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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.

Anonymous Referee #1

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The paper presents an interesting data set showing evidence of the influence of sea state on gas transfer. This is a complex and important topic with a limited observational record, so this report is a welcome addition. Direct flux measurements of higher solubility gases clearly provide insight into the surface stress related component of gas exchange, in isolation from the effects of bubbles.

The data clearly show a reduction in transfer velocity related to sea state which is not considered in empirical gas transfer formulations. The results of this work support the conclusion from other studies that for DMS and other mildly soluable gases, power-law gas transfer models are of limited use. The suppression of DMS transfer velocity was mentioned in at least one prior report (Vlahos 2010), although I agree the explanation offered there is less plausible.

The observation that sensible heat flux is not similarly suppressed is reasonable given the air-side dependence of heat transfer resistance.

A few comments on the presentation of the data:

1) Due to low wind speeds, results from ST181 seem consisent 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).

2) 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’>ˆ2 + <w’v’>ˆ2), 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 Fig3 in Fairall et al. JGR, 111, D23S20, 2006.

3) 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 supression of k_dms. In this study it may be difficult to compute a wave age excluding swell, which limits it’s usefulness. 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

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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??

4) 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.

5) 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 whitcaps and suppressed k is probably coincidental. 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.

Overall, a nice piece of work which raises interesting and relevant issues.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 13285, 2013.