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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Forest-atmosphere exchange of ozone: sensitivity to very reactive biogenic VOC emissions and implications for in-canopy photochemistry" *by* G. M. Wolfe et al.

Anonymous Referee #1

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General:

I must admit it was a bit surprising for me to find almost no 'hard' facts in this manuscript. It is a 'mere' modelling exercise which presents hardly any measurements, and thus also no evaluation of the model implementations. This is unusual. However, I found it to be a very interesting manuscript which gives a good overview about the uncertainties of ozone deposition, VOC emission estimates and air chemistry processes within the canopy. Based on a recent implementation of an air chemistry model, knowledge gaps are demonstrated and possible (!) solutions for gap closure are outlined. This way modelling results may inspire new investigations rather than only integrate

various measurements into a consistent overall picture.

The authors use the recently described and evaluated CAFE model for their air chemistry analysis. Since there are only few changes from the published version, a repetition of the model description is certainly not needed. Nevertheless, some more detailed basic descriptions of the site (tree species, stand height) and the model are necessary to better understand the modelling exercise. Particularly, I suggest to briefly mention the principles of the canopy model (leaf are distribution, voc emission calculation, stomata conductance and transpiration, heat balance calculation, time step(s), drivers) and how ground emissions are estimated (i.e. NOx is 'prescribed' but where direct measurements available for the whole period or does this include some model assumptions?). For example the VRVOC emission is assumed to origin from specific storages (Eq. 6) – are other VOC treated similarly, or is a light driven emission of isoprene and other terpenoids considered?

Specific:

- P2, L23ff: Although the high-time of ozone damage studies might have passed, there are more recent papers around that those mentioned (e.g. Bytnerowicz et al. 2008, Goumenaki et al. 2010, Matyssek et al. 2010, Zapletal et al. 2011). The flux-based index is also vigorously demanded by Matyssek and Innes 1999). - P8, L7ff: I am not sure here but Archibald et al. 2010 also suggested an increased OH recycling mechanism. Is this the same here? Is it somewhat related? Or is it something totally different? - P12, L5ff: This chapter describes the scenarios applied. However, it would be much more convenient to derive those from a table indicating the basic differences and similarities of the runs. - P15, L10ff: If the noon-time relative carbon loss at a hot day is 1.4-2.1% it is certainly not 'well in agreement' with annually losses of 4%. This would mean that the relative loss is higher in spring and autumn to match this number. On the other hand, it is a rate well in accordance to estimates (e.g. Sharkey and Ye 2001, Tingey et al. 1980). - P26, L8ff: To investigate the potential effects of deposition on VROX is certainly valuable. However, I wonder why this is none with only

a single assumed velocity rate. It would be more convincing if the sensitivity would have been tested over range of velocities. The results could be used to underline the argumentation later on used in this paragraph. Also, I would like to encourage the authors to provide a figure about this.

Technical: - P6, L12: The abbreviations used in Eq. 1 should be indicated in parenthesis close to the respective descriptions. - P7, L3: sometimes? I don't understand the need for this sentence. - P21, L15: replace 'decreases' by 'changes' - P27, L11: insert 'or all' after 'fraction' - P27, L21: reference for this 'likeliness' ? - Fig. 5: The description here should be improved. How are the RO2 concentrations derived? Is this necessary for steady state calculations?

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