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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 #1

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Review on ms. Bg-2016-7: "Surfactant control of gas transfer velocity along an offshore coastal transect: results from a laboratory gas exchange tank" by R. Pereira, K. Schneider-Zapp and R. C. Upstill-Goddard

General comments: The manuscript deals with a current and interesting topic targeting to contribute to the understanding of surfactant control on gas transfer velocity across air-sea boundary. This may add to the knowledge on the role of oceans in the climate changes. The author collected sea surface microlayer and subsurface water and measured surfactant activity and coloured dissolved organic matter. Using a custom-designed air-sea gas exchange tank the authors evaluated corresponding values of the gas transfer velocity. To my opinion Abstract and Results and discussion section should be rewritten. Abstract reads as compiled Result section. It should be rewritten or expanded with the main conclusions. Results and discussion also give a lot of results with too short discussion. The aims are poorly written. Therefore I suggest major revision along comments listed below. Specific comments: Abstract: P1, l 15: for nonprofessional photochemist it is not clear what does it mean: k660 (kw for CO_2; freshwater; 20oC). Is it CO_2 or CH_4? P1, l 19-20: I do not understand how such variability should be taken into account when evaluating marine trace gas sources.

Introduction p1, l 32: surfactants are organics as well p 2, l 41-43: The aims are poorly written and must be rewritten. The aims should be written as hypotheses which are tested in the paper. Why CDOM was measured? Part of aims was written on p3, l 80-81.

Materials and Methods p2, l 48: I am not sure if triplicate sampling is necessary as sampling is time consuming while both SML and SSW are not in a steady-state. p2, l 64: Are CDOM measurements performed in the filtered samples? If not which is the influence of the particles (living and non-living) on the measured data? p3, l 79: Applied turbulence settings should be listed here.

Results and Discussion p3 l 111: While the authors state here: "For all four parameters temporal variability generally exceeded spatial variability" in the Abstract the statement is opposite: "Spatial SA variability exceeded its temporal variability."??? Those are opposite statements. P4, l 116: If the authors suggest that there was expected relationship between SA in the SML and SSW, it should be cited. p4, L 136: I do not understand why the authors explicitly discuss DOM as SML is always enriched in POM of non-living and living origin. Are the measurements carried out on filtered samples? It should be pointed out in the Methods section. p4, l 144: Surfactant activity of autochtonous origin can be very high during bloom period. The authors may check Chl a data for sampling dates from the satellite observations. P4, l 146: I suggest saying lower molecular weight marine CDOM, than LMW marine CDOM. The authors do not know on molecular weight of marine CDOM, apart from well-known fact that terrestrial humics are of higher molecular weight than marine humics. P 6, L 200-202: The authors state: "the observed spatio-temporal variation in R660 and its relationship with SA (Fig. 3) is a consequence of compositional differences in the surfactant fraction of the SML DOM pool". Unfortunately throughout Results and Discussion section it is not discussed in such way. I suggest improving this section by discussing straightforward the influence of surfactant having different composition on R660. p6, l 222: Pity that the authors did not measured DOC. These measurements would give information on the hydrophobicity/hydrophilicity of surfactants (high SA and low DOC-hydrophobic surfactants; high SA and high DOC-hydrophilic surfactants).

Technical corrections: The authors should be consistent in writing: sub-surface water or subsurface water P1, l 7: I suggest adding full name of CDOM P 2, l 46-47: I suggest removing Table 1 and adding coordinates into Fig. 1. p5, l 184: Twice written "at the" p5, l188: October is Autumn and not Winter. Table 2: I suggest adding borders between different sampling dates to allow easier data comparison. It should be defined what is total CDOM absorbance (250-450 nm). Is it integration or is it average value over the whole 250-450 nm spectra? The same data are given both in the Table 2 and Fig. 2, with more data (S 279-295, S 350-400, Salinity) given in Table 2. I suggest that the authors decide on how to present data, in figure or table. Personally I prefer data given in Figs. than in tables. Similar situation is with Table 3 and Fig. 3. Table 2: S275–295, S350–400 and salinity are not listed in the table caption. Table 3. Three different turbulence settings should be listed in the caption. p4, l 121: comma to be removed Figure 1. It would be good to draw Blyth River into this fig. Figures: I suggest lines and symbols to be presented in different colours for sampling dates. This would significantly increase the clearness.

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