

## ***Interactive comment on “A measurement system for vertical seawater profiles close to the air/sea interface” by Richard P. Sims et al.***

### **Anonymous Referee #2**

Received and published: 13 April 2017

General comments: The authors presented a new sampling technique to sample and measure the vertical profiles of physical and chemical parameters in the subsurface layer of the ocean (<10 m). This indeed improves the very surface layer sampling for chemical tracers. The paper is overall well written with the exception of a few confusing sentences. Some of the figures present redundant information, and could be removed. However, the main problem of the paper, I feel, is lack of discussion on how the whole technique may impact the estimation of air-sea exchange. Does it really matter that we need to sample the very subsurface layer (e.g., ~1 m) of the seawater to obtain a very accurate flux estimate? Or it may be good enough to sample 4 m below the sea surface as the traditional sampling system? Another issue is that, how much is the stratification in the surface layer due to physical processes? How much can we correct these effects with relatively inert tracer, e.g. CFC-11, for estimating air-sea fluxes? If this correction

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is accurate enough, then do you really need to sample the very subsurface layer?

Specific comments: Pg2, lines 22 – 30: Authors discussed near surface stratification due to certain physical processes here. It is worth to note and mention that, people have been using CFC-11 to correct physical effect on estimated air-sea fluxes (see Lobert et al., 1995; Butler et al., 2016; Yvon-lewis et al., 2004; Hu et al., 2013).

Pg12, lines 7 – 9: “The delay between a bucket switch and CO<sub>2</sub> change in the Licor was timed at 138s. . .”. This is confusing. Does this delay include CO<sub>2</sub> response time in the equilibrator and the time from sampling to the equilibrator? If it is, define it. Also, this sentence seems out of place here.

Pg12, lines 21 – 25: list equations to be clearer.

Pg 12, lines 26 – 28: redundant information.

Fig. 5: I don't think this figure provides extra information or value other than the sentence described in Pg 11, line21. So, you may consider remove it.

Figs. 6 & 7: Redundant. Recommend to remove fig. 6. Also, fig. 7 looks a little messy with depth contours.

Pg. 16, line 18: Density was not used in the later discussion. I am not sure why you want to mention and discuss density profiles here. Consider remove it from the text and figure.

Pg. 17, lines 3 – 5: why do fco<sub>2</sub> profiles show the largest difference in the surface, and not in the layer where the temperature showed the largest difference (4.5 – 2 m below water)? The explanation given by the authors is not convincing. Since the amount of co<sub>2</sub> outgassing due to surface seawater warming (during upcast) can be calculated using temperature, salinity and solution of CO<sub>2</sub>, it is not hard to estimate how much the difference between downcast and upcast was due to physical effect, and how much was due to biological influences.

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Pg. 17, lines 17 – 19: 3 uatm seems a huge difference considering the variation of  $f_{CO_2}$  observed in the subsurface layer. Did the authors consider the different response time in two different equilibrators? Why are their measured values so different?

Pg. 18, lines 15 – 16: confusing. To me, the increased fluorescence is likely due to phytoplankton located at the bottom or below the subsurface layer.

Figs. 9 and 10: redundant information. Consider remove fig. 9

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Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2017-7, 2017.