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Interactive comment on “A modified micrometeorological gradient method for estimating O₃ dry deposition over a forest canopy” *by* Z. Y. Wu et al.

T. Foken

thomas.foken@uni-bayreuth.de

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The measurement of deposition fluxes above tall vegetation is a never ending story because of many challenges. Very important is the small gradient of temperature and trace gases above the canopy, which is often lower than the detection limit of the sensors/analyzers (Foken, 2008, p. 135). The authors try to overcome this problem by using a gradient between a level above the canopy and one within the canopy, with a significant increasing of the gradient. Unfortunately, they do not discuss the influence of relevant processes at the top of the canopy on the new proposed method, like roughness sublayer or mixing layer (Garratt, 1978; Finnigan, 2000; Harman and Finnigan,

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2007, 2008) (Raupach et al., 1996), decoupling (Thomas and Foken, 2007a), coherent structures (Collineau and Brunet, 1993a, b; Thomas and Foken, 2007b), scalar similarity (Ruppert et al., 2006), and reactions. Some of the effects may not be relevant due to the selection of only 26 % of the data set for the analysis. Because the above-mentioned processes have a daily and annual cycle, it would be interesting to see a daily and annual cycle of the availability of the data. I assume that only situations with moderate and high wind velocities and a good coupling of the atmosphere with the upper canopy layer were used.

The most relevant problem is the calculation of the aerodynamic resistance in Eq. 5. This leads to an overestimation of the deposition velocity by the aerodynamic gradient method (AGM). But this aerodynamic resistance is also used in the proposed micrometeorological gradient method (MGM), Eq. 11. I assume that z2 is equal to h, because no other measurements were available. It is extremely difficult to make exact measurements at the top of the canopy because of the extreme gradient at this height, the heterogeneity of the forest and a possible dependence on the wind direction and the strong influence of the roughness sublayer (mixing layer). The authors encountered this problem through the strong influence of the wind velocity on the results, because the wind field penetrates more or less into the forest and the level with the extreme gradient is either a little bit above or below the top of the canopy.

It is not true that the AGM always overestimates the deposition velocity. If you measure not at the top of the canopy but at two levels at certain distances from the top, and apply a roughness sublayer correction function (Garratt, 1978), you can measure fluxes accurately (Wolff et al., 2010a; Wolff et al., 2010b; Foken et al., 2012). Unfortunately, this method is limited due to the accuracy of the gas analyzer, which is probably not good enough for ozone.

Because the aerodynamic resistance in Eq. 5 – and therefore also in Eq. 11 – is too small (flux and deposition velocity are too large), this must be compensated for by the aerodynamic resistance in the layer from h to z3, Eq. 10, so that the sum of both resistances in Eq. 12 is again accurate and a deposition velocity (flux) can be calculated in a good agreement with the eddy-covariance data. In other words, the calculation of the integral in Eq. 10 must be wrong (too large resistance), even when the Eqs. 13 ff appear to be in a good agreement with the theory. What was the tuning parameter of your model?

By the way, the applied universal function by Businger et al. (1971) in the modified form by Högström (1988) already includes a turbulent Prandtl number for the sensible heat flux ,or a turbulent Schmidt number for trace gas fluxes (Foken, 2006). On the other hand, you use a turbulent Schmidt number of 0.8 (p. 786, line 9); make sure that you did not use the turbulent Schmidt number twice.

The modified Bowen ratio method (MBR) was not the main topic of the paper, but it is important to show a good scalar similarity between ozone and the proxy (carbon dioxide). This is not trivial, because the ozone flux is influenced mainly in the morning by high reactions with NO, emitted during the night, and the assimilation is probably limited in the afternoon (Ruppert et al., 2006).

For the final publication you should show which phenomena at the top of the forest canopy you excluded due to the data selection. The influence of the roughness sub-layer should be discussed and the main point is: Because Ra (z1:h) is obviously too small, how have you modified Ra(h:z3) so that Ra(z1:h) + (Ra(h:z3) is again accurate?

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