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Comment

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

Anonymous Referee #5

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This manuscript presents data on ozone fluxes measured on managed grassland during the GRAMINAE campaign in May-June 2000. A particularly interesting feature of the campaign is that the grass was cut and the field was fertilized during the measurement period. The authors conclude that these events had no significant effect on the deposition velocity of ozone, while the partitioning between stomatal and non-stomatal fluxes was altered. This is an unexpected and interesting result. Even though the MS deals with an important topic that would be suitable for the scope of Biogeosciences, unfortunately I cannot recommend publishing it, unless the following major concerns can be addressed in an adequate manner. In practice, this would require further data analysis and a comprehensive revision of the text.

(1) Ozone flux measurements

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(a) The fast response sensor used in the study requires a continuous calibration with a co-located reference monitor. However, no such monitor was included in the measurement set-up, but measurements taken at a different location, 1 km away, were used for calibration. However, ozone concentrations can vary significantly even within such a short distance, depending on local NO_x emissions, topography, etc. As the site description in the MS is very limited, it is not possible to evaluate if these factors may have had an effect on the derived concentrations. Moreover, according to a companion paper (Sutton et al. 2008; including most of the authors of the present MS), the reference O₃ concentration is measured 5 km, not 1 km, from the flux site. The authors should carefully justify the use of the non-co-located monitor for calibration.

(b) The authors argue that, even though the fast response sensor requires a calibration to evaluate ozone flux, no calibration is needed for deposition velocity (p.1071, l.21-22). However, this is true only if there is no offset (i.e. $b=0$ in Eq. 1). If $C = aU + b$, then $v_d = \langle w'C' \rangle / \langle C \rangle = a \langle w'U' \rangle / (a \langle U \rangle + b)$. This shows that in a general case v_d depends on a and b , i.e. on calibration. (C is concentration, U voltage, v_d deposition velocity and w vertical wind speed; $'$ denotes fluctuations and $\langle . \rangle$ averaging)

(2) Data processing

(a) Even though the ozone sensor is said to be "fast", this needs to be quantified. The dynamic response of such measurement systems (including the effect of tubes and possible filters) is never perfect, which may result in a considerable flux loss due to attenuation of high frequency fluctuations. This effect should be assessed and the related high-frequency flux loss quantified and possibly corrected for. Correspondingly, the high-pass filtering may result in a flux loss due to low-frequency spectral attenuation.

(b) The argument for not applying the WPL correction for heat is obscure. The fact that the lag time is longer than in some other systems does not justify the conclusion that the temperature fluctuations will vanish in the inlet tube. The authors should provide a

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more coherent justification for ignoring the heat correction, including technical details on the inlet tube and flow rate.

For further questions on data processing, see the detailed comments below.

(3) Data analysis

(a) Figure 3a indicates that the average daytime O₃ concentration was clearly lower during the 1st period than during the other periods. Figure 3b/Table 1 indicates that the measured average morning/daytime flux during the 2nd period is significantly higher (more negative) than during the other periods. However, the authors conclude that the differences in the fluxes are small (p.1080, I.5-7, 21-23). If this were the case, there should be a much larger difference in deposition velocity between the 1st and 2nd periods than shown in Figure 3c and Table 1.

(b) The partitioning between stomatal and non-stomatal fluxes is based on a model rather than an analysis of data. It is a common practice to estimate stomatal resistance based on water vapour fluxes. All the required data for deriving stomatal resistances are available (water vapour fluxes are used for the WPL correction, p.1073, I.16), and stomatal resistances are indeed derived in a companion paper cited in the MS (Nemitz et al. 2009; including all authors of the present MS). It appears very strange that no attempt was made to utilise this information.

(4) Deposition modelling

(a) It is stated in the abstract that "a detailed deposition model for ozone is used to parameterise and to calculate the deposition velocity" and in the introduction that "deposition model for ozone is parameterised and tested against measurements". However, the modelling section (Section 3) repeats a model description that has been presented in previous publications but does explain how the parameter values were obtained. The only parameter values that are not reported in the MS are those of $r_{st,min}$ and b_{st} , but it is not clear at all if these parameters were selected based on the measurement

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data and, if so, how this fitting was carried out. Similarly, it is reported that the value of R_{soil} was "chosen" but the rationale behind this is not explained. In my opinion these cannot be considered sound procedures for model parameterisation and testing.

(b) The modelling results play a key role in the interpretation of measurement data. It is obvious that the partitioning of fluxes largely depends on the assumptions of the deposition model and thus cannot be considered a reliable estimate, unless the individual components of the modelled bulk surface resistance can be validated. No such data are presented in the MS, nor is any previous validation discussed.

(c) I have problems in understanding the parameterisation of cuticular resistance, R_{cut} , and the discussion on the role of the in-canopy resistance, R_{inc} . Why would R_{cut} increase with increasing LAI (Eq. 13)? The parameterisation of R_{inc} (Eq. 14) is originally based on a very different vegetation structure and has not been shown to be applicable to grasslands. In fact, the authors discuss results from the same experiment (from papers they co-author) that suggest there are problems with the present R_{inc} (p.1079, l.12-18).

(5) Presentation

(a) The description of the site and measurement methods is too brief. Even though the MS is part of a Special Issue, which includes papers with more detailed descriptions, every paper should be independent enough and provide the information about the measurement site, instrumentation, etc. needed for understanding the results. On the hand, the deposition model is described in detail, directly repeating information from previous publications. In this section, it should be made more explicit how the model was modified within the present study.

(b) There is confusion as to what is considered results, discussion and conclusions. Section 4 ("Results and discussions") presents measurement data and their interpretation. Some of this interpretation is based on the modelling results, with the implicit assumption that the adopted model is correct. In Section 5 ("Discussions"), evidence

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contradicting model assumptions is presented, for example concerning the in-canopy resistance studied at the same site. In addition, this section contains a rather speculative discussion on the role of NO emissions without any quantitative data. However, in Section 6 ("Conclusions") the model-based interpretations of Section 4 and the speculation on the processes affecting NO-O₃ interactions are adopted as conclusions of the study without any indication of methodological caveats. Correspondingly, the abstract presents the conclusions as if the mechanisms were resolved by actual measurements.

In general the deficiencies in the presentation and language make it difficult to evaluate the results. There are many inaccurate expressions throughout the paper; see the detailed comments below.

Detailed comments

- p.1072, l.10: "calculated"?
- p.1072, l.21: What is meant by "regressions"?
- p.1073, l.1-2: So a constant lag value was used for all data?
- p.1073, l.20-22: How did you define "poor fetch"? Why was this filtering needed as you also applied the footprint-based filtering?
- p.1073, Eq. 2 & p.1074, Eq. 3: A minus sign is missing, since the downward flux is considered negative in Figs. 2 and 3.
- p.1073, l.9-10: Please specify the high-pass filter type.
- p.1073, l.10-12: The coordinate system is rotated twice, but as all axes are affected it is misleading to call the rotation 2-dimensional.
- p.1073, l.18: Corrected for what?
- p.1075, l.11: A wrong Eq. is referred to.
- p.1076, Eq. 13: Unit missing.

- p.1076, l.14: Unit missing.
- p.1077, l.1: Formally, c_c is the concentration at $d+z_{0c}$, where d is the zero-plane displacement and z_{0c} the nominal roughness length of ozone, rather than at the physical canopy height.
- p.1077, l.11: According to Fig. 1, the maximum soil water content was less than 0.15.
- p.1077, l.19: What kind of "difficulties"?
- p.1077, l.21-22: In the figure, the fluxes are negative.
- p.1078, l.1: I'm unable to see the "lower wind-speeds" in Fig. 1.
- p.1078, l.8-9: If this refers to Fig. 3, then there is a clear difference for most of the morning, not only at midday.
- p.1078, l.10-15: The text in this paragraph is difficult to follow.
- p.1078, l.11: "seems that v_d were smaller"? Table 1 shows that they really were smaller.
- p.1078, l.17-19: How do you conclude this? The LAI change from 3 to 0.14 has a larger effect on $1/R_{cut}$ than $1/(R_{inc} + R_{soil})$.
- p.1078, l.20-21: This statement is not in accordance with Table 1, which shows that on average the model overestimates during both the daytime and night-time.
- p.1079, l.2: I am unable to find any deposition velocities for grasslands from this paper.
- p.1079, l.9-11: Any consequences to the present study?
- p.1079, l.20: "larger" than what?
- p.1080, l.5: "difference" in what?
- p.1080, l.12-13: Units missing.

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- p.1080, l.20: "higher"?

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