

Interactive comment on “Formation and distribution of sea-surface microlayers” by O. Wurl et al.

Anonymous Referee #2

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There is a long lasting interest in the effects and sources of sea surface microlayers in the ocean. Not the least because the SML represents the first barrier of molecules and organisms between the atmosphere and the deeper layers of the ocean. Depending on its composition, the SML may affect the exchange of gases between these two environments. Thus estimates of the extent and strength of sea surface microlayers in the oceans seems an important first step to understand some of the variability in the influx and efflux of anthropogenic greenhouse gases for example.

Wurl et al. address this issue by an extensive new dataset (337 samples) collected in various costal and oceanic stations around North America. Sampling was done with a glass plate sampler and measurements included surfactants, total dissolved carbohydrates and chl a in the SML and at 1 m depth, respectively. Together with

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wind speed data measured on site and modeled primary production from satellite data they aimed at seasonal global maps on the intensity, or enrichment, of sea surface microlayers in the ocean.

The sampling (a science in itself) and other analysis is done thoroughly, however, I have some reservation on the story of the manuscript:

There are no relationships that would make it possible to clearly derive the intensity of enrichment in the SML. Primary production was used to distinguish the three trophic states (eu-, meso- and oligotrophic) and wind speed was used as a threshold value above which the SML is likely to be broken up continuously. Global mapping of the SML seemed to me the major goal of study, however, I wonder whether the current dataset allows for such global extrapolation. Furthermore I completely agree with anonymous reviewer #1 (for the reason he invokes) that is important to report concentrations of the surfactants rather than presenting enrichment factors only.

In general the text should be more focused and particularly the discussion should be extensively edited and shortened. There is much in the discussion that does not directly refer to the data measured in the current study. However, there is data enough that allows for an informative paper on the SML provided the current draft is given a good overhaul. Particularly the finding, that the SML is 'stable' at wind speeds of up to 10 m/s was interesting to me.

Specific Comments:

Introduction:

Page 5721; line 14-15: If the authors make a statement like this, then I would expect them to be more specific on what the arbitrary thresholds where and why they are considered arbitrary.

Page 5721; line 28-29: For me, this an overstatement as the dataset used is mainly restricted to sites near North America with one study at Hawaii.

BGD

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Methods:

Page 5722; paragraph 1: To be able to judge the trophic situation by the reader, I suggest to introduce the different sampling sites more thoroughly or at least provide links or citations where information can be found on e.g. latitude/longitude, major nutrient concentrations etc.

Page 5723, paragraph 1: Is there any special reason why the authors deviated from the sampling scheme of Harvey and Burzell? I assume it is because of Zhang et al., however, as written this is not that clear.

Page 5724, line 1: In general I suggest to check that abbreviations in the main body of the text are properly introduced on first use (here for example CA or HI).

Page 5724, paragraph 1: I can only assume that the chemical measurements including Chl a were done for all stations. This should be more explicitly pointed out. How much volume was used for the different analysis?

Page 5726, line 9-11: I don't think it is necessary to explain the categorization of the different wind states in such detail. It is not used anyway as far as I could see.

Results:

The results are confusing to me. I think with better tables (indications of which parameters were significant, more detailed captions of parameters presented) and more direct description of the tables and figures this could be improved. The final global maps are divided by seasons, however, seasonality was not clearly presented in the results section.

Page 5727; paragraph 1: This paragraph does not adequately describe why slick samples have been excluded from the analysis. If the mean enrichment is not significantly higher why are they excluded? How many slicks did the authors encounter? I miss references to data in tables of figures and there is no description/presentation of the frequency of rainfall events e.g.

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Page 5727; line 16: I am not familiar with the Teq unit. I suggest that the authors include a brief description of different Teq units for different oceanic environments to give the reader a feeling for the numbers– this might be done in the methods or in the discussion.

Page 5727; line 28: I suggest introducing the relation between phytoplankton and surfactants in the introduction more detailed, but not in several places as is the case for the current draft.

Page 5729; line 15 onwards: There is hardly any data presented in this section. The description of how the global mapping was done should go into the methods section and should be mainly discussed in the discussion section. I would delete the paragraphs on global mapping in the results.

Discussion:

Page 5731; line 1: This counter argument is quite weak considering that the current dataset is biased to regions north of 30°N and there is no data presented to the south of the equator.

I agree with anonymous reviewer #1 that the discussion should be extensively edited but also shortened. Much of what is there now, is more a review of other papers than a discussion of the presented data and findings. E.g. the paragraph on UV (Page 5734) and the ‘discussion’ on TEP seem out of place. At least it was not obvious to me what point the authors tried to make here.

Page 5738, line 7: Is there any explanation for the counter intuitive trend between surfactant enrichment in the SML and primary production?

Page 5739, line 8: In the light of the presented data the conclusion is an overstatement.

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