

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

Anonymous Referee #1

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This paper reports an intrusion of coastal waters into the offshore pelagic water of the ultra-oligotrophic Eastern Mediterranean Sea by analyzing the physical and biogeochemical parameters. The authors combined the data from near real time multi-satellite observations with those from in situ measurements in order to identify and characterize the coastal water intrusion. They suggest that the coastal water intrusion has an important impact on the ultra-oligotrophic Levantine basin ecosystem through (1) transport of nutrients and coastal derived material and (2) formation of local, dynamically isolated, niches.

Main comments This paper is well-written and easy to understand. I have only minor

C7656

comments below.

Other detailed comments P17976, L13-15: Nutrients are supplied from the deeper layer during the winter overturn. The authors should consider if this conclusion is true for all seasons or the limited period.

P17976, L20-23: Neither Siokou-Frangou et al. (2010), Krom et al. (2010) nor Tanaka et al. (2007, 2011) is the paper reporting the hydrology of the Mediterranean Sea. Please cite the original paper.

P17979, L2124: Please indicate the detection limit and the precision of the nutrient analysis.

P17979, L21-26: Samples for nutrients and picophytoplankton were taken in duplicate. Please specify if the results are shown as average value of duplicated samples or based on single measurement.

P17980, L12 and elsewhere: Please specify the average values \pm 'what'?

P17982, L22-25: I understand that the rate of chlorophyll increase here presents the difference between growth and loss (i.e. net increasing rate). This may suggest that loss rate was insignificant compared to growth rate in this water.

P17983, L8-9 & L26-27: Please specify if the authors did correlation analysis or regression analysis, and if the relationship was statistically significant.

P17983, L12-16: It would be interesting to analyze if concentration of Si(OH)_4 was anomalously high compared to that of NO_3+NO_2 and PO_4 in the coast or NO_3+NO_2 and PO_4 were preferentially consumed during the transport. How about spatial changes of nutrient stoichiometry?

P17983, last paragraph: Please specify if only picoeukaryotes abundance showed significant relationship with water temperature. If *Synechococcus* abundance does not show significant relationship with water temperature but is higher inside the patch than

C7657

outside, will the data interpretation be revised? Although the authors mention "It is reasonable to assume that the higher abundance of *Synechococcus* and picoeukaryotes inside the patch was a result of available nitrate and phosphate supplied from the near shore", they should consider that heterotrophic bacteria are also able to consume inorganic N and P. The authors may need to comment on why *Prochlorococcus* did not show any significant spatial trend. Physiological responses to nutrient availability may be different among these picophytoplankton groups? It would be interesting to compare chlorophyll concentration (a proxy of total phytoplankton biomass) with water temperature.

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

C7658