

## ***Interactive comment on “A revised parameterization for gaseous dry deposition in air-quality models” by L. Zhang, J. R. Brook, and R. Vet***

### **Anonymous Referee #1**

Received and published: 22 May 2003

The paper presents the structure of a "big-leaf" dry deposition model that can be included in air-quality models. A large part of the paper deals with the description of representation in the model of the in-canopy transfer and substrate uptake resistances. The performance of the model is then evaluated by showing a comparison of the simulated and observed O<sub>3</sub> and SO<sub>2</sub> dry deposition velocities for one site and by presenting the range in simulated dry deposition velocities as a function of some input parameters, like biomass (LAI), friction velocity, temperature and relative humidity.

The paper does describe an extension of a model, with as a new feature the parameterization of the non-stomatal uptake of gases, presented in a previous document in Atmospheric Environment, 2002. The dry deposition model itself doesn't seem to

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contain a substantial new feature compared to other already existing dry deposition models, also described by the authors in the introduction. The additional value of introducing in more detail some of the non-stomatal uptake processes and stomatal uptake can be questioned since the discussion about the selection of other resistances (e.g., soil/snow resistance) and land cover parameters (snow cover, soil moisture) is indicating that there are many other limitations by the assumptions being made. This is due to the fact that the required input parameters are simply not available (soil wetness, snow cover), or due to the lack of observations to motivate the selection of specific values. Most of the selected values are also just presented without a critical discussion about these limitations and on what physical/biological/chemical bases these have been selected.

Specific comments:

Pp 1779; "especially since stomatal uptake only occurs ....dominates over non-stomatal uptake". This is assumed to be true for gases like O<sub>3</sub> and SO<sub>2</sub> for dry vegetation canopies but there might a selection of other trace gases for which non-stomatal uptake can be large, and even for gases like SO<sub>2</sub> and O<sub>3</sub>, the role of non-stomatal uptake is still not well understood to make such a generalization.

Pp 1779; "thereby excluding the effects of meteorology". There are dry deposition models available that include for sure the role of meteorology for the non-stomatal resistance. Any models that for example include the term for the in-canopy turbulent transport, which is also being described later on in this paper, consider the meteorology to some extent. What is really missing in this discussion in this paper in general is the possible role of aqueous-phase chemistry for the non-stomatal uptake. Since a part of the model description deals with the role of canopy wetness for dry deposition, I guess it is also essential to discuss to some extent that the non-stomatal uptake for wet canopies is expected to be affected by the processes occurring in the water film covering the leaves, e.g., the co-deposition of NH<sub>3</sub> and SO<sub>2</sub> and the controlling role of the pH. This parameter is not included in the developed parameterization but it should

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at least be mentioned as being a potential key component for the dry deposition over wetted surfaces.

Pp 1780; "and possibly other factors that were not measured". That statement is too vague; probably there the authors could include a short discussion about the previously mentioned potential role of aqueous-phase chemistry.

Pp 1780; "with adjustments to some parameters", what is being adjusted here? The following model description shows that there are many parameters included, with all quite some uncertainty, which means that adjustments can result a large-variety of calculations. The adjustments and the motivation to make these should be shortly mentioned.

Pp 1780; "very good agreement between model results and measurements"; very good agreement between what? SO<sub>2</sub> dry deposition fluxes, velocities, or also other gases, O<sub>3</sub>?

Pp 1780; "Choosing this 26-category.....models developed elsewhere". Because of what? It should be addressed why the distinction of these 26 categories would benefit other air-quality models. Is it because these categories are spatially resolved at a high resolution, or because of the fact that for these categories the properties like LAI, surface roughness, etc., are well established? This is actually a quite essential issue since many of the limitations of the surface trace exchange models are mainly related to the land cover characterization.

Pp 1781: "The uncertainties in Ra... are small". This statement has also already been criticized by one of the other reviewers stating that this is not true. There can be quite large differences amongst the models in these parameters, which is also relevant to this study since not only the species for which the surface resistance is generally the controlling term (O<sub>3</sub>, SO<sub>2</sub>) are included but also species like HNO<sub>3</sub> and H<sub>2</sub>O<sub>2</sub>, which VdŠs are mainly controlled by Ra and Rb.

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Pp 1782: The discussion around equation 4 is basically reflecting a (modified) version of the method proposed by Wesely, 1989. That reference should at least be included to give some of the credits of the concept of scaling the various resistances with the SO<sub>2</sub> and O<sub>3</sub> resistances. In that context the term "scaling parameters" should also be more specifically addressed since these probably reflects, similar to Wesely's approach, the solubility and the reactivity.

Pp 1782/1783: The definition of the fraction of stomatal blocking is presented without any discussion on what criteria the values and the dependence on solar radiation has been selected. In the 2002b paper this is probably addressed in more detail but it should be at least included if these values are defined using any experimental data or rather selected arbitrarily, also since you would rather expect a dependence on a parameter like rainfall.

Pp 1783; I have shortly checked the equations to calculate the stomatal resistance and guess that a large part of this can be removed by simply referring to the Zhang 2002a reference and some references of other studies that have also applied similar parameterisations including the light dependence and the attenuation functions that correct for temperature, vapour pressure deficit and water stress effects.

Pp 1784: The dependence of the leaf water potential on solar radiation in equation 6f should at least be discussed in a little more detail since the link between these two parameters and the selected constants is not obvious. In other dry deposition models, where soil moisture is available, this parameter is being used to determine the water stress term. In many models, soil moisture might not be available directly from the meteorological model but there are large-scale soil moisture databases available which could be used as an alternative to include this term.

Pp 1784: The discussion about  $R_{ac}$ , the within-canopy turbulent transport to the soil surface should at least include some references to previously done studies that have proposed equations for this term quite similar to that presented in equation 7 (Erisman

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and Van Pul, Atmos. Environ., 28, 2595-2607, 1994, and Wesely, 1989). This also brings me to the point about the role of the canopy height, which is included in the alternative relationships. Why did the authors not include this parameter since one can expect that when one includes 26 very different land cover types with canopy height ranging between  $< 0.5$  m up to  $> 20$ -40 m that this might be an essential parameter. Some discussion about this should be included.

Pp 1785: I agree with the statement that information on the Rsoil is quite limited but the sparse information available suggest for example that O<sub>3</sub> uptake by soils is probably controlled by soil organic material (enhancing the removal) and soil moisture (limiting the removal through covering the reaction sites, and gas-transfer), which is consistent with a selected lower value for vegetated surfaces (high soil organic content) compared to the non-vegetated value. Is this also the motivation of the authors to select these values, some motivation would be appreciated. Concerning the statement "based on previous studies", could you add some reference(s)?

Pp 1786: The SO<sub>2</sub> soil resistance; the discussion about the role of soil moisture for the RsoilSO<sub>2</sub> shows that some of the dependencies are selected very arbitrarily. Yes, tropical forest can be expected to be wetter compared to the desert but there might other parameters which play a key role, like the soil pH, with the large differences between the alkaline dessert soils and the acid forest soils. Also, the potential role of uptake by the litter layer, which can be quite thick for tropical forest, should be included.

Pp 1786: "relative humidity (as a fraction)", what do you mean here with fraction?

Pp 1787: The presentation of the temperature corrections of the resistances in equations 10a/b should also include some of the references of previous work (Wesely, 1989, Erisman)

Pp 1787: Is the snow fraction really usually not available in meteorological models? In models like the ECMWF is it available. Offline fields from such models could be applied to constrain the dry deposition models.

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Pp 1788: "For some surfaces a constant  $z_0$  value...is given". Also here it would be nice to see a short discussion on what basis constant versus a range of  $z_0$  values have been assigned.

Pp 1789: It is nice that the paper includes a comparison of modelled and observed O<sub>3</sub> and SO<sub>2</sub> dry deposition velocities (It would have been even nicer if some additional sites would have been selected, which data have not been used to develop the parameterisations included in the model). A remaining question is how the model has been constrained for the model comparison. What kind of input data has been used? The local observed micro-meteorological parameters or the output of the regional scale meteorological model?

Pp 1789: There is a discussion of the fact that the model does not reproduce the observed diurnal cycle in the O<sub>3</sub> dry deposition velocity, with a maximum value in the morning and lower values in the afternoon. This somehow suggests to me that probably in the afternoon there might have been an increased role of the water pressure deficit or for example a reduction in the net radiation due to increased cloud cover. If we assume that the model uses the observed net radiation then the misrepresentation of the diurnal cycle could be due to the combined effect of an underestimation of the vapour pressure deficit effect (which would increase  $R_{st}$  in the afternoon) and an underestimation of the maximum stomatal uptake. This could be included in some short discussion of the comparison.

Pp 1790: "and estimated the typical range of  $V_d$  values that can be expected"; What do mean with this? It is much too vague and should be explained in more detail. I guess that you compiled as many available measurements to come up with the range of  $V_d$  for various land cover types and conditions.

Pp 1790: "we show results for only 9 species in Table 2". Why did you select these 9 species? Is there any special reason to select those and of so those motivation should be given.

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Pp 1791: You are referring to the species NO<sub>2</sub> and NH<sub>3</sub>. In the paper there is not any mentioning of the possible role of the compensation point for surface exchanges of these species. The motivation to ignore this in this study should be shortly addressed.

Pp 1792: "Under wet conditions, V<sub>d</sub> values are even larger". You mention that the HNO<sub>3</sub> V<sub>d</sub> is controlled by the aerodynamic resistance and then the question arises if the V<sub>d</sub> increases for wet conditions due to a change in the aerodynamic resistance due to the fact that the meteorological conditions are different or is there still a small further decrease in the already small surface resistance? You would actually expect that with rainfall the aerodynamic resistance would generally increase, related to the generally observed drop in turbulence intensity.

Pp 1793: in line with the comment of one of the other reviewers; if you would have shown the performance of previous model versions, or alternative models that do not include the proposed parameterisations, then it would be easier to convince that with this model you indeed calculate dry deposition velocities which agree better with the observations. Now it is actually open to reader to believe that this is really true and an invitation to colleagues that have developed/applied alternative dry deposition models to perform the same model comparison and to see if it is really an improvement.

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Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1777, 2003.

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