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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A model-based analysis of foliar NO_x deposition" *by* Erin R. Delaria and Ronald C. Cohen

Anonymous Referee #3

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The authors present a simple 8 layer box model based on gradient diffusion to study NOx retention by forests. The model is tested against data from two contrasting forest environments in the USA. A pine forest and a mixed hardwood forest. The study investigates an important aspect of atmospheric N-cycling that is not well represented in many models. NO emissions from soils below dense/tall canopies are partly retained by the canopy due to NO2 deposition or organic-N formation. This reduces the effect of soil NO emissions on atmospheric chemistry (e.g O3 formation) and N-burden. Given the importance of this topic I am supportive of publication after considering the comments given below.

Major comments:

While I appreciate that the model should be kept simple to allow broad applicability, I

am missing the discussion about limitations of this approach put by these simplifications. Especially, the effect of stability and coupling and decoupling of the forest canopy by coherent structures (e.g. Thomas and Foken, 2007; Sörgel et al., 2011; Steiner et al., 2011). Both effects can change residence times of compounds inside the canopy dramatically if compared to a near neutral stratification and homogeneous turbulence (which are underlying assumptions in the model equations used) and therefore influences the chemistry and the portion of NOx being transported out of the canopy. Furthermore, the authors state that: "Our model is able to closely replicate canopy fluxes and above-canopy NOx daytime mixing ratios observed during two field campaigns, one in a western Sierra Nevada pine forest (BEARPEX-2009) and the other a northern Michigan mixed hardwood forest (UMBS-2012)."

This implies that the model chemistry and physics are right because the model is able to reproduce measurements, but according to the authors "Advection concentrations are set to fit with the observations during BEARPEX-2009 (Min et al., 2014) or UMBS-2012 (Geddes and Murphy, 2014; Seok et al., 2013) and are used to maintain reasonable concentrations (Table S1)." The authors need to explain in more detail how the advection term was used to "maintain reasonable concentrations". In table S1 just one advection concertation is given for each site. Was this used as a constant background value throughout all simulations? How large is this advection term compared to other terms of the budget equation?

According to Fig. 3 and 4 the models have difficulties to reproduce fluxes correctly in the morning hours even if above canopy concentrations show reasonable agreement (below canopy concentrations are also not in agreement in the morning). For the BEARPEX campaign, measured fluxes are around zero until 7:30 while modelled fluxes increase from midnight onwards. This points to the influence of some of the processes not considered in the model (e.g stability and coherent exchange).

To be able to judge the abilities of the model to adequately reproduce the exchange the physical basis of the exchange parametrizations needs to be clearer. The authors

refer mostly to Wolfe and Thronton 2011. Unfortunately, the basic concepts underlying the derivation of the model equations are not discussed in detail in this work. What I could reconstruct is:

A) a relationship for in canopy u_star on above canopy u_star is given and the work of Yi et al. 2008 is cited as a source. Finnigan et al. 2015 showed that some of the assumptions of the work of Yi et al. 2008 are error prone. The relationship given might still be applicable but needs additional justification.

B) from the u_star profile a σw profile is constructed by the relation given by Raupach et al., 1989 that σw= 0.125 u_star.

C) Diffusivities are calculated finally by including a "near field correction factor (r) " that Makar et al. 1999 introduced to the far field diffusion equation given by Raupach (1989) to account for near field effects and to calculate the dispersion in a eulerian framework and not as originally used by Raupach an lagrangian. According to Wolfe and Thronton 2011 τ/Tl is used to "tune" r, with typical values of τ/Tl 1-4. According to Makar et al., 1999 r changes from about $\sim$0 to 0,99 in that range of τ/Tl. Therefore approaching the far field (i.e. diffusive) regime. The canopies considered in this manuscript are comparably low and open. The canopy height is about 20 m and 10 m and the LAI is 3.5 m2m-2 and 3.2 m2m-2 (overstory)+ 1.9 m2m-2 (understory), which might explain the short residence time (< 3 min) in the model and may suffice the choice τ/Tl >= 2 (domination of far field diffusion). However, this may not be easily generalized to taller and denser canopies. Therefore, I am puzzled that the authors cite (on P8) the work of Jacob and Wofsy 1990 as to be "consistent" with the derived residence time < 3 min in this work as JW give the flushing time of the canopy (0-40 m) as 60 min during day and 300 min during night time!

As the residence time is essential in determining the chemistry and the deposition inside the forest this point needs to be further elaborated.

Specific comments:

P6 L4: "and are dependent upon plant physiology." => They also depend on the physical and chemical properties of the compounds. P8 L31: Did the different canopy shapes change the residence times or was this kept constant? Are canopy structure and LAI independent from the residence time in the model? P9 second paragraph: Here again the question how much influence has the "advection correction" here?

Technical comments:

P3 L21: "below the boundary layer" => replace by either "within the pbl" or "below pbl top" P8 L31: "is was" => is Fig.3 and 4: Please use same spacing of time axis for all panels. Makes it easier to compare. Figure 3d): which time intervals are used for "morning" and "afternoon"? Figure 4b): Move NO2 label in graph as the subscript 2 is hidden within the data points.

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