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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Behaviour of tracer diffusion in simple atmospheric boundary layer models" *by* P. S. Anderson

P. S. Anderson

Received and published: 9 July 2007

Response to Interactive comments on "Behaviour of tracer diffusion in simple atmospheric boundary layer models" by P. S. Anderson

Items from the reviewer that have been actioned are in bold. Description of changes are given underneath

General Comments on the resubmitted paper.

Much of the text has been re-written or re-ordered, to simultaneously address the suggestions of the reviewers to both the chemistry and physics aspects

Stephane Bauguitte is now given as co-author to acknowledge his significant contribution to the paper in preparing the NO & NO2 data used in the model validation sections.

Anonymous Referee #1

Received and published: 16 February 2007

General comment:

1. The model would benefit from a treatment of the atmospheric radiative transfer which accounts for the wavelength dependence in the diurnal and seasonal variation of the irradiance reaching the surface. This should be considered when employing the model to investigate case studies, for instance photolysis and photochemically induced release of NOx at high solar zenith angles.

Reply: The section describing the use of surface global radiation as a proxy for the surface tracer flux has been re-written with a major additional section discussing the chain of assumptions that allow global shortwave radiation, G, to be an approximately linear proxy for surface tracer flux, with emphasis to how well these assumptions hold in the specific case of in-snow nitrate photolysis which is assumed to generate NOx.

2. Although the model has been employed to asses NOx measurements at 4 m above snowpack, and as pointed out by the author, the determination of vertical profiles should contemplate the chemistry of these species. In the current version the model assumes a constant loss term but not transformation (recombination chemistry and photochemistry, which should be included in the model to establish trace gas vertical profiles.

2a. The partitioning of NO/NO2 and the total NOx budget may be different at different altitudes within the boundary layer due to the likely inhomogeneous distribution of chemical scavengers and aerosol over Halley Station.

Reply: This aspect is addressed explicitly in the discussion. All the chemical activity that extracts NOx from the air column is encompassed in the loss time scale, tau. This is in situ loss, not advection. The loss at any height is proportional to the magnitude of the concentration (loss = C/tau). It is indeed the case that model tau is constant with

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height, and, whilst there is no a priori reason for this, the model makes this assumption for reasons of parsimony. This last point, that the paper is presenting the behavior of a highly simplified model, is made more explicitly. The paper is addressing the behavior of diffusion in the boundary layer using a “stripped to the bone” model. The interest is that this minimal parameterization still captures the gross general behavior of tracer measurements at 4 m. Such a simple model is ideal for testing, say, the sensitivity of surface measurements to reactions aloft, as are described by reviewer. If no sensitivity is discovered, then this implies that surface measurements are inadequate for validating profiles from more complete boundary layer air chemistry models.

The model presented is a tool to aid conceptualisation, not a solution. Anonymous Referee #2

Received and published: 31 January 2007

General Comments: This is an interesting addition to the CHABLIS special issue with a simple analytical method being developed to predict the vertical behavior of tracers in a diurnally varying boundary layer. Because of the linearity in the model, a number of physical and chemical processes may not be well represented but it does provide a simple conceptual model of the diurnal and vertical variation of NO and NO2.

1. It would be useful to lay out the critical assumptions that underlie the analysis and their potential limitations earlier in the manuscript rather than introducing them sequentially through the text.

Reply: as per comment 1 from the first referee, the section describing the use of surface global radiation as a proxy for the surface tracer flux has been re-written to indicate the limitations of the simplified model. Section 2.1 has been re-written to describe of the model explicitly in terms of diffusion/loss scheme, initial conditions, boundary conditions and forcing terms.

2. In some places the text becomes a bit wordy and could be made more direct and to

the point.

Reply:

sections removed are ů P13114 L2-11

sections re-written are ů P13114 L 29-P13115 L8 (description of Methods Section) ů Section 2.1.

Occasional repeated points have been deleted.

Specific comments:

3. Introduction: the discussion of diffusivity belongs in a technical section

Reply: P13114 L2-11 removed as this is included in section 2.1: Model Description

4. P13112, L23: note that not only altitude differences are important but also the partitioning between latent and sensible heating.

Reply: P13112, L23: has been expanded to make clear that altitude differences cause temperature difference, which then cause, for example, difference in the partitioning of the surface energy balance.

5. P13113, L18-19: Is the assumption of well mixed BL consistent with the results of King et al. who point out that Halley is characterized by much less convection than Dome C.

Reply: The assumption of well mixed is appropriate under neutral or convective conditions, but is invalid under strongly stable conditions. P13113 L 19-22 have been re-written to emphasize this point.

6. P13113, L28: Is it accurate to say that diffusivity is proportional to height (i.e. a linear dependence) rather than more generally a function of height.

Reply: P13113 L28 has been expanded to use "function of" and then to explain that the linear form is an extension of surface layer theory.

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7. P13113, L29: Profiles of "what" evolve to straight lines? (I assume U and T)

Reply: P13113 L29 has been expanded and state to what the profiles are referred.

8. P13114, L26: The discussion of the loss term is critical to elucidate at this point: If I have this right, the observed time series of NO and NO2 together with the diurnal cycle of the actinic flux constrains the diffusion model: the loss term then captures all that is not know about the details of the BL behavior and chemical conversion terms. This needs to be made very clear.

Reply: the constraints (forcing term, G(t), and free variable, τ) are made clearer in section 2.2. The method of tuning τ to get agreement in 4 m NOx maximum is made explicit in a new section, section 2.4: Model validation and tuning.

9. P13115, L23: The diurnal variation of J(t) seems to be critical - care to comment on other locations where there is no diurnal variation?

Reply: Note that J(t) is replaced by G(t), to highlight that the model is forced by the surface shortwave global radiation, which is a proxy for integrated J values in the snow. The validity of using G(t) as a proxy for surface NOx flux is discussed in detail in section 2.2: Forcing Terms. The case for sites with no diurnal variation is covered at the end of section 2.4.

10. P13116, L16-19: I assume that K(z) proportional to z is an initial condition? It might be useful to organize this section around boundary conditions, initial conditions, and forcing functions explicitly. P13117, L4: u* does not change with height by definition.

Reply: Section 2.1: Model Description. has been reordered as suggested, which clarifies, initial conditions, fixed and free parameters, and forcing. P13117, L4: agree, u* is constant; this line has been rewritten.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 13111, 2006.