Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2019-72-RC2, 2019

Ocean Science
Discussions

Interactive comment on "Air-sea gas exchange at hurricane wind speeds" *by* Kerstin E. Krall and Bernd Jähne

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Received and published: 21 August 2019

This paper is a description of an analysis of water- wind tunnel measurements of the rate of transfer of trace gases. Twelve gases are uses and the simulations are for very strong winds – crudely equivalent to 10-m atmospheric wind speeds up to 80 m/s. The results are expressed as a parameterization of water-side gas transfer velocity, kw, in terms of the water-side friction velocity, u*w. The authors use the breadth of the solubility of the gases to separate the so-called direct or free surface (air-water molecular scale) transfer to two bubble mediated mechanisms which they refer to a 'surface area' and 'volume flux' mechanisms. Their main finding – that bubble transfer is negligible compared to the direct transfer for all gases up to wind speeds of 30 m/s – is surprising (at least to this reviewer). For example, the COARE gas transfer

algorithm (Fairall et al. 2011; Blomquist et al. 2017), which is based on Woolf's 1997 bubble parameterization gives bubble and direct transfer approximately equal at a wind speed of 8-10 m/s. The authors discuss this in their section 4.6 and conclude the difference in CO2 and DMS transfer velocity observed by Blomquist et al cannot be due to bubbles. Certainly an amusing controversy that is worth airing. The paper is fairly well written and the authors are certainly experts in the field. They try conscientiously to go the extra mile in explaining the differences in free surface and bubble transfer mechanisms and even provide information on possible sea spray transfer (although that cannot be assessed with their measurements). The paper is, with a few exceptions, well referenced. While the overall implications for open ocean gas transfer are open to interpretation, the actual wind tunnel measurements appear to be solid. So, in my view the paper should be published with some clarifications and improvements. The authors' conclusions are difficult to reconcile with open ocean measurements but I leave it to them to consider how to handle that. In my opinion they are too dismissive of the large number of experimental (tracers and eddy covariance) studies that indicate insoluble gases and CO2 have substantially higher transfer velocity above 15 m/s. I question that the Zavarsky paper is sufficient cover.

I think the paper would benefit from more careful consideration of mixing actual measurements, assumptions, and inferences. I don't understand why they used Powell's open ocean estimates of Cd when they could have used Donelan's 2004 results actually determined in the Miami wind tunnel. Also, please give us a sentence explaining Takagadi's method for getting friction velocity so we don't have to go look it up. I am guessing they assumed a momentum balance at the interface to compute u*w from u*a (square root of ratio of air to water density). This assumes that the growth of the wave field has negligible effect of the momentum balance. Is this right? Also, they switch back and forth between u*w and just u* - I assume they mean the same thing. Just be advised that it is quite a stretch from a setting on an instrument dial to actual waterside friction velocity. Another example, McNeil and D'Asaro (2007) did not 'measure' gas transfer velocities, they inferred them from water concentration measurements. Furthermore, their basic assumption in the analysis is that both free surface and bubble transfer velocities scale with u*w and nothing else. However, there is considerable evidence that the air volume flux from breaking scales as u* and other wave parameters (see Deike et al. 2017). Also note that Deike and Melville (2018) used this approach to estimate kb for DMS and CO2 – treating both gases as highly soluble. They present measurements of bubble area from the wind tunnels but no estimates of volume flux. I think the bubble volume flux data should appear in Fig. 7c. Finally, the authors might note that Rhee et al. 2007 found considerable enhancement of k for insoluble gases when bubbles were introduced.

Page 2 line 19. U*a is a measure for momentum extracted from the wind. Some of that momentum is realized in the ocean via direct viscous transfer at the interface, some goes into growing waves and some is later realized as turbulence when waves break. If locally waves are in dynamic balance, then the momentum flux from the air is the same as the momentum flux realized in the ocean. So, how close is the balance in a wind tunnel?

Page 3 line 12. Bubbles may also suppress turbulence through density stratification.

Eq (5) This terminology is confusing with the un-numbered equation on Page 2 line 14.

Page 4 line 1 Suggest referencing bubble model work of Liang et al. GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 27, 894–905, doi:10.1002/gbc.20080, 2013 and earlier work.

Page 5 Eq (7) I am confused by the terminology. Can Qb be volume flux and Qb/As also be a volume flux? If we equate kr with the volume of air ingested per unit area per unit time (units velocity), then that should be on the order of 30 cm/hr at u10=15 m/s and u*w=2 cm/s (see, Deike et al, 2017). That does not compare well with Fig. 7 c, where kr doesn't reach those values until u*w is greater than 10.

Page 5 Eq (10) This equation is similar to Woolf parameterization. For the volume flux

is kr=2450*whitecap fraction (cm/h) and the parameter e=kc/kr*sqrt(600). How does this compare to your results?

Page 7 section 2.3. This discussion of droplet effects is a little confusing. It seems to me that ejecting a droplet does not change the waterside concentration, so their measurement method does not capture it. If the drop has time, it would transfer gas to the air and that would reduce free surface transfer further down the line. Is that what they are trying to say? This argument about time scales ignores the fact that the droplets leave the wind tunnel before they can do much transferring – this is discussed in Andreas and Mahrt 2016.

The discussion of field measurements of k for DMS and CO2 is very useful (Section 4.5). It also illustrates the rather inconclusive state of field observations. The data from Zavarsky et al (2018) show essentially no difference between CO2 and DMS. The analysis of Fairall et al. (2011) which compiled all the direct flux data to date showed significant differences. The HIWINGS data shown in this figure are quite surprising for CO2. Blomquist gives k CO2 a power dependence of u10ˆ1.68, which is not linear. Because of the conditions, I don't think the HIWINGS data below U10 of 10 m/s should be considered. Even ancient information such as Wanninkhoff's famous formula indicate a quadratic wind speed dependence for insoluble gases. Earlier suggestions that k CO2 should scale as Uˆ3 were based on the assumption that whitecap fraction scaled as Uˆ3. More recent observations have shown that this is not the case, with much weaker wind speed dependence at high wind speeds. I think the authors may be placing too much importance on Zavarsky. From Wanninkhoff's radioactive tracers, to a number of deliberate tracer studies, and perhaps all other eddy flux measurements CO2 goes at least quadratic with wind speed. At u10=18 m/s, the value is close to 100 cm/h for open ocean measurements.

Figure 10. What drag coefficient is used for the curve shown for modeled DMS and CO2?

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