Atmos. Chem. Phys. Discuss., 14, C10317–C10319, 2014
www.atmos-chem-phys-discuss.net/14/C10317/2014/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "DMS gas transfer coefficients from algal blooms in the Southern Ocean" *by* T. G. Bell et al.

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Received and published: 18 December 2014

I'm really happy to see this paper. By measuring both the DMS Flux and the DMS Interfacial Concentration Delta (essentially sea water DMS) on a small enough spatial (time) scale, the authors are opening the way to empirical studies of the impact of biological activity on the chemical and physical properties that control gas exchange. They measured DMS fluxes and sw concentrations on the same scale as the heterogeneity in phytoplankton blooms. They have yet to match a particular flux to the footprint which generated that flux and compute a transfer coefficient for that patch of a bloom, but they came close. Imagine the value of a dataset in which k could be derived for dozens of points in which the winds and temps were constant, but surfactant and biological

activity varied from patch to patch!

By steaming toward a bloom from downwind they measured the footprint offset as 2.5 km, considerably larger than the value predicted by a commonly used footprint model. In view of the high sensitivity of these models to their many assumptions, empirical data from fast swDMS and DMS fluxes is of great value. This was a significant achievement and a valuable contribution from this paper.

The huge variance in eddy covariance-derived k's (vs U) has always been frustrating, even though the bin-averages are well-behaved. This paper offers us an explanation for a portion of that variance, especially in productive regions: dividing the flux from a high-swDMS patch by the delta from a low sDMS patch will generate an unrealistically high apparent kDMS. The inverse will of course happen in low-swDMS patches, creating unrealistically low apparent kDMS's, and too wide a range in derived k values. This scatter would be reduced if we matched the flux and sw concentration to the same footprint, or if spatial/temporal averaging could bring the flux and concentration measurements onto suitably similar spatial scales. Longer averaging times might eliminate some of the extreme values in the SOAP data set. Many published flux measurements have been averaged to an hour before the computation of k's.

One lesson from this paper is certainly that one must exercise caution when making faster and faster geophysical measurements. Once the temporal or spatial scale of the measurement becomes smaller than that of the phenomenon under study, considerations like footprint offsets can produce unrealistic conclusions. k derived from a flux averaged over an hour divided by an hourly concentration difference would be more accurate than a 3 min k derived from a high flux from an active footprint, divided by a 3 min small concentration difference from sitting in a clean patch. Measurement speed can open one up to big mismatches, as well as big discoveries. These authors used their speed to derive new information about the flux footprint distance.

Lines 15-17, page 28459: The equilibrator time constant is never stated. Is the 3-4 min delay to the miniCIMS itself or (as stated) to the inlet of its equilibrator? From the experimental design I would think the former.

Page 28643, last 10 lines: You can't really see this point clearly in the last panel of Fig 2. Maybe put the red symbols in front? In some places it could be random scatter, just from eyeing the figure. Saying it's not is kind of absolute without some backing.

Page 28464, last 5 lines: Absolutely right! Hooking up the right flux and seawater concentration measurements is critical. Their mismatch is the source of much of the scatter in the k vs U diagram.

This is a very welcome paper. I strongly recommend its publication and commend its authors for doing the work.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 28453, 2014.