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Interactive comment on "The organic sea surface microlayer in the upwelling region off Peru and implications for air–sea exchange processes" *by* A. Engel and L. Galgani

Anonymous Referee #1

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Engel and Galgani present data on the enrichment of orgnic matter in the microlayer collected from an upwelling system. In brief, the idea of enrichment of organic matter in the microlayer is not new (has been published before with way more discussion), the data added to the pool collected by others is incremental only (i.e. it does not illuminate us beyond what is known). Additionally the section on air-sea gas exchange and aerosol is odd. No data of the former sections is discussed here, and it stands like a little review. Some of the approaches are questionable (TOC/DOC measurement, microlayer sampling). See below for details

Page 10580 Line 23: Sieburth (1983) suggested for the first time that the SML has a gel-like nature. It has been then experimentally confirmed by Wurl and Holmes (2008)

and later summarized by Cunliffe and Murrell (2009). In this context here, Sieburth (1983) should be cited.

Sieburth, J.McN., 1983. Microbiological and organic–chemical processes in the surface and mixed layers. In: Liss, P.S., Slinn, W.G.N. (Eds.), Air–Sea Exchange of Gases and Particles. Reidel Publishers Co., Hingham, MA, pp. 121–172.

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Line 15: Authors need to be more careful in the selection of citation. The term "dynamics" means a process over time, and Bigg et al. (2004) didn't report data on phytoplankton dynamics. Authors need to check other citations.

Line 29: Authors use the term "sea-surface microlayer" and its acronym SML randomly.

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Line 25: Isn't it the first data set? In this case calling it the "most extensive" is a bit unusual.

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Line 16: The withdrawal rate of 20 cm/s by Harvey and Burzell (1972) has been revised by Carlson (1982) to 5-6 cm/s. As clearly shown by Carlson (1982) fast withdrawal rates collects thicker layers, and his revised rate of 5-6 cm/s correspond to a thickness of about 50-60 μm. Zhang et al. (2003) showed experimentally that the SML has a typical thickness, although varying with sea state, of 60 μm. The methodological flaw of Engel's and Galgani's study causes underestimations of enrichments as bulk water probably diluted collected SML.

Carlson, D. (1982). A field evaluation of plate and screen microlayer sampling techniques. Mar. Chem. 11, 189-208.

Zhang, Z., Cai, W., Liu, L., Liu, C., and Chen, F. (2003). Direct determination of thickness of sea surface microlayer using a pH microelectrode at original location, Sci.

China Ser. B, 46, 339–351.

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Line 1: The formula represents the thickness of the collected water layer, not necessarily thickness from SML. There is no formula to calculate the thickness of the dynamic SML, even though it is assumed to be in a range of 40-100 μm depending on sea state. Zhang et al's study (2003) supports it by lab experiments.

Line 14: Authors analyzed TOC/DOC according to Sugimura and Suzuki (1988), but Suzuki (1993) retracted the paper as their method produced erroneous data (shown by Benner and Strom, 1993). I am surprised seeing a citation to Sugimura's and Suzuki's paper. Despite some QA measures, no results are presented and calibration every 8-10 days seems with my experience inappropriate. I have to assume that TOC/DOC data in this study lack on accuracy.

Suzuki, Y. (1993). On the measurement of DOC and DON in seawater. Mar. Chem. 41, 287–288.

Benner, R., Strom, M. (1993). A critical evaluation of the analytical blank associated with DOC measurements by high-temperature catalytic oxidation. Mar. Chem. 41, 153–160.

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Line 1: Again, calibration every 8-10 days seems inappropriate for the encountered concentrations on TN and TDN.

Page 10589

Line 8: The reported thickness of collected layer (50 μm) can't be right, not with the fast withdrawal rate of 20 cm/s the authors applied here. The reported thickness corresponds to a withdrawal rate of 5-6 m/s. The authors should note that the cited paper of Cunliffe et al. (2013) refers to SML sampling guidelines clearly suggesting 5 cm/s as

rate.

Page 10589 to 10592

A more through data analysis (e.g. multivariate analysis) would be beneficial to describe new insights into enrichment patterns. The presented coefficients represent a moderate correlation, but Most of the findings in the current paper have been reported in the past, and it is not clear what the new results are.

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Line 11: Lower TEP enrichment could be an artifact of fast withdrawal rates applied to the glass plate sampler. Line 15: How about losses of TEP, POC via increased sea spray? Line 17: Data on size distributions are not discussed.

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Line 8: Any reference to TEP enrichments in slicks?

Page 10597-10600

Both sections on effects of the SML on air-sea gas exchange and aerosol composition are well known, and extensively reviewed in the past (Liss and Duce, 1997; Cunliffe et al. 2013; Carpenter and Nightingle, 2015). These sections seem like afterthoughts without much discussion on own observation, and if so data are over-interpreted. For example, the statement "SML may play a particularly important role for exchange of relevant climate gases" is just a repetition of other recent studies and reviews (Salter et al., 2011; Cunliffe et al., 2013). So what is new here?

Table 3: The column heads are wrong as no temperature or wind speed are presented, but correlation coefficients.

Figures 2,4 and 5: Are over interpreted by the knitting algorithm. What settings in ODV have been applied?

Figure 6: The y-axis needs to be revised to make it more readable. Why the authors do not presented POC enrichments. POC is known to be well enriched.

Figure 7: Why does Figure 7b contain much fewer data points than Figure 7a? I understand that the authors measured each parameter at every station.

Interactive comment on Biogeosciences Discuss., 12, 10579, 2015.