



Interactive comment on “

Comparison of methods for the determination of NO-O₃-NO₂ fluxes and chemical interactions over a bare soil” by P. Stella et al.

Anonymous Referee #2

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As mentioned in the introduction, the investigation of the NO-NO₂-O₃ triad is highly important for green house gas and reactive nitrogen chemistry research in the atmosphere. Because measurements in the atmosphere are – even in remote areas – nowadays influenced by anthropogenic sources, each paper is welcome that discusses different aspects of this problem, as well as methodological issues regarding the measurements.

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The paper has a significant problem in the interpretation of the data, and this comes to a focus in the conclusion that “The AGM flux uncertainties were mainly due to friction velocity”. Atmospheric turbulence is a phenomenon which allows a very effective transport between the atmosphere and the underlying surface. The methods used in the paper, the eddy-covariance method (EC), the aerodynamical gradient measurements (AGM, Eqs. 1-3), the transfer time (Eq. 13), and the uncertainty analysis (Eq. 8) are based on a fully developed turbulent regime. In the case of low wind velocities and also as a consequence of low friction velocities, this assumption is not valid. Turbulence is either intermittent or still missing with a nearly laminar flow. These conditions must be excluded from the application of the above given methods. This can be done e.g. with a test on steady state conditions (Foken and Wichura, 1996; Vickers and Mahrt, 1997). If the test fails, the data must be neglected or analyzed with special methods like conditional sampling or wavelet spectra. Also the test on developed turbulence with integral turbulence characteristics is possible (Foken and Wichura, 1996). Another way is used in ecology, where all data with friction velocities below a given threshold are neglected (Goulden et al., 1996; Papale et al., 2006). I propose for the revision of the paper the latter method with a threshold of $u^* = 0.15 \text{ ms}^{-1}$ for bare soil, which is much lower than the threshold used in ecology. The neglecting of all non-turbulent situations does not only change some of the figures like Fig. 5 but also some of the text and the conclusions. A careful analysis of the developed turbulence is highly relevant for the application of the AGM because of large gradients and very small diffusion coefficients and, consequently, very low fluxes.

Further remarks:

p. 5485, line 22 ff. and Eq. (1): Please make sure that you used the friction velocity from EC data (i) and not from the calculation with the AGM (ii). For case (i) the measurements of the wind profile are not relevant and for case (ii) the distance constant of the anemometers and the possible overspeeding correction are relevant (Wieringa, 1980).

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p. 5486, line 8: For the reader it would be helpful if the reference of the footprint model used can be given.

p. 5486, line 20: Is the flow turbulent? Please give the Reynolds number.

p. 5487, line 15 ff: The universal function by Dyer and Hicks (1970) and especially the von-Kármán constant of 0.41 are not state of the art and should be probably replaced by the function by Businger et al. (1971) in the modification by Högström (1988), see e.g. Foken (2006).

p. 5488, line 5: Perhaps some information about the software used and the quality control would be helpful.

p. 5488, line 12: Please replace Monin-Obuchov length by Obuchov length (Businger and Yaglom, 1971;Foken, 2006;Obukhov, 1971).

p. 5488, line 15: It is positive that you used the Obukhov length with the buoyancy flux (in your case combination of the sensible and latent heat flux). But in this case you have also to replace the temperature by the virtual temperature. Why did you not use the buoyancy flux, which you measured directly with the EC method and to which you probably applied the Schotanus et al. (1983) correction to determine the sensible heat flux. Remark: The universal functions are defined with the Obukhov length without buoyancy flux.

p. 5489, Eq. 6: This equation is trivial and has been used for a long time for all ozone flux measurements.

p. 5491, Eq. 10: You make the assumption that the turbulent Prandtl and Schmidt numbers are identical!

p. 5491, Eq. 12: Why you did not make a stability correction (universal function)?

p. 5492, Eq. 13: Please give the definition of R_a and R_b . This means no extension of the paper, because most the relevant equations are already given.

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p. 5495, line 29ff and p. 5498, line 27ff: Probably it would be interesting to separate the data set for wind coming from Paris and the other wind directions and to make the following investigations for both data sets.

p. 5497, line 3 ff and p. 5499, line 4ff: This part, and similar parts in the paper, must be revised. If no fully developed turbulence exists, the Monin-Obuchov similarity is not fulfilled (basis for AGM), therefore no turbulent flux can be determined and no error of a turbulent flux exists. If a flux is very low or even below the detection limit a relative error makes no sense. Please give for these cases an absolute error, probably in combination with a relative error for larger fluxes. Fig. 5 must be revised accordingly.

p. 5498, line 23 ff: Except for ozone only at night (but here are the fluxes nearly zero due to the missing turbulence) and in some single cases, the gradient is larger than the detection limit of the applied instruments. This is not new and an overview has already been given by Foken (2008, p. 134-137). Please add your efforts to make the application of the AGM nevertheless possible. For ozone your results are not so bad. Please also discuss the possibility of detecting, for developed turbulence, the effect of chemical reactions (p. 5497, line 10 ff).

p. 5503, line 17: What is “soil ozone flux”? Figs. 3, 5, 9: The high accuracy of the regression calculations is unrealistic.

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