Biogeosciences Discuss.,
doi:10.5194/bg-2016-7-RC2, 2016
© Author(s) 2016. CC-BY 3.0 License.

Biogeosciences Discussions

Interactive comment on "Surfactant control of gas transfer velocity along an offshore coastal transect: results from a laboratory gas exchange tank" *by* R. Pereira et al.

Anonymous Referee #2

Received and published: 3 March 2016

R. Pereira et al. present data on the effect of surfactants on the gas transfer velocity between the ocean and atmosphere. This subject is poorly understood, but essential for the understanding of air-sea gas exchange of climate-relevant gases. The study utilizes a laboratory gas exchange tank designed by the same group. The tank allows comparing gas transfer velocity under artificial turbulence, but under in situ turbulence as acknowledged by the authors. It still allows investigating fundamental processes. However, the discussion of the results has to be elaborated, also in terms of the literature.

However, some corrections and suggestions are designated for the authors' attention; otherwise I recommend the manuscript as suitable for publication. Overall, prior pub-

Printer-friendly version

Discussion paper

lication some sections of the manuscripts have to be rewritten, especially section of Results and Discussion. For example, the authors need to discuss their observations with the available literature on the chemical composition of the SML.

Abstract I agree with the comments of reviewer #1 that the abstract is too much based on results without outlining the main conclusions.

Introduction L35, P1: Under slick conditions, e.g. wave-damped water surfaces, surfactant activities are probably high enough to form a dense layer acting as a barrier. However, under non-slick conditions, still under influence of surfactants, I believe passing of gases through the air-sea interface is controlled by slow diffusion-driven transfer, not because the interface is a barrier.

I also feel that the introduction misses to describe biological properties of the air-sea interface (or sea surface microlayer) as microbes could be a direct source of surfactants.

The authors miss also to describe the purpose and aim of the study. It should be added as a final paragraph to this section describing overall objectives and hypothesis.

Materials and Methods L46, P2: I assume for the field work a medium-sized vessel was required, and I am wondering how the authors can be sure to collect the SML free of disturbance and contamination inevitably caused by the vessel. L61, P2: Not clear if CDOM was measured in both SML and SSW, or only in SSW. How were the samples filtered? L82, P3: What is the range of applied turbulence? How is it measured? As TKE? L84, P3: I got the meaning, but an odd sentence hard to grasp.

Results and Discussion P111, P3: Temporal versus spatial variability are poorly presented and discussed, and, as also mentioned by Reviewer #1, opposite statement appears in abstract. Reference to a figure would make it clearer. P115, P3: Provide regression plot. P value exact 0.001? As expected from . . .citation? P116, P3: provide standard deviation for the range of EF.

Printer-friendly version

Discussion paper

CC BY

P122, P4: How do the authors define “high spatial and temporal variability”? Giving the range, doesn’t provide any information about variability. P133,P4: The relationship for SML is not strong (r2=0.45) and even not significant (p=0.06). This misinterpretation have to be corrected. P137,P4: Lichterfeld et al. 2013 report finding about HMW DOM in SML. Have to be discussed here. P151, P4: There is plenty of literature available (Lichterfeld et al., 2013, work by Carlson, work by Frew) supporting the observation. Also known enrichment of TEP supports the idea of a HMW matrix in the SML (see Wurl et al. and Cunliffe et al.)

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-7, 2016.

Printer-friendly version

Discussion paper

CC BY