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Atmospheric Measurement Techniques Discussions

Interactive comment on "A relaxed eddy accumulation system for measuring vertical fluxes of nitrous acid" *by* X. Ren et al.

Anonymous Referee #2

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General comments:

The manuscript by Ren et al. is a nice study on the development and application of a relaxed Eddy Accumulation system to measure fluxes of nitrous acid (HONO) in the atmosphere. The instrument is well described and some highly interesting results are presented. In contrast to another recent HONO flux study (Zhou et al.) a correlation with the NO2 concentration and the radiation is observed, which would contradict the normally proposed nitric acid photolysis. The manuscript should be published in AMT after some minor corrections.

Minor comments:

Section 2.1, page 4109-4110:

When describing the REA system (see also Fig. 1), specification of the flow rates used for MFC#1-3 would help already here to better understand the set-up and procedure used. In addition, the time delay given on page 4109, line 27 (and in section 3.2), is not constant and is depending on the operation mode, i.e. is ca. 350 ms for the measurement mode, but ca. 450 ms in the deadband mode. Was this considered?

Page 4111, line 19:

If only 22 % of the measurement time is used for sampling in each channel (see page 4114, line 21) shouldn't the actual detection limit be higher than 3 ppt, or is this already considered here?

Page 4111, line 20:

The measurement accuracy of the HONO instrument of only 15 % seems to be quite high, which would mean that the minimum detectable HONO flux would be limited when the concentration difference between the updraft and downdraft air mass is <15%. For the REA system used here, two HONO instruments are used and both can have different systematic errors (gas/liquid flow rates, etc.), except from the error of the liquid nitrite calibration standard, which is however typically only a few %. More details on the errors of the HONO flux would be helpful.

Page 4116, line 23:

The formulation is misleading. E.g. photosensitized conversion of NO2 on humic acids is not a "photolysis". I suggest: "Several photochemical heterogeneous processes. . ."

Page 4117, line 1 and 2:

The references of Ammann et al., Arens et al., Bröske et al. and Aubin and Abbatt refer to studies of dark reactions, whereas here a photochemical process can be inferred from the experimental results. If the author would also like to high-light dark HONO

sources, other potential candidates have also to be added. However, I suggest deleting the references to the dark sources.

Section 4.1:

The authors observed a very nice correlation of the positive HONO flux with the radiation and the NO2 concentration, which would be in line with the photosensitized conversion of NO2 on humic acid surfaces. In the paper by Stemmler et al. (2007) a parameterization of the HONO source strength is specified. It would increase the strength of the present manuscript, if the author tried to check whether the experimental flux data fit to this parameterization.

Page 4118, lines 21-21 and page 4119, line 19-20:

The sentence, especially the one on page 4119 (“deposition”), may be misunderstood. The authors mean that in the western US the pH of the soil is lower compared to other regions caused by lower precipitation of strong acids (HNO3, H2SO4). Caused by the low pH, the equilibrium HONO/nitrite in the soil will be shifted to nitrite, leading to a high (and not low, as specified. . .) HONO deposition (is also an acid...). Both sentences should be reformulated.

Page 4120, line 14:

Abatt, J. P.:

Page 4120, line 27:

Abd El Aal

Page 4121, line 29:

source of HONO?

Fig. 3 (top, left) and Fig. 4 (left red): The HONO flux data in the two figures is different? E.g. the dip at 12:00 in Fig. 4 is not shown in Fig. 3.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 4105, 2011.