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Biogeosciences Discussions

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.

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We thank the referee for the evaluation of our manuscript and for the constructive review provided. Specific issues raised are addressed here below.

1. Referee: 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. Authors: we will rephrase that part of the abstract and refer to the data by Engel and Galgani (2015) in the discussion section.

2. Referee: Page 19376, line 7. "biological liability" should be biological lability Authors: We have corrected it.

3. Referee: 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. Authors: the referee is right, we will supplement and discuss more extensively about the purpose of this study in the revised version.

4. Referee: 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. Authors: we will shorten the description of the analytical methods for amino acids, gel particles, heterotrophic bacteria and phytoplankton cells making reference to Engel and Galgani (2015).

5. Referee: Page19381, 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 groundwater, J. Geophys. Res., 117, G04006, doi:10.1029/2012JG002072. Authors: Amino acids generally comprise a large fraction of bioavailable organic matter and are preferentially consumed by microbial activity quite rapidly. In surface waters they may be easily photodegraded too. Therefore, the amount of carbon included in amino acids is considered as a good indicator of DOM diagenesis. The study by Peter and colleagues (2012) refers to ground water, presenting three indicators of DOM diagenesis: amino acids concentrations, carbon-normalized yields of amino acids, and degradation index. As the referee suggests, we will compare our results with the study by Peter et al. (2012). Kaiser and Benner (2009) and other references already cited in the text (e.g. Davis and Benner, 2007). We are aware of the studies suggested by the referee and the calculation of the degradation index for amino acids. However, the degradation index calculated by Dauwe and colleagues refers to POM in sediments and could at best also be only an indication for DOM diagenesis.

6. Referee: 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. Authors: The referee is right; we will thoroughly revise the results section avoiding discussing data in that context.

7. Referee: 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. Authors: we will revise the results combining these data to understand CDOM enrichment in the SML even better.

8. Referee: 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. Authors: It is true that terrigenous material may have negative correlations to salinity as their concentration is higher in freshwater bodies. However, in the SML of the Peruvian EBUS, a combination of processes due to the complexity of the system may be responsible for DOM accumulation and alteration, such as upwelling of colder waters (as also indicated by the negative relationship of F1 to temperature). Therefore a straightforward relationship between CDOM and salinity cannot always be established in this case. In our study the negative correlation coefficient of F1 to salinity, although significant, was low (-0.24). In the revised version, we will rephrase the findings suggesting that part of the variation in F1 may be accredited to a salinity gradient which probably does not reflect input of terrestrially-derived material but local upwelling and remineralization processes of DOM.

9. Referee: P19394, line 22. Table 2 should be Table 3. Authors: We corrected it.

10. Referee: 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. Authors: we appreciate the suggestion and we think it is a good idea to have the conceptual model as graphic abstract, so we will emphasize it in the abstract.

Interactive comment on Biogeosciences Discuss., 12, 19373, 2015.