

Interactive comment on “Determination of oceanic ozone deposition by ship-borne eddy covariance flux measurements” by L. Bariteau et al.

Anonymous Referee #2

Received and published: 8 November 2009

General Comments: This paper presents the first open ocean ozone eddy correlation (EC) flux measurements. This is a very important step in understanding the controls on open ocean gas transfer. There is a great need for direct flux measurements of a range of trace gases that are influenced by different chemical, biological, and physical processes. The technique and data analysis are presented clearly and thoroughly. This paper should be published with minor corrections.

Specific Comments:

Pg. 1934, Abstract – Perhaps it would be nice to see the range of deposition velocities or fluxes measured using this technique in the abstract.

Pg. 1935, line 6 – It is stated that EC is the preferred method for the measurement of

C729

gas fluxes but there is a paucity of references to previously reported open ocean flux measurements.

Pg. 1937, line 23 – The sampling lines were treated prior to use. Is this to avoid ozone absorption on the walls? Does this treatment happen once before installation? Would this treatment be ok for a sampling period longer than those presented here? Would there be any background ozone blanks to worry about with this treatment? Are there any considerations of blanks in the line at all (maybe this is trivial for the measurements)?

Pg. 1938, line 14 – How often were calibrations performed? How were concentrations calculated (by interpolation, or by daily calibrations, or by taking the mean of all the calibrations)? The calibrations were very precise but there were some differences (e.g. DOY 206, what happened there?), which may contribute some uncertainty (scatter) in the flux numbers.

Pg. 1940, line 14-18 – Regarding motion correction, just wondering if the ozone detector feels the effect of the ship's motion?

Pg. 1942, line 3 – There is miswording in this sentence, “...transport through the sample though the...”.

Pg. 1944, line 2 – How often underway would these “puff tests” be performed? There could be slight changes in the tubing delay for other reasons, like pressure waves or pump characteristics, which may change the delay slightly. In Figure 7, when the cross correlation method is compared to the puff method, how close in time are the two measurements made? I am getting at the fact that each 10 minute data set could have a slightly different delay but you can't do a puff test for all the measurements. However, because changes in delay are stated to only have a 6% effect, these are more academic questions than practical ones.

Pg. 1945, line 1 – Since the puff tests did not show any significant change in the

C730

delay time before and after the filter was changed, do the authors still believe that the filter was the cause of the initial change in delay detected by cross correlation? What is the authors' best guess as to why the delay did not change after subsequent filter changes? It is possible that I am missing something here.

Pg. 1948, line 22-23 – Do the authors also assume that the low frequency portion of the spectrum is the same for both scalars or is this determined before the high frequency correction? The low frequency end of the cospectra could have some large features that could be very different for ozone than for temperature. It seems that non-stationarity of ozone is examined but is the low frequency end of the temperate cospectrum also considered?

Pg. 1949, line 2 and Fig. 9 – I am not sure why the authors chose 0.4 Hz as the cutoff. It appears that the cutoff is 0.3Hz based on the lines drawn on the plot. This would change their response time to 0.5s.

Pg. 1951, line 17 – The text reads Eq. 11 but I think it should say Eq. 12.

Pg. 1952, lines 2-12 – Did this filter account for all low frequency features in the 10 min cospectra?

Pg. 1952, line 23 - How was 8km determined to be open ocean?

Results – It would be nice to see some plots of F vs. U or F vs. atmospheric ozone concentration. A lot of text here was devoted to the deposition velocity, but since this paper is focusing on flux measurements some flux plots would be nice to see. Also, plots of wind direction or back trajectories would help to illustrate if the concentrations and fluxes were influenced more by land processes or by oceanic process.

Pg. 1954, line 26 – The division sign in the equation is under the square root sign. References – There is a reference (and citation) to Storch et al or to von Storch. This is a typo. Also, one reference is missing for Lenschow et al.(1982).

Figure 1 is bit confusing to follow. It may be helpful if the colored lines were defined in
C731

the caption or in a legend.

Figure 3 – The caption contains mistakes. At the end of the sentence describing the STRATUS cruise there is "(magenta)" after it had been defined as red (which appears correct). The last line says the other cruise track is the light colored track but I believe that should say magenta.

Fig. 12 – The caption is confusing; there may be a wording problem there. The text regarding this figure is much clearer than the caption. Consider revising the caption to match the text.

Supplemental material, pg. 7, .line 66 – What is "man of counts"? I guess I don't understand this terminology.

Supplemental material, pg. 7, line 73-75 – The sentence starting with "The other experiment shown..." is a bit awkward. I had trouble understanding it at first. You may want to consider revising.

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 1933, 2009.