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Interactive comment on "A simple model to estimate exchange rates of nitrogen dioxide between the atmosphere and forests" *by* J. Duyzer et al.

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1. General comments

In their modelling study, Duyzer et al. describe and test a simple two layer model to estimate the exchange of NO_2 between the forest canopy and the atmosphere. The model includes chemical production ($NO+O_3$) and loss of NO_2 by photolysis. The two model layers represent the upper crown region and the trunk space in the lower part of the canopy. Vertical exchange between these two layers and between the upper layer and the atmosphere above the canopy is calculated by applying a resistance-analogue relationship to turbulent exchange (K-theory). The model is tested using data sets from two different forest sites with coniferous and mixed deciduous vegetation. The main idea of the study is to simplify an existing detailed approach from multi- to just two model layers. This is justified with the computational and parameter requirements of the detailed models in large scale applications.

The study addresses important questions related to chemical, biological and physical processes within the plant canopy which have a strong impact on the atmospheric budget of NO_2. Therefore, the subject is closely related to nitrogen oxides emissions from European forest ecosystems and thus fits nicely into the scope of this special issue. However, the manuscript does not provide new concepts. In my opinion, the presented modelling approach is not up to date. The description of the experimental set-up is insufficient and, as admitted by the authors, the data quality is too weak for model evaluation. Therefore, I do not recommend the publication of the manuscript in BG.

2. Specific comments

2.1. Theory of turbulent exchange

First of all it should be mentioned, that the detailed models, cited as a reference at p.1035, l.21 (Gao et al. 1991, Duyzer et al. 1995[1]) apply K-theory, i.e. resistance-analogue relationships within the canopy. K-theory has been widely used for different purposes (for example Baldocchi, Atmosp. Env. 22(5), 1988; Jacob and Wofsy, JGR 95(D10), 1990). However, it is known for more than fifteen years, that these models make wrong assumptions on the nature of turbulence within the canopy (M.R. Raupach, Q.J.R.Met.S. 113, 1987), which is also indirectly admitted by the authors (see p.1049, l.6). Compared to these "first generation" K-models, there are advanced techniques for different scales available that deserve at least a short discussion (e.g.

[1]this reference appears twice in the bibliography

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two-layer model by McNaughton & van den Hurk, BLM 74, 1994; for a comparison of Lagrangian and Eulerian multi-layer models see Siqueira et al. JGR 105(D24), 2000).

Even though K-theory (applied within the canopy) is still very popular as it is often said to be "useful", I am concerned about the way the authors parameterised and tested their model.

2.2. Turbulence parameterisation

It seems that there were no valuable observations available on the flow field and turbulence properties of the two investigated canopies (see second point of the notes on the model at p. 1039-'40). What is the rationale for applying a turbulence parameterisation to a forest canopy that has been derived for a maize crop canopy? What is the factor b in the parameterisation of R_{inc} (Equation appearing in the text at p.1040, l.10) and what value is chosen based on what rationale? Is it reasonable to apply the same parameterisation to two forest canopies which have obviously completely different structure? What kind of differences between the two canopies have been observed (vertical Biomass/Leaf area distribution)? I would expect a very distinct trunk space at the coniferous site and a much more uniform distribution at the deciduous forest site. This would probably result in different flow regimes and turbulent timescales which determine the effectiveness of the chemical and biological source and sink processes for NO and NO_2.

2.3. Experimental data

The description of the experimental set-up and the data sets used to constrain, parameterise and test the model is very short and not complete. I do not recommend to give a full description of the measurement protocol (which should then be given elsewhere e.g. in the reference paper cited at p.1041, l. 14), but some basic information is required. What technique has been applied to measure the fluxes (p.1042 l.3: "dedicated monitors") and concentrations ("slow response instruments" (see p.1042 l.4)? LAI is an important parameter because it is used to calculate R_{inc}. How has it been estimated on both sites? In my opinion, the value of 14 for the coniferous site is huge and not detectable by non-destructive methods because commonly applied optical sensors saturate at LAI> 8 due to the absence of light.

Compared to the Speulderbos site, information on the experiment at the Soroe site is even more rare (p.1042, l.22: "The experimental set up was similar to that of Speulderbos").

The lack of appropriate information is also a main reason why the evaluation of the model fails. As the authors admit, the quality of the available data is "low compared to what would be required for true model testing". Furthermore, the discussion of flux divergence is purely speculative since observations of NO fluxes above the ground are not available.

2.4. Results

Section 4.1 describes the data processing and includes a statement on the weak data quality. The first part of Section 4.2 and Subsection 4.2.4 describe the boundary conditions of the model calculations at the two sites (without giving numbers for NO soil emissions and background concentrations of NO, NO_2 and O_3). Formally, this information belongs to Section 3. Furthermore, the information given on data processing is inappropriate because the exact treatment of the raw data (data gaps, rejection, averaging technique) remains unclear. For example, Fig. 2, and Figs 5-11 show wide gaps of missing data in the observed and calculated time series, which are not explained.

The ozone fluxes (Section 4.2.3) should also not be included in the result section be-

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cause the observed ozone fluxes have been used to calibrate the model (thus the good model performance for ozone is not surprising). What results are remaining? Most of the points made in Sections 4.2.1, 4.2.2 and 4.2.4 are interpretations of the disagreement between model calculations and observations. The results demonstrate the importance of the turbulence parameterisation (see above). However, decreasing the resistance R_{inc} for the coniferous site by a factor of 10 lacks any physical basis as no direct observations are available. What are the absolute values of the resistances? The results for the second site are restricted to p.1047,l.11-16.

2.5. References

Taking into consideration the interdisciplinary background of the study, the bibliography is relatively short. References on the theory of turbulent exchange within the canopy are definitely out of date and/or ignore the important progress that has been made during the last two decades (for example see M.R. Raupach (1988,1989), J. Kaimal and J. Finnigan (1994), Van den Hurk and McNaughton (1994), G. Katul et al. (1999), M. Siqueira et al. (2000). This is also a major problem of the introduction, which attempts to present the background to the study: The biological processes are not mentioned at all. Except one references, all cited papers here are more than seven years old. Furthermore, some relevant statements are given without any reference, for example at p. 1050 l.20 ("Several authors have indicated [..]") or at p.1047, l.11 ("[..] from earlier studies.")

2.6. Conclusions

The authors conclude, that "the model calculations [..] illustrate the complexity of exchange between the atmosphere and forests especially for reactive trace gases". Well,

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this statement is rather trivial. In my opinion, and in contrast to the conclusions at p.1049,l.11, the model fails to show the major features of NO_2 exchange because these features rely on processes (in-canopy turbulence, biological uptake) which are not well described in the model.

3. More specific comments

p. 1035, l.29-1036 l.3 this point remains unclear

p. 1036, l.7 What means "conceptual"?

p. 1036, l.8 The network for the resistances related to transport as drawn in Fig. 1 (Rc-Rb-(Cleaf)-Ra-(Cair)) is wrong since Cleaf (i.e. the concentration at the leaf surface) must be followed by Rc (see also Hicks et al. 1987, Water, Air, Soil Pollut. 36, 1987). Since the canopy characteristics (for example stomatal uptake) are treated as bulk properties, the use of a boundary-layer resistance makes not much sense.

p.1038, l.15 trunk space up to $0.75 h_{can}$ seems to be very large.

p.1039, l.20-21 This is not clear. The biological uptake of NO_2 seems to be very important and it seems that Rc is just derived from the observed ozone flux and concentration.

p.1040, l.28 Global radiation is not a good estimator for jNO_2 within the canopy because it is biased due to the different attenuation of short and long-wave radiation.

p.1043, l.16-27 Confusing

p.1045, l.2 Confusing

p.1048, l.3 “sum of the fluxes”: what about NH_3, HNO_3, etc.?

Fig.4 and 5 Do not match the captions and are probably exchanged.

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