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

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We thank anonymous referee 2 for the extensive comments.

Referee comment: 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.

Response: Thanks for this hint, we added the following two paragraphs discussing these issues at the end of the introduction:

Up to now, no gas exchange measurements were performed in wind-waves tanks at free-stream velocities larger than 20 m s^{-1}. The highest gas transfer velocity measured in fresh water is 180 cm h^{-1} at a wind speed close to 20 m s^{-1} (Komori and Shimada, 1986). Higher gas transfer velocities were only measured during the WABEX-93 experiment (Asher et al., 1995) in a fresh water surf pool without wind but with breaking waves. The highest measured gas transfer velocity in a laboratory, corrected to a Schmidt number of 600 measured at a fractional whitecap coverage of 0.067 was 450 cm h^{-1} (Wanninkhof et al., 1995).

In late 2010 the first high-speed wind wave facility became available at Kyoto University with free-stream wind speeds larger than 40 m s^{-1}, opening up new experimental opportunities in the laboratory. It remains an open question, however, whether high wind speed conditions can adequately be simulated in laboratory facilities. This concerns mainly the spatial scale of breaking waves and the deep injection of bubbles with the resulting bubble dissolution flux pathway. Therefore it makes sense to perform first a pilot study with limited effort to explore the feasibility of such experiments. The results of such a pilot study are reported in this paper.

Referee comment: 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.

Response: The authors are well aware of the valuable previous work. However, having performed measurements with only two tracers with rather high and very similar solubilities, similar Schmidt numbers and no bubble or whitecap measurements available for the presented study, all statements would be highly speculative and thus give no really relevant information.

We added several paragraphs in sections 5.1 and 5.2 to further explain and proof this in detail:

[the agreement between McNeil and D'Asaro (2007) and our data] does not mean that it is possible to transfer these laboratory data directly to the field. The conditions are too different. Fresh water was used instead of sea water. The scales of the short-fetch waves in the laboratory are much smaller than at sea, deep injection of bubbles and the resulting bubble dissolution flux pathway does not occur with a mean water depth of only 0.8 m.

One important conclusion can be drawn nevertheless. If a dominant pathway for gas transfer induced either by enhanced turbulence due to wave breaking, the enlarged surface area or mediated by bubbles would be missing in the laboratory experiment, the gas transfer velocity measured in the laboratory would be significantly smaller than that measured in the field. This is obviously not the case. More specifically, this means that the bubble dissolution flux pathway cannot be the dominant mechanism at these high wind speeds.

The tracers used in this pilot study span only a small fraction of the Schmidt number – solubility parameter space, (Fig. 8). Especially gases with low solubilites are missing, where bubble-mediated gas transfer can be expected to be even higher. In addition no bubble density spectra or whitecap coverage parameters are available. Therefore, a more detailed analysis makes no sense and is omitted in this paper. With such limited data, any model on bubble-mediated gas transfer can be fitted to the data (Krall (2013)) with the result that no conclusive statements are possible. Especially any extrapolation to a gas with lower or higher solubility than the tracers used in this study is highly speculative and very likely incorrect.

Referee comment: The authors claim Keeling's 1993 paper as being an "empirical parameterization" but that is not my impression [...]

Response: Correct, Keelings approach is not empirical. However, the word "empiri-

cal" did not appear in the sentence where we cited Keelings papers, but in the next sentence. Still, a misunderstanding is possible. Therefore we removed the word "empirical".

Referee comment: 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.

Response: See comment to previous paragraph. In addition it must be added that there are NO other lab data available with wind speeds higher than 20 m/s.

Referee Comment: 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. [...]

Response: This estimate of 100-200 seconds is wrong. The highest measured transfer velocity (not the value extrapolated to a Schmidt number of 600) was about 720 cm/h. With a water height of 80 cm, the lowest measured e-folding time for gas transfer was around 6.7 min. Due to the high inner surface of 1.8 m^2, the gas equilibrator used is faster than the one used by Loose et.al, 2009 (internal surface 0.54 m^2). The measured equilibrium time constant with hexafluorobenzene was (see Krall 2013, pp 56-57) 1.2-1.3 min, making the equilibrator five times faster than the fastest gas exchange time constant in the facility.

Referee Comment: Page 1973, Line 10: Also should point out there are issues in working in windwave tunnels.

Response: A sentence was added to address these issues

Referee Comment: 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.

Response: Wording corrected.

Referee comment: 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.

Response: Wording corrected.

Referee comment: Figure 1: Not correct to extend the McN2007 line below 7 m/s.

Response: Corrected. Fig. 5, also showing the McN2007 parameterization, was corrected as well.

Referee Comment: 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?

Response: The extra numbers are the page numbers where the corresponding references are cited. We have no idea how they came into the paper, because they do not appear when we generate the text. We will resolve this issue with Copernicus Publications' editorial office.

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