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Biogeosciences
Discussions

Interactive comment on "Seasonal signatures in SFG vibrational spectra of the sea surface nanolayer at Boknis Eck Time Series Station (SW Baltic Sea)" *by* K. Laß et al.

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Based on the technique of vibrational sum frequency generation spectroscopy (VSFG), Laß et al. presents a seasonal characterization of the sea-surface nanolayer. The use of VSFG studying the monolayer on top of the ocean′s surface has been already demonstrated by the same authors, and here they present data from a long-term study.

My basic concern is that the authors assumed that their ex-situ measurements are representative for natural conditions, and that oxygen in surface water (1 m depth) is a good proxy for primary productivity. The discussion needs to be re-written and focus on the major statements of the mansucript. The manuscript is well structured and mainly

clear, the language is fluent.

Abstract: Revise carefully and provide a concise summary of the manuscript. Description of sampling station and period should not be part of the abstract, but of the methodological section

Page 3179, Line 5 I think that the nanolayer is much too thin to consider it as a place of particle enrichment. The sea-surface microlayer is a better description for floating particles.

Page 3179, Line 8-14 The statements here are contradicting stating first that the nanolayer has only a minor effect on air-sea gas exchange, and subsequently discussing its role in wave damping and further to its role in climate due to imparing gas exchange.

Page 3180, line 7 As the community starts to understand the role of sea-surface microlayers in air-sea interactions, the claim that nanolayers are "prerequiste for an improved parameterization of its role in the context of climate models" is extremly speculative and should be removed.

Page 3181, Line 13 Use acronyms, here BE.

Page 3181, Line 20 I do not understand how the theoretical enrichment of 12.5 has been derived? I think the natural enrichment processes, especially at the micro- and nanometer scale is so complex, that assuming ex-situ nanolayers are representative to natural ones are speculative. How about bubble transport, micro-scaled turbulence, atmospheric deposition influencing chemical composition? What means "is expected to re-stablish"? How sure are the authors about their statement? In addition, details of the sampling platform (research vessel, small-sized vessel, zodiac) has not been reported at all, although critical for sampling the micro- and nanolayer.

Page 3186, Line 1 and throughout manuscript What is a overlayer?

Page 3185, Line 21 to Page 3186, Line 12 A signal at 3700 cm-1 is also present in

August (see Fig.1) and contradicts the interpretation that the broad signal between 3400-3500 cm-1 indicates intensive organic layers. Indeed, the signal at 3700 cm-1 seems to increase from March to Oct/Nov, and then to August.

Page 3186, Line 25 I disagree that oxygen concentration maxima in surface water (1 m depth) is a good indicator for spring algal bloom. How to ensure that the oxygen maxima are not due to storm events commonly occurring in spring? The wording is awkward. . .how can a coincidence be a good indicator? According to the Figure 2, some of the chlorophyll maxima occurred in Dec 2009, and February 2011. It seems rather early, and I am wondering if the authors correctly plotted the data. Oxygen maxima occurred in February during the whole study period. . .again a season of probably more storm events. I suggest to present wind and oxygen data during sampling period (not monthly average value).

Page 3189, Line 5 What is a correlation function? I know functions obtained from regression analysis, but not correlation. Please explain.

Page 3191, Line 15 Any references for the presence of lipo-polysaccharide and/or TEP like material on the ocean′s surface?

Page 3191, Line28 It seems that Wurl et al (2011) reported a time series of enrichment of total dissolved carbohydrates, but not the absolute concentration. Lowest enrichment was found at times of highest productivity, and it seems wrongly interpreted by the authors.

Page 3192, Line 14 to Page 3193, Line 11 A rather length discussion if oil spills and bathing could have been biased to the results seems to be inadequate. For example, the authors claim that sloppy feeding by zooplankton may lead to pronounced accumulation of organic material in the nanolayer without reporting any data on zooplankton abundance. I believe such data should be available from the time series station at Boknis Eck. Overall, the discussion requires major revision, and focus on the major statements of the paper rather than on bathing.

Overall, the manuscript requires major revision, especially in terms of methodology , data evaluation and discussion.

Interactive comment on Biogeosciences Discuss., 10, 3177, 2013.