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Interactive comment on “Modeling atmospheric CO_2 concentration profiles and fluxes above sloping terrain at a boreal site” *by* T. Aalto et al.

Anonymous Referee #1

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General Comments:

This paper intended to address the influences of topography and land use on atmospheric CO2 profile and the regional representative CO2 concentration using modelling method. These are important issues in atmospheric CO2 studies and well fit into the scope of ACP. The modeling method used in this study, i.e. coupling a 3D regional scale CO2 transportation model with a local scale vegetation CO2 flux sub-model, is novel, although both models may still need to be refined in order to better address the problems. The observed diurnal variation of CO2 flux at the forest site was well simulated, but the observed diurnal variation of CO2 concentration at both forest and hill top sites were not captured by the model. The explanations for the discrepancies were generally clear. The main limitation of this paper belongs to the fact that only one day simulation results were presented (i.e. 22 July 1998). This may weaken the main conclusions draw from this simulation study.

In general, I found this paper was well structured, clearly presented and informative, therefore worth to be published in ACP subject to some minor revisions.

Specific Comments:

(1) Unlike the influences of land use on CO2 flux, which were explicitly treated in the model, the influences of topography on CO2 flux (e.g. through radiation, atmospheric stability, water availability etc.) were not well represented, at least, not clearly described in this manuscript. If that was the case, the conclusion that CO2 profiles were more affected by upwind land use than topography (P10020 L12-13) might be partly attributed to the modeling schemes used.

(2) Due to lack of explicitly simulated soil layers, the model did not include soil respiration simulations; instead it used in situ measured values in this study. While it did not affect the main conclusions drawn from this study, it would definitely restrict the broad applications of the model while respiration measurements are not usually available. It might be appropriate to add a few more sentences to address this weakness.

(3) In P10027 L21-L22, it was stated that "the simulations were first performed ĚĚi.e. for 22 July 1998", I could not find out in the following texts what other simulations were conducted except for some sensitivity tests. I would suggest add one small paragraph between sections 3 and 3.1 to give readers more information about the model runs, e.g. different runs conducted; input, output and modeling time steps; model initialization etc.

(4) In Appendix A, surface latent heat flux LE was calculated by Eq. (A2), while potential ET (also represented the actual ET since there was no water stress at the site) was calculated by Eq. (A6). Theoretically, LE should equal to λE (λ is the latent heat required by water vaporization). Please explain why you need two equations to

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calculate the same variable.

(5) There were a few miss-typing errors need to be corrected, e.g. P10021 L18 (effected); P10029 L16 (m2) and L28 (fores); Fig. 8, LH, should be LE to be consistent with Appendix A.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 10019, 2005.

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