

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

Anonymous Referee #4

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General Comments:

The paper presents measured and modelled ozone dry deposition velocities and fluxes over a grassland under three different scenarios of surface conditions. The method used in the manuscript is scientifically sound and the results will be a valuable contribution to the scientific community to better understand ozone dry deposition processes. The paper needs some improvements as specified below.

Specific Comments:

1. Abstract needs some careful re-writing. I suggest the abstract first discuss the measurements, including one or two sentences stating the measurement period, site and purpose; then a few sentences describing the observed phenomena. Model practise,

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model results (including its comparison with measurements) and possible explanations can then be described in the second paragraph of the abstract.

2. Page 1078, line 20: The paper shows that the model used in this study underestimates daytime V_d and overestimates nighttime V_d . This implies that the diurnal cycle of the non-stomatal resistance was most probably not correct modelled. Note that friction velocity (u^*) is higher during the day and lower during the night. Although the in-canopy aerodynamic resistance is treated as a function of u^* , the cuticle resistance is not. The formulas of in-canopy aerodynamic resistance and cuticle resistance presented in Zhang et al. (2003) were both treated as a function of u^* , and thus, might provide a better diurnal cycle. It is worth to add a sensitivity test on this point and it is quite easy and straight forward to do so.

3. The statement on page 1079, lines 9-11 is not correct. Zhang et al. (2002) formula gives smaller in-canopy aerodynamic resistance (thus, larger soil uptake), but larger cuticle resistance, at smaller LAI. The author is recommended to look at the details in Zhang et al. (2003) (Atmos. Chem. Phys. 3, 2067-2082), which describes individual resistance terms separately, to better understand the formulas.

4. Page 1079, lines 19-20. This statement might not be true. The underestimation during the day could also be caused by modelled non-stomatal resistances (see comment No 2 above).

5. Section 4 has a title: Results and discussions; and Section 5 has a title: Discussions. These two sections need different titles or combined into one section.

6. Sections 4, 5 and 6 need some improvements. I can tell no big difference between Sections 6 and 5. While Sections 4 and 5 needs more detailed discussions (not so many speculations), Section 6 may just briefly summarize the field experiment, major findings, and suggestions of future work.

7. The data used in this study is very limited, i.e., less than 4-week data for all three

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different scenarios (pre-cut of the grass cut, after cut, and after fertilization). There is a small possibility that the small data set, if not statistically significant, might not represent the real phenomena. A brief discussion on this limitation, if possible, will make the conclusions stronger.

8. It would be clearer if (1. period, 2. period, 3. period) were changed to (1st period, 2nd period, 3rd period) in all figures and their captions, Table 1, as well as in the text.

9. Technical corrections. I suggest the author to ask an English native speaker to do a proof-reading. For example, many paragraphs only contain one sentence. I would expect a paragraph to have more than one sentence.

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

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