

## ***Interactive comment on “The influence of transformed Reynolds number limitation on gas transfer parameterizations and global DMS and CO<sub>2</sub> fluxes” by Alexander Zavarsky and Christa A. Marandino***

**M. Yang (Referee)**

miya@pml.ac.uk

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This paper looks at the implications of air flow separation on previously published gas transfer velocities as well as on the global oceanic CO<sub>2</sub> and DMS fluxes. The paper relies heavily on the recently published ZA18 from the same group, which argues that when wind and waves are aligned the leeside of the wave is sheltered from the wind and encounters less turbulence. This theory was used by ZA18 to explain the fairly low transfer velocities of DMS and CO<sub>2</sub> during a recent Indian Ocean cruise at wind speeds over 10 m/s. The first reviewer has already given a detailed review, pointing out

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some mathematical inaccuracies. The authors have supplied a new, revised version of the manuscript. It is this revised version that I will comment on.

Overall, I find this paper still very confusing and the main results self-contradictory. The abstract states that corrections of Nightingale et al 2000 and Wanninkhof 2014 for air flow separation leads to INCREASES in the gas transfer velocity. However, applications of these corrected  $k$  parameterizations led to  $\sim 10\%$  DECREASES in the global flux magnitudes. This doesn't make any sense from a superficial level. Figures 2 and 3 show that correcting for air flow separation moves wind speed to the left (i.e.  $U_{alt} < U_{10}$  when there's suppression), while  $k$  remains unchanged. Figure 6 (left panel) shows that correcting for air flow separation moves the global wind speed distribution to the left. Is it this adjustment in wind (rather than an adjustment to  $k$ ) that causes the global fluxes to reduce in magnitude? But then Figure 4 and 5 show that the actual  $k$  values (the individual dual tracer points) are adjusted upwards (rather than the wind speeds adjusted leftwards) for the Nightingale et al 2000 data. How? Not clear. Seems to me that there are two possible philosophical approaches: a)  $U_{10}$  is a good predictor of  $k$ , and thus points below the expected mean  $k$  vs.  $U_{10}$  relationship (suppressed data) should be adjusted upwards in terms of  $k$ , or b)  $U_{10}$  is not a good predictor of  $k$  as waves are also important; thus we either need a new  $x$  variable that includes wave-wind interaction, or adjust  $U_{10}$  to account for waves as the authors have done in the  $U_{alt}$  calculation. But in this paper the authors seem to be taking both of these approaches.

The bottom line is that the authors are trying to address an interesting and important topic. Unfortunately I find the paper far from publishable currently. And so I recommend a major revision and give the authors a chance to clearly address the issues raised.

Comments:

At a given wind speed, there is probably a range of gas transfer velocities as a function of sea state. Recent works from Blomquist et al 2017 and Brumer et al 2017

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demonstrate the efficacy of the 'wave-wind Reynolds number'. Some of the variability in sea state may be encapsulated by the authors' transformed Reynolds number, fine. At expected times of gas transfer suppression, the authors decided to adjust the wind speed (U10) downwards to a transformed wind speed (Ualt) by using a threshold in the transformed Reynolds number. I think this binary treatment (i.e. either suppressed or not suppressed, instead of varying degrees of sea state effect) is overly simplistic. What is the quantitative reasoning for adjusting wind speed downwards to the threshold REtr value in the case of suppression? Why not adjusting to an even lower |REtr| value, for example? And could there be times when k is 'enhanced' relative to the mean relationship?

For DMS (Figures 2 and 3), k is simply shifted to the left due to the U10 to Ualt correction. However, Figure 5 shows that the dual tracer k values from Nightingale et al 2000 are actually shifted upwards, while wind speed remains unchanged. It looks like R2 is worse in Figure 5 than in Figure 4. How did the authors make this latter correction (Eq. 14?) and why the inconsistency in approach? The authors did not apply the U10 to Ualt correction to the N00 data, as with DMS, because N00 data are more affected by bubbles? Also, the authors implied that the N00 dataset were taken in places (many coastal) and during times when gas transfer suppression is predicted to happen more often than the global average. Following that logic, shouldn't the global fluxes be higher, and not lower, if the original N00 contained a lot of suppressed gas transfer data?

In the case of W14, it is a single global average point averaged over multiple years. It presumably does include the full range of sea states. This single k point is pinned against a global mean wind speed (accounting for wind distribution). So if the functionality of W14 is correct, I don't see how it needs to be corrected at all to account for air flow separation. Does the right panel of Fig 6 imply an upward adjustment in the k value, or a leftward adjustment in wind speed? Shouldn't fluxes computed from [original W14 x NCEP wind speed distribution] be the same as those computed from

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[adjusted W14 x corrected wind speed distribution]? It's worth noting that in the revised wind distribution, there is far more occurrence of 'zero wind speed', which in the W14 formulation would result in zero flux. Are the authors saying that under conditions of moderate-to-high wind speed, when wind and waves follow each other, there is no gas transfer?

Some technical comments: Your Eq. 1 is presented from the perspective of air concentrations. Since you're talking about water-side controlled gases, it seems more appropriate to present this Eq. from the perspective of water concentrations, i.e.  $k * (C_a * H - C_w)$  or  $k * (C_a / H - C_w)$ , depending on whether your H is water to air or air to water. Also, your Eq. 1 adopts the convention of positive flux into the ocean. That's consistent in sign to your global CO2 flux, but not to your DMS flux. Please be consistent.

Eq. 3: I think you have left out the H term. Should be  $1/ktot = 1/kw + H/ka$  (if H is water to air)

In many of the plots, I think it's misleading to call Ualt 'wind speed' on the x-axis, and have both k vs U10 and k vs Ualt on the same plot.

Figures 9 and 10. Which original parameterization is used? Please specify in the captions.

Finally, the results from ZA18 are heavily used in this current paper. While the presented argument of air flow separation is a neat theory, I don't think it's well backed up by the observations for at least three reasons: 1) It is conceivable that transfer velocity varies with the directional difference between wind and wave as well as with the relative wind velocity relative to the wave phase speed. However, I don't understand the directional dependence in the formulation of the transformed Reynolds number. Air flow separation and sheltering are argued to occur when wind and waves are aligned (and not occur when they are orthogonal). However,  $\cos(0) = 1$  and  $\cos(90) = 0$ . And it is a low transformed Reynolds number that is argued to cause a suppression (or limitation) in gas transfer. This seems contradictory. 2) in ZA18, the authors attempted to

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explain previous transfer velocity datasets with the flow separation theory, but did not use actual (in situ or modeled) wave data. This is a significant shortcoming in my view. I have the ECMWF wave and in situ wind data from those cruises. It is not obvious that waves from cruises when gas transfer suppression were observed differed obviously from the waves during other cruises. The authors are welcomed to contact me and use these data to further improve their work. 3) a lot of high points and noise in the kDMS and kCO<sub>2</sub> data occurred when the delta C were very small. Not only are fluxes very noisy under these conditions, any small bias in delta C would also significantly affect the derived k. Is there still a noticeable 'suppression' if the authors remove these low delta C points?

These last comments are not criticisms of the ACPD paper, but partly explain why I find the current paper rather unconvincing.

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