

Interactive comment on “The effects of experimental uncertainty in parameterizing air-sea gas exchange using tracer experiment data” by W. E. Asher

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Review of biogeosciences acpd-8-16693-2008 by W M. Asher

(I write this having already seen Rik Wanninkhof’s review; different from the ordinary review process where the referees don’t see each other’s comments before writing their own. I’ll mention where I have the same concerns as Rik.)

The basic idea of the paper is a good one, and I’m overall supportive of the interpretation leading to the main conclusion, that, in some experiments at least, a "major portion&" of the scatter around experimentally derived parameterizations of air- sea gas transfer is due to measurement error in many experiments. The paper is well-written

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and clear in its layout and style.

I have no problem with the analysis of the wind-wave facility measurements, which have comparatively large measurement errors on the actual gas concentrations (7% on He-3 for example). Asher shows that these lead to a substantial proportion of the scatter in the gas exchange estimates. I do however, have some reservations on the analysis of the ocean dual tracer experiments.

These, as Asher notes, generally have lower uncertainties on the measurement of gas concentrations (2% or thereabouts), but there is an additional uncertainty in the mixed layer depth that is not present in the wind-tunnel experiments, and this dominates in the calculations made in the present work. The assumption is made that the uncertainty on this depth is 20%. This may be a good assumption for experiments conducted in stratified waters where the mixed layer depth is defined by a pycnocline, which may change rapidly. However, it is I think unduly pessimistic for experiments made in well-mixed coastal environments. The ones with which I'm most familiar were those performed in the North Sea. For precisely the reason that it was important to define the mixed layer depth accurately, these were conducted in a region off the Dutch coast where the water column is mixed from top to bottom, so that the mixed layer is defined by the bathymetry, which was well-characterized and uniform. 20% is too high an estimate for the uncertainty in mixed layer depth in those experiments – it's more like 10%.

The general point then is that I would like to see included more discussion of how the main result of the paper changes with the uncertainty assigned to this parameter. This comes down to much the same criticism as Rik Wanninkhof makes: there needs to be more discussion on the effect of different error sources, and I would particularly single out the uncertainty in the mixed layer depth. The paper needs a more realistic analysis of whether 20% is correct. My guess is that it is realistic, perhaps even optimistic, for experiments done in stratified water columns, but not for shallow-water experiments.

Detailed specific comments P16694 line 19: it would be a good idea to provide ref-

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erences for the first few sentences in this section, thus for some of the "several new oceanic data sets", the "new information concerning the fundamental mechanisms of controlling air-water exchange" etc.

P16695 line 27: The reference for origin of the purposeful dual tracer method should be Watson et al 1991, not Wanninkhof et al 1993. (I hope my co-referee will concur!)

P16696 line 4 &"It can be argued that the overall dependence follows either a polynomial dependence or a segmented linear dependence with U." The same could be said for any set of data, no matter how well they constrained a given function, so this doesn't really say anything useful.

P16697 equation 3: the second "delta" is not well-defined. As written it could mean $\ln(\text{He}_2/\text{Sf}_2)$; $\ln(\text{He}_1/\text{Sf}_1)$ or $\ln((\text{He}_2-\text{He}_1)/(\text{Sf}_2-\text{Sf}_1))$, where the subscripts 1 and 2 denote the measurements made at the beginning and end of the interval Δt .

Also, the equation looks to me to be wrong: surely it should contain the atmospheric values of atmospheric He and sf6? The concentrations in the expression are actually the deviations from the values in equilibrium with the atmosphere, not the absolute concentrations.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16693, 2008.

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