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# *Interactive comment on* "Air/sea DMS gas transfer in the North Atlantic: evidence for limited interfacial gas exchange at high wind speed" *by* T. G. Bell et al.

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We thank both reviewers for their positive and constructive comments. We address their specific points below:

Reviewer 1: A) Due to low wind speeds, results from ST181 seem consistent with either trend in Fig 4. They are presumably grouped with the ST191 results (pg.13300, line26) on the basis of wave age, but this may be due to the way wave age is computed (more below on that).

The data from ST181 could be consistent with either trend. We have rephrased the



sentence to: "Most notably, ST184 and ST187 define a trend line with a slope (k/U) roughly twice that defined by the data from ST191."

B) The method for computing C_D10 and C_H10 in Section 3.3 and Fig 5 is not given. From the distribution of scatter in Fig 5 it appears to be computed from sqrt(<w'u'>ËĘ2 + <w'v'>ËĘ2), that is, from both streamwise and crosswind stress components. While technically correct, this approach has the drawback of amplifying noise (the crosswind component is mostly noise) and biasing scatter in the result; because the sqrt is computed, negative values which might arise from normal variance in the measurements are excluded. An alternate approach might be to use the streamwise component alone, <w'u'>. For example see Fig A1 in Fairall et al. J.Climate, 16, 571, 2003, or Fig 3 in Fairall et al. JGR, 111, D23S20, 2006.

The C_D10 and C_H10 results are calculated using the streamwise approach (i.e. rotation of winds so that the horizontal component is all in u before considering the covariation with the vertical component). We suspect that the cause of greater scatter is that our data are only averaged over 10 min periods whereas the data in Fairall et al 2003 are 1 hr averages. Considering this, the agreement between the data and COARE is reasonable.

C) i) The use of wave age is complicated by several definitions for this term. The authors should provide more detail on how wave age is computed. In the development of wave & wind stress theory, wave age is usually defined with respect to pure wind seas – that is, excluding swell. See Drennan et al. JGR, 108, 8062, 2003. I suspect large wave ages shown in Fig.6 are influenced by swell. This may be why there seems to be a poor correlation between wave age and suppression of k_dms. In this study it may be difficult to compute a wave age excluding swell, which limits its usefulness.

The computation of wave age is given in Lines 418-421 and it is noted that this includes the swell component of the wave spectrum. We agree that the wave age of the pure wind sea would be a very useful future measurement.

ii) Because wave ages around 1 or less than 1 are of most interest, it might also be better to correlate to inverse wave age, as in Drennan 2003. Also, in Fig6 for ST191 the model computed wave height is considerably greater than the observed Hs. If the model is correct, this implies a young sea state, but the wave age is about 1??

Figure 6 has been changed to show inverse wave age. The wave age for ST191 is about 1, which as currently defined does suggest a young, wind-driven sea.

D) i) The relationship between Ke, wave age and k_dms from the Soloviev 2007 model should be specified, and the authors should verify that their computed wave ages are appropriate for use in this model (see above comment). Also, there are a few other wave parameterizations which might be informative in Fig 8. The COARE model incorporates wave parameterizations from Taylor and Yelland 2001 and Oost et al. 2002. These models are by now somewhat out-of-date, but their inclusion might nevertheless be informative and the approach is empirical, in contrast to the more theoretical approach of Soloviev 2007.

Additional details have been added on the relationship between Ke, wave age and k in Supplementary Materials and referred to in the main text. We have adjusted the Soloviev 2007 model in Fig 8 to represent the mean wave ages from Stations 187 and 191 (U10n/CP = 0.36 and 0.89 respectively). We have decided not to include additional COARE wave model parameterizations in Figure 8 as these are for very young wave ages (U10/CP > 3; Taylor and Yelland, 2001).

E) i) It seems the same data are presented several times in subsequent figures and I'm not sure they are all necessary. The models in Fig3a might be added to Fig.8, for example. And I'm not sure about the significance of Fig7b. The correlation between whitecaps and suppressed k is probably coincidental.

We think it would be confusing to present wave-related model parameterizations early in the manuscript before this issue is fully discussed. We agree that the correlation with whitecaps is not causal, but feel that it is important to show the data.



ii) Bubbles have limited influence on k_dms but whitecap coverage tends to increase with wave height, and the correlation with wave height seems more fundamental.

We agree. The purpose of Fig 7b is not to suggest a link between bubbles and suppressed k. These data effectively rule out a bubble-driven amplification of the high k values at intermediate wind speeds.

Reviewer 2: A) It is a pity that directional wave spectra, surfactants and turbulence have not been measured during the campaign as this would be important data to verify the discussion of this manuscript.

We agree.

B) The effect of nonbreaking waves have been reported by [1] and a number of measurements have been conducted using thermographic techniques by different groups. Such efforts could be mentioned in the manuscript.

We have added a sentence on Line 69 to read: "There is evidence that waves can influence near surface turbulence (Saveleyev et al. 2012; Schnieders et al., 2013) and that the presence of swell may modify the roughness of wind-seas (Garcia-Nava et al., 2012; Garcia-Nava et al., 2009)."

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