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Biogeosciences
Discussions

Interactive comment on "Intrusion of coastal waters into the pelagic Eastern Mediterranean: in situ and satellite-based characterization" *by* S. Efrati et al.

S. Efrati et al.

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We are thankful to the reviewer for his thorough reading and constructive remarks. Bellow are our replies to the reviewer comments point by point.

1. Comment: Comparing figure 4b (calibrated fluorescence) and figure6 (satellite transects, in particular the b), the ranges are different. Calibrated fluo ranges between 0.01 and 0.09, whereas satellite Chl between 0.05 and 0.2. If I interpret well the figures, you have a factor of 2 between the two Chl estimations. I suggest commenting the satellite ocean colour limits in the Mediterranean in general, and in the region you analysed in particular. Although I'm persuaded that the results of the authors are robust (because

obtained for the most in relative), you have to convince the readers about the quality of the satellite Chl estimations.

1. Reply: We thank the reviewer for drawing attention to this issue. The fluorescence data is not calibrated and the plot is in arbitrary units. This is now mentioned explicitly in the caption of Fig. 4. Indeed, as mentioned by the reviewer, in the scope of this manuscript the absolute chlorophyll values are less of an issue, as the satellite observations are mainly used for spatial characterization of the patch. Yet, we acknowledge the importance of adding such discussion. The following text was added to the revised manuscript: "Satellite retrieval of Chl in the Mediterrenean are systematically overestimated, among others due to the presence of suspended Saharan dust and coccolithophores in the water column (Claustre et al., 2002; D'Ortenzio et al., 2002) . Furthermore, since the study area is close to and influenced by the coastal zone, the waters can be considered as case 2, where optical properties are mostly influenced by mineral particles or colored dissolved organic matter (Morel and Prieur, 1977). As in Bignami et al., (2007), we thus point to the limitations of the term "chlorophyll" as used in this paper, and note that there may be differences between the satellite retrievals and and in situ values.".

The following references were added accordingly: "Bignami, F., Sciarra, R., Carniel, S. and Santoleri, R.: Variability of Adriatic Sea coastal turbid waters from SeaWiFS imagery, Journal of Geophysical Research-Oceans, 112, doi:10.1029/2006JC003518, 2007

Claustre, H., Morel, A., Hooker, S. B., Babin, M., Antoine, D., Oubelkheir, K., et al.: Is desert dust making oligotrophic waters greener? Geophysical Research Letters, 29 (10), doi:10.1029/2001GL014056., 2002.

d'Ortenzio, F., Marullo, S., Ragni, M., Ribera d' Alcalá, M., and Santoleri, R.: Validation of empirical SeaWiFS algorithms for chlorophyll-α retrieval in the Mediterranean Sea. A case study for oligotrophic seas. Remote Sensing of Environment, 82(1), 79−94,

2002.

Morel, A., and Prieur, L.: Analysis of variations in ocean color, Limnol. Oceanogr., 22, 709 – 722.,1977”

2. Comment: Please be more precise in the methods you used to identify the patch spatial characteristics (pag 17980, lines 10-15)

2. Reply: The following sentence was added: “The patch boundaries are delimited by the location of the Chl front, which separate the area of high-Chl associated with the patch, from its low-Chl surroundings”

3. Comment: Following the author’s interpretation of the data, a patch of coastal waters intruded for about 100km in the open ocean Levantine waters. This likely means that optical characteristics of the waters of the patch (at least in surface) are of case II (i.e. typical of coastal water). I suggest to verify additional ocean colour products (see for example Bignami et al. 2007, which could improve the interpretation of the results.

3. Reply: As suggested by the reviewer, we have examined maps of PIC, POC and CDOM, which indeed show a similar spatial pattern (see attached). Yet, to our understanding, adding these figures to the manuscript does not contribute to the interpretation. We do however acknowledge it is important to reefer explicitly to possible contribution of suspended particle. This was done in the revised manuscript (see above the reply to first comment).

Interactive comment on Biogeosciences Discuss., 9, 17975, 2012.

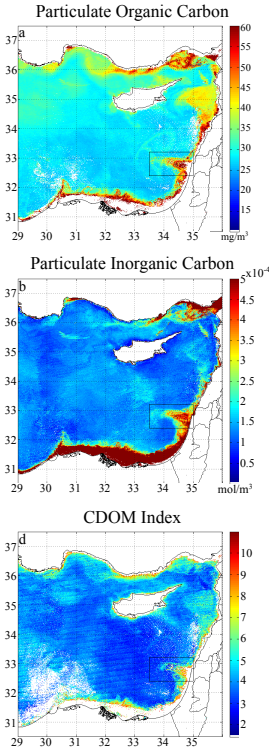

Fig. 1.