

## ***Interactive comment on “Changes in optical characteristics of surface microlayers hint to photochemically and microbially-mediated DOM turnover in the upwelling region off Peru” by L. Galgani et al.***

**Anonymous Referee #1**

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General comments: The present study investigates the compositions, enrichments and biological production of chromophoric dissolved organic matter (CDOM) in the surface microlayer (SML) and underlying water (ULW) of the upwelling region off Peru. The compositions and variations of the CDOM in these two distinct seawater pools and what controls those compositions are indeed of great interest for oceanographic and atmospheric communities due to their impacts on the emission of climate-relevant gases and the climate system as well as oceanic carbon cycling. Overall, the data appear to have high quality, however, the presentation and interpretation drawn from the data

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need major revisions before the paper can be considered for publication. Specific comments: 1. Page 19374, lines 10-14. Because the data of DOC, amino acids, marine gels, and bacterial abundance were cited from Engel and Galgani (2015), it is more appropriate to describe these biochemicals in the discussion section. I suggest that the authors omit the sentences “In order to understand...microbial alteration processes” from the abstract. 2. Page 19376, line 7. “biological liability” should be biological lability. 3. Page 19378, lines 6-8. Please clarify and supplement the purpose of this study. It can be emphasized that the meaning of the CDOM differs from those of other biochemicals (DOC, amino acids, etc) and is more specific about what scientific questions will be addressed in this study. 4. Page 19380, the section “2.2 Chemical and biological analyses” need some reorganization for conciseness. I found that the analyses procedure of DOC, amino acids, phytoplankton, gel particles and heterotrophic bacteria were mostly copied from the paper of Engel and Galgani (2015). I think that there was no need to make a detailed description of the analytical methods for these compounds. 5. Page 19381, lines 11-13. In this study, using 2% (THAA%-DOC) as the threshold for DOM lability may be inappropriate, because the THAA yields in different sea areas are not comparable. I think that a direct comparison for their values is more reasonable. If possible, I suggest that the authors could calculate the “degradation index” (Dauwe and Middelburg, 1998; Dauwe et al., 1999; Kaiser and Benner, 2009; Peter et al., 2012) based on the amino acids mole percentages, which can help to evaluate the degradation states of organic matter between the SML and ULW. Dauwe, B., Middelburg, J.J., 1998. Amino acids and hexosamines as indicators of organic matter degradation state in North Sea sediments. *Limnology and Oceanography* 43, 782–798. Dauwe, B., Middelburg, J.J., Herman, P.M.J., Heip, C.H.R., 1999. Linking diagenetic alteration of amino acids and bulk organic matter reactivity. *Limnology and Oceanography* 44, 1809–1814. Kaiser, K., Benner, R., 2009. Biochemical composition and size distribution of organic matter at the Pacific and Atlantic time-series stations. *Marine Chemistry* 113, 63-77. Peter, S., Y. Shen, K. Kaiser, R. Benner, and E. Durisch-Kaiser, 2012. Bioavailability and diagenetic state of dissolved organic matter in riparian

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groundwater, *J. Geophys. Res.*, 117, G04006, doi:10.1029/2012JG002072.

6. I suggest the authors avoid discussing data in the results section. For example, sentences on lines 4-7 (page 19386), lines 21-23 (page 19386) and lines 1-4, (page 19391) belong to the discussion section. 7. P19392, in the section 4.1. Lots of data including temperature, salinity, wind speed, radiation and different DOC type refer to Engel and Galgani (2015) in the SML and ULW. If the authors can combine these environmental parameters to discuss the enrichment of CDOM, that will help to increase our understanding of CDOM enrichment. 8. P19393, line 26. The component F1 showed a protein-like fluorescence of autochthonous material, and those (F2, F3 and F5) had the characteristics of terrestrially derived fulvic-acid like or humic-acid like DOM. But as showed in Table 3, the autochthonous component F1 negatively correlated to salinity, and no correlations were found between the terrigenous components and salinity. It is in contradiction that terrigenous material usually negatively correlated with salinity. 9. P19394, line 22. Table 2 should be Table 3. 10. P19395, lines 23-25. The authors present a good example of the conceptual model of CDOM production and removal between the SML and ULW. I suggest that the author could emphasize this model in the abstract section to attract readers.

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