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Biogeosciences Discussions

Interactive comment on "Succession of the sea-surface microlayer in the Baltic Sea under natural and experimentally induced low-wind conditions" *by* C. Stolle et al.

Anonymous Referee #3

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Review of Ms: bg-2010-90 "Succession of the sea-surface microlayer in the Baltic Sea under natural and experimentally induced low-wind conditions" by C. Stolle, K. Nagel, M. Labrenz, and K. Jürgens

Recommendation: After revision, the manuscript is recommended for publication

The manuscript describes the investigation of the effect of induced low wind on organic matter and bacteria abundance and productivity in the sea-surface microlayer. The authors designed two experiments, including few field and mesocom studies. The description of chemical and biological properties of the SML is poor and the work present some new findings in particular for slick formation. However, I have some major con-

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cerns as listed below:

1)Sampling strategy

Sampling of the SML is critical and controversy, especially for biological properties. The authors argument to chose the glassplate sampler is confusing. First they state the glass plate potentially dilute SML samples with bulkwater. Then they claim that glass plate sampler introduce no bias in the measurement of biological parameters, even so in their earlier paper (Stolle et al. 2009) they report inhibition of bacterial productivity by 90% in samples collected with the glass plate and metal screen sampler.

According to my opinion, the glass plate sampler is applicable to most chemical parameters and collect layers equivalent to the actual SML thickness (e.g. 60um) measured with in-situ micro-electrodes (Zhang et al. 2003. J. Colloid Interface Sci. 204, 294-299), that means without dilution. For that reason, statement on page 3157, L6 is misleading as it may be valid only microbiological parameter.

As reported by Cuncliffe et al (2009), hydrophilic polycarbonate membrane is the best choice for molecular microbial analysis including nucleic acids. That would not solve the problem of inhibited bacterial activity as the membrane does not collect sufficient volume. However, that needs to be addressed in more details in the discussion, in particular in regards of the author's earlier findings in Stolle et al. (2009)

2) Observation period

Unfortunately, the authors did the observation for each experiment only for a period of four days which limits the scientific value of the study with atotal of four experiments. As multiple parameters may control SML enrichment processes, routine long time-series observations may prove to be the best way to study this complex system. I understand that time constraints may have limited observation but revisited the study site for a period of 3-4 weeks may have been useful. As describe in the manuscript (3157, L26), the mesocoms were not robust enough to last longer than four days, but

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an inprovement in their design seems to have been necessary.

I am particular concern about the model the authors describe with this limited observation in section 4.3/Figure 5. I recommend deleting the description of the model and Figure 5 due to the limited observation made.

3) Limitation to coastal waters

I feel the authors need to highlight that the experiments have been conducted in proximity to the shoreline, e.g. under coastal conditions. The mesocom have been deployed in an area heavily influenced by the inflow river water. The comparison with the field study, which seems to be located further offshore, is difficult due to the choice of different sites.

Overall, I suggest that the authors highlight the differences of the sampling sites in the section of Methods and Discussion. I feel also that the title should be changed to "...in the coastal Baltic Sea..." or "Succession of coastal sea-surface microlayer..."

More specific comments:

P.3161, L21

I do not understand the last sentence here. The authors need to clarify if they have measured TOC/TON or DOC/DON? Or was DOC/DON measured in addition to TOC/TON? If so, why since TOC=POC+DOC. Analyzing unfiltered coastal waters in a HTCO analyzer is prone for high errors due to particulates accumulating on the catalyst affecting recovery. Using an autosampler, particles tend to settle to the bottom of the sample vial. Report relative errors and blanks for those measurements.

P. 3163, L13 and L15 Did the bacterial abundance and activity decrease to the value from the first day?

P.3162, Section 3.1.1 Obernosterer et al. (2008) observed a linear relationship between wind speed and POC/PN enrichment. As this manuscript report all those param-

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eters, the authors should investigate for a similar relationship to add further evidences that the enrichment of particulates depends on wind stress.

P.3164, L1 Rewrite this sentence as it is somewhat contradictory.

P.3165, L15 Were the enrichments of DOC/DON similar between outside and inside mesocom. Have depletion of DOC/DON (EF<1) observed?

P.3165, L23 Discuss inhibition of bacterial activity using glass plate sampler. That may have affected the observation of lower bbacterial activity in the SML.

In Figure 3e, during experiment 3 high bacterial activities has been observed in the ULW during the last two days. Those observation seem to be exceptionally high, and affect the statistical conclusion that bacterial activity was significant greater in the ULW than in the SML. Regarding the measurement of bacterial activity, it seems to me from p.3159, L 9 that only a single sample of 2.5 mL has been measured, e.g. no triplicate. Is that correct? The authors need to discuss those two observation during the last two days of the third experiment.

P.3166, L1-7 In L4 it says that the enrichment were different ($p<0.04$) but the authors claim then that the difference were not statistically different using Bonferroni correction. It is a confusing sentence.

P.3166, L21 That is the reason why SML studies requires observation over a longer period. contradictory

P.3166, L25-27 SML is spatially very heterogenous due to dispersion processes by wind and surface currents. I believe the observation that enrichments are always higher inside the mesocom is not surprising as no dispersion occurs within the mesocom. This observation could be an artifact of the experimental design. P.3170, L7 Low cell-specific activity in the SML may have been caused by the glass plate sampling technique using a hand wiper as the authors reported in an earlier paper (Stolle et al. 2009, p71, section “tank experiments”). The authors seem to ignore their earlier

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findings on the effect of sampling technique on 3G-TdR incorporation activity.

P.3170, L13 It may be worthwhile to mention a study (Elasri and Miller, 1999; Appl. Environ. Microbiol. 65, 2025-2031) showing that biofilms offer considerable protection from UV radiation to bacteria. The SML with its hydrated gel matrix is certainly a biofilm habitat.

P. 3172, L15 Based on the limited observation period for each experiment (3-4 days) and the heterogenous nature of the SML I feel that this statement is rather speculative. The experimental design of mesocom studies may have also caused some artificial enrichment.

P.3173, L6 Some important references are missing here reporting that the bacterioneuston is a different microbial ecosystem compared to the community in the underlying water, at least in estuarine and coastal waters (Fehon and Oliver, 1979, Estuaries 2, 194-197; Cuncliffe et al., 2008, ISME 2, 776-789; Franklin et al. 2005, Environ. Microbiol. 7, 723-736). As those studies refer to estuarine/coastal SML, I suggest to include these references in the discussion. The authors cite the excellent work by Obernosterer et al. 2008. However Obernosterer et al's work refers to an oligotrophic area in the South Pacific. . .very different to the conditions of the presented study.

Interactive comment on Biogeosciences Discuss., 7, 3153, 2010.