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Atmospheric Measurement Techniques Discussions

Interactive comment on “Sources of uncertainty in eddy covariance ozone flux measurements made by dry chemiluminescence fast response analysers” *by* J. B. A. Muller et al.

Anonymous Referee #3

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Interactive comment on ‘Sources of uncertainty in eddy covariance ozone flux measurements made by dry chemiluminescence fast response analysers’ by J. B. A. Muller et al.

Anonymous Referee #2

General Comments

I fully agree with the comments from referee 1 concerning the introduction and the conclusion of the MS. Both sections should be shortened substantially. Paragraphs 1 to 3 of the introduction give a lot of information about ozone deposition processes and ozone impact on plants, which are very interesting but not really useful to introduce

a technical MS dedicated to the sources of uncertainty in ozone flux measurements. These three paragraphs should be considerably shortened, and merged into one, in order to get more rapidly to the paragraph which actually introduces the MS. In the conclusion, several parts which specifically concern the analysers used in this study should be moved in the Results section (see comments by referee 1). The last paragraph of the conclusion, as well as Section 3.4, should be removed because the discussion on ozone deposition processes is beyond the scope of the MS. For the same reason, lines 2-7 of the abstract should be removed.

Specific Comments

Section 2.2.1: Page 2249, line 21: How was chosen the threshold value of u* (0.03 ms-1) ? It is rather low compared to what is commonly used to filter turbulence data.

Section 2.3: Page 2253, lines 24-26: This ‘final visual inspection’ seems rather subjective to me. What is the percentage of data removed by this way ? Are they punctual 30-min flux values or do they correspond to quite long periods in specific meteorological conditions (e.g. just after rainfall)

Section 3.1: This section (‘Meteorological conditions and analyser performance’) belongs more to ‘Experimental’.

Section 3.2: In this section, the authors test the performances of RM, ROM and DCM as if they were three alternative methods for calibrating any fast ozone sensors. They write (p 2255, lines 22-23): ‘All three methods presented here are in principle viable options to calculate ozone fluxes’. This is not true. If a sensor has no offset, ROM is strictly equivalent to RM. If there is an offset, then RM is in principle not valid and one must (!) use ROM or DCM. Since it was shown (Fig. 1) that the ROFI analyser has a large offset (changing with each disc), and it is told (p 2255, line 17) that ‘the offsets on the GFAS are larger than on the ROFI’, what is the point of using RM in this study ? Instead of writing (p 2255, lines 23-25) ‘it could be argued that the offsets in this study are large enough to preferably choose the DCM or ROM over the RM’, the authors should clearly state, at the beginning of Section 3.2, that 'since the offsets in this study are large, it is necessary to choose the DCM or ROM over the RM', and they should leave all the results from RM totally out of the MS. This would also allow the reader to compare more easily the results from ROM and DCM in Fig. 3. Concerning the Disc Calibration Method, the authors write (Section 2.2.4, p 2252, lines 13-14) that this method 'has an implicit assumption that there is no degradation of sensitivity over the time period considered'. Fig. 2 shows that this assumption is not fulfilled for the Disc Period 6 (24/08/07-28/08/07) of the GFAS analyser. Therefore, I don't understand why, in Fig. 3(d), results from DCM for the GFAS analyser are presented during this period of 4 days when the DCM is in principle not valid. I also wonder if the loss of sensitivity observed in Fig. 2 for the Disc Period 6 is exceptional or a common feature of these discs. In the latter case, DCM would not be valid most of the time. On this point, the authors write (p 2256, lines 19-22): 'It is possible to minimise such errors in DCM and ROM by choosing shorter periods over which to calculate calibration factors, however there is a trade-off with potentially larger errors in the fitted parameters with fewer points used in the regression calculation'. Even with fewer points used in the regression calculation, this would be much better than using a constant calibration factor over the whole disc period if the disc sensitivity strongly decreases (as for Disc Period 6 in Fig. 2). Is it what the authors actually did for Fig. 3 ? In this case, what was the time scale of the regression ? Else, results from DCM in this figure would be very uncertain and the authors could not conclude that DCM is the most appropriate method for comparing the two analysers.

Section 3.3: There are two significant issues I have with this section. First, I am not really convinced that DCM is the best method for comparing the two analysers (see above). For example, based on Fig. 3, it seems that the difference between them is lower on 22nd August when using ROM. Therefore it would be helpful to complete Figs. 4 and 5 with chronological series of fluxes obtained with ROM. It may be redundant with Fig. 3 if the latter is made clearer by removing the RM results, but it would make Section 3.3 more easily readable in itself. Second, the comparison of the analysers is

handicapped by the choice of 17th August as an example of day when the flux data do not agree (see comments by referee 1). I think a better choice would be: (i) for Fig. 6, 12th and 21st August (two days with good agreement between GFAS and ROFI), 13th and 22nd August (two days with significant differences); (ii) for Fig. 7: 12th and 13th August or 21st and 22nd August. I also think it would be useful to present meteorological conditions in Fig. 4, or in Fig.5, or at least add information in the text on this point. For example, I wonder if the disagreement between the two analysers is larger or smaller for wet conditions, as the authors point out in the introduction that 'the sensitivity of the discs depend on humidity' (Guesten et al., 1992). Lastly, I agree with referee 1 that the presentation of the cross-correlation between the ozone signals (in Fig. 7) brings nothing to the discussion about the time lags. I also agree with referee 1 on the necessity to perform corrections for high-frequency losses, especially if the difference between raw fluxes from the GFAS and the ROFI is correlated with wind velocity (since high frequency losses are expected to increase with wind velocity and to be larger for the analyser which has the higher time lag, i.e. the ROFI). The latter could be plotted in Fig. 4 or Fig. 5, as well as air relative humidity and solar radiation, or the authors could give in a table the mean diurnal values of these meteorological variables for the days presented in Figs. 4 to 7, and precise if and when rainfall occurred during the experiment.

Section 3.4: This section (including Fig. 9) should be removed because the discussion on ozone deposition processes is beyond the scope of the MS.

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 2241, 2009.