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Discussions

Interactive comment on "First air–sea gas exchange laboratory study at hurricane wind speeds" *by* K. E. Krall et al.

Anonymous Referee #2

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This is a very interesting paper although I think it could be improved by adding some further analysis. Overall, there is merit in gas transfer studies performed in wind-wave tunnels since the fundamental processes that affect the gas flux are the same in a wind tunnel and in the ocean. The problem is that there are issues in scaling results from a wind tunnel to the open ocean, and these scale issues likely become more important at higher wind speeds where the wave field in a wind tunnel cannot recreate the wave field in the ocean. Therefore, when analyzing the results from these laboratory measurements it is important to provide evidence that what was measured has relevance to the open ocean. This is especially relevant since most criticisms of bubble gas transfer studies done in the laboratory are that they do not well simulate deep injection of bubbles and the resulting bubble dissolution flux pathway.

The experiments themselves are characteristic of what is expected from Prof. Jaehne's group at the University of Heidelberg and with the exception of one detail that I feel is important (see below) there is no reason to think that the data are in error. However, the authors only briefly mention previous work on gas exchange in the presence of bubbles, as if it had no relevance to their measurements. This, in my opinion, might be an error on their part since they talk only vaguely about the enhancement of k for the lower solubility gas (HFB) versus k for the higher solubility gas (DFB), and do not really discuss whether or not this "enhancement" is consistent with previous work. The authors almost seem to have a bias against discussing any of this previous work, except to dismiss it in passing as being irrelevant.

The authors claim Keeling's 1993 paper as being an "empirical parameterization" but that is not my impression of how Keeling derived his large-bubble functional form for k. It was based mostly on an analysis of fluxes to individual bubbles and then measured bubble size distributions (although my memory of the paper is somewhat hazy at this point). Asher et al. (1996) did assign empirically derived coefficients to the Keeling functional form, but the point is that it might be instructive to at least see if those coefficients with Keeling's function reproduce the observed enhancement of k. (I did the calculation quickly, but using the coefficients from Asher et al. (1996) for evasion in Keeling's model I find that the ratio of k(DFB) to k(HFB) is 0.67 whereas Krall and Jaehne (this manuscript) measured it to be 0.7. This agreement might just be happenstance, but it might also be that the previous work could help stimulate more detailed analysis of what is going on in terms of which gas transfer pathways are dominant in the high-wind wind-wave tunnel.

There is also a possible issue in their gas analysis method, although the authors do not provide enough details to determine if there is a problem or not. The measured values for k imply an e-folding time for gas transfer that is very fast, something on order of 100-200 seconds at the highest wind speed given the depth of the tank. It is not clear that the gas equilibration method has a response time that would be fast enough to measure gas concentration changes on the same timescale. If the measurement time is lagging the actual concentration changes, there would be biases introduced into k that would be wind-speed dependent, and this might explain the "regime change" observed in the data. I admit I am not certain this is not a problem, but most equilibrators have characteristic response times on order of 5 minutes (e.g., Loose et al, 2009, Water Resources Research) which would imply problems with the measurement methodology at high wind speeds. There needs to be some evidence presented in the manuscript in the form of plots showing the step-function time response of the gas measurement system (including the equilibrator), and not just assertions, that the response time was fast enough to adequately resolve concentration changes in the wind-wave tunnel.

My recommendation is that the paper be returned to the authors for "major revisions" to address the two main issues discussed above before being accepted.

Minor Points:

Page 1973, Line 10: Also should point out there are issues in working in wind-wave tunnels.

Page 1973, Line 13-14: Better way to say it is that k and the concentration difference describe the flux, which defines gas transfer across the air-sea boundary. Saying the flux describes the gas transfer is of redundant. The flux is the gas transfer.

Page 1975, Lines 3-5: kb is not usually parameterized in terms of the whitecap fraction. Its contribution to the total transfer velocity is scaled using the whitecap fraction. kb is parameterized in terms of scale factors, diffusivity, and solubility. This is

not semantics.

Figure 1: Not correct to extend the McN2007 line below 7 m/s. (McN d'A do not in their paper (see inset to their Figure 3).) Even extending their parameterization (as they do) is extrapolating to lower wind speeds using the bomb-C14 gas transfer number, which is a globally averaged value. Aside from that, there is a problem reconciling the functional form for McN2007 with Eq. 2 in the text.

McNeil and d'Asaro 2007 and a few other references: There appears to be a number of extraneous numbers in the citation, as though random years are being added to the citation. Was something misconfigured in the citation manager?

Interactive comment on Ocean Sci. Discuss., 10, 1971, 2013.