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Biogeosciences Discussions

Interactive comment on "Measurement and modelling ozone fluxes over a cut and fertilized grassland" *by* R. Mészáros et al.

Anonymous Referee #2

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This paper presents a set of measurements and model analysis of ozone deposition to a grassland side, before and after cutting. In principal, such a dataset should be a useful addition to a rather sparse set of data. The results presented, that deposition velocity (Vg) does not change after a drastic change in LAI are rather surprising, and call for a thorough analysis to determine the causes and provide a good explanation. Unfortunately, this paper presents only a superficial analysis. The authors use lots of space to describe their model (although with errors in some equations I think), and very little to explain their surprising results.

I cannot recommend publication of the paper as it is. If the authors can present a more convincing analysis in any re-write of why LAI can be reduced by a factor of 10 with no significant effect on Vg then I would very much like to see this, and encourage them to

make the attempt.

- Eqns (15) and (16) as stated are simply wrong. Eqn (15) ignores the important role that the Rb term still has to play - the quasi-laminar resistance has not been encountered at z=h. Equation (16) is wrong as it ignores the non-stomatal pathways, which are still a sink of ozone for concentrations taken at z=h. The authors have omitted Cieslik's remarks on what "top of canopy" means in his formulation, which probably explains some of this (Cieslik's term top of canopy is odd though, since this is above z0+d and hence not the place where stomatal and non-stomatal can be separated.

  Have the authors really calculated Fst from C(h)?

- Eqn (13) for Rcut seems very odd. As LAI increases Rcut increases - so the more LAI the less cuticular uptake. Very strange! I really hope this is a mis-print, as otherwise the model is seriously in error.

- The details of the model approach seem to be very similar to that presented in Meszaros et al., 2009 (Atmos. Env., 663–). Why not simply refer to that paper for such technical details?

- Is LAI as used here 1-sided, 2-sided, projected?

- The plots show wind-speed, T and RH. I would have liked to see ustar and 1/L as well, to try to get some clues as to what is going on. As the author's results are so surprising more information is required to try to make sense of it.

- p1077, section 4.1, is the soil water here measured? If modelled, how do they know if their estimates are reasonable?

- p1077, section 4.1, lines 14-15 are confusing, since earlier in the text it is stated that LAI decreases to 0.14 m2/m2.

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- p1078, section 4.2. The authors mention lower wind-speed, but isn't lower soil-water a factor too?
- p1078, section 4.2. It is said that LAI is smaller by a factor of "more than 10". Do they mean 20? "larger than 3" divided by 0.14. This whole paragraph highlights the problem and begs for more analysis!
- p1078, lines 20-25. The author's model uses a straightforward LAI-scaling for Gst, with sun and shade fractions accounted for. Here is stated that Fst decreased from 60% of total flux before the cut to 40% after. Shading is said to account for this effect. This surprises me, since it would take an awful lot of shading to counter-balance a ten-fold reduction in LAI. And even shaded leaves take up ozone. This behaviour of the model really needs to be explained.
- p1079, Discussions. The authors state that the reduction in LAI allows for increased non-stomatal flux, keeping Vg roughly constant. How can this work? The two non-stomatal pathways are cuticular and the soil. The factor of 10 reduction in LAI should reduce the cuticular losses by a factor of 10 also - the reduction in Fcut should be even greater than the reduction in Fst since the latter does have some sun-shading non-linearity. (Although according to the very strange eqn (13), non-stomatal deposition in the authors model does in fact increase as LAI decreases. Non-stomatal conductance is a maximum at LAI=0.) The remaining pathway is to the soil, but here the resistance is said to be 600 s/m - giving a max Vg of 0.17 cm/s.
- p1079, Discussions. On the whole this "discussion" section is far too weak, with little more than hand-waving arguments about things that might possible affect Vg. This is not acceptable for a publication discussing a very counter-intuitive result.

Interactive comment on Biogeosciences Discuss., 6, 1069, 2009.