The effect of CDOM absorption is assessed using the inversion of remote sensing reflectance. The unpublished inversion algorithm used is said to have an RMSE of 0.02 m⁻¹ for total absorption, yet the retrieved absorption of aCDOM+asp retrieved is 0.0054 m⁻¹. Of course, I understand that the stated 0.02 m⁻¹ is not necessarily representative of the ability of the model in these waters, nevertheless, a better statistics of its performance in oligotrophic waters need to be given to convince the reader that the value provided is not off by a factor of 4 or more (especially since the error given on the 0.0054 m⁻¹ is 0.0011 m⁻¹).

7) How do the profiles of lithogenic silicates presented in figure 10, compare with other open ocean area. Are they really much higher than those in other oligotrophic waters?

8) If the lithogenic silicate is an important cause of backscattering, why don't we see

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profiles of bbp similar to those measured for lithogenic silicate?

9) I find it surprising that given the suggested large influence of the submicron lithogenic silicate on bbp, that the changes during the day of bbp (second aspect discussed in the paper, p. 7889, line 5 to 10) are only slightly smaller than those of cp which would be much less affected by this non-living part of the particle pool.

In other words, while the authors tend to follow one line of evidence, there are several observations that must be addressed to verify that their hypothesis is really supported by their observations.

Second part

My main comment here is that I feel the presentation of the results of diurnal variations in bbp is fine, novel and interesting, but the discussion surrounding is it too long and mostly unsupported. As such the discussion about the sources of potential sources of variability could be shortened. As well the derivation of production rates appears to me a bit far fetched, while it can be done mathematically and compared with other values it really doesn't bring anything to the paper as it cannot be validated nor do we have any way to know what it means in reality (since we do not know the sources of variability in diurnal changes of bbp). These variations, once we know their amplitude (say 14% per day) can be very simply transformed into production rates using some POC values: it doesn't mean that it means anything to do it. In my opinion this section should be shortened considerably, results can be put in a table for future uses and a minimum discussion of the diurnal variation should be kept. To me the most important finding related to these diel changes is that a significant fraction of the bbp is most likely caused by living (growing and respiring) organisms (or their diurnally variable wastes). This is what should be highlighted (though, as noted above, it is somewhat at odds with the first part of the paper. . .).

Technical points

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1) p. 7862, line 7. No need to define and use the abbreviation BP, it is more confusing than anything else as it is often used for “bacterial production”.

2) P. 7863, line 10. “Marginal” seems an inadequate adjective as used for seas since marginal seas already have a specific meaning. Perhaps another adjective can be found.

3) P. 7871, lines 13-15. I think it is an oversimplification to state that the DCM is caused by an increase in the intracellular chl content. You might skim the results of Grob et al. (2007) which clearly suggests that cell numbers also play an important role in this feature. Papers cited here though excellent are a bit dated (before the flow cytometry era).

4) P. 7871, lines 17 to 20. The section explaining why there is an increase in the fluorescence/chl a ratio is confusing and perhaps even wrong. Shouldn't the ratio discussed be fluorescence / Tchl a for this discussion to make sense? Are the authors assuming divinyl chlorophyll a is not fluorescing (it is in very similar ways to monovinyl chlorophyll a)? In any case I do not follow the logic here.

5) P. 7872, line 2 to 5. I don't understand how these two sentences follow each other. Prochlorococcus and Synechococcus are both part of the picoplankton.

6) p. 7873, line 17. I think “photoacclimation” should be replaced by “photoadaptation” since we are most likely dealing with different genotypes here (see for example Raven & Geider, 2003).

7) Figure 3 Backscattering panel. I think the important information to show here is the mean profile; therefore the axis could be scaled so that extreme peaks are outside the graph (say -0.0001 to 0.002 m⁻¹). Similarly for cp, the x-axis could be limited to 0.12 m⁻¹.

References

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