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

L. Zhang, J. R. Brook, and R. Vet

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Reply to Referee #1:

General comments

We thank the reviewer for a detailed and critical review. We agree with this reviewer on the aspects that this model is an extension of our earlier studies, the concept of the big-leaf model is not new, the approach used here is not an innovation compared to other existing big-leaf models. However, the paper presents substantial new material which is important and necessary for colleagues in the dry deposition community to adopt the model if they want to. We have already been asked by several researchers in North America and Europe for information in this update and feel that the paper makes an important incremental step forward in modelling dry deposition. More detailed discussions and reply to every review comments are presented below.

The motivation of this paper is to provide a complete description of a model that can be used by different air-quality models. As we mentioned in the introduction, The Wesely (1989) model is used in most air-quality models and now new knowledge is available to update this kind of model, especially for non-stomatal uptake and for wet canopies. Shortly after the O3 non-stomatal resistance paper (Zhang et al., 2002b) was published, we received e-mails from colleagues in and outside Canada enquiring as to how to extend the non-stomatal resistance to other species and to other land types. We therefore see that it is important to document in the literature how to extend our earlier studies to other land types and species and also update some other issues we think that can be improved (wintertime resistance parameters, the handing of input parameters such as LAI and z0). We know that readers will have difficulty extending the non-resistance formula presented in our earlier study to other land types and species since only O3 over 5 different land types were discussed in that paper. The first application of this newly developed non-stomatal resistance parameterization has been done by CHRONOS group in Canadian Meteorological Center in Montreal and the results are promising (Robichaud et al., 2003, http://www.meds-sdmm.dfo-mpo.gc.ca/cmos/Congress2003/Abstracts/177.html). Note that Robichaud et al (2003) adopted the non-stomatal resistance formula based on the draft paper of the present study. Thus, the current paper will benefit our colleagues who might want to try this new formula. This is also the reason why we chose a widely used land use scheme (BATS) in the present study.

We agree with the reviewer on the statement that limitations exist due to many input parameters being unavailable in air-quality models, but weą́rd like to point out that many newly developed air-quality models have these parameters available from meteorological drivers (e.g., Canadaą́rs AURAMS and CHRONOS driven by GEM). We admit that many values are presented without a critical discussion about limitations in this paper, but we think that the physical/chemical bases are clear for most input parameters. Some discussions have also been presented in our three earlier publications (as explained in the paper) which we do not want to repeat here.

We also agree with the reviewer that big uncertainties exist in this newly developed non-stomatal parameterization. However, we feel strongly that this new scheme is an improvement compared to earlier models, most of which only use constants for cuticle uptake. We feel that limitations and uncertainties should not stop us from using, evaluating and improving the parameterizations, especially with more and more data showing the same relationship between non-stomatal resistance and meteorological conditions (see references cited in the paper). Also, publication of the new approach can stimulate more discussions, evaluations and improvements within the dry deposition and air-quality modelling communities. For example, after publication of the new non-stomatal resistance parameterization, a few groups around the world are trying to evaluate and extract similar relationships between the non-stomatal uptake and meteorological conditions (Massman, 2003, personal communication, manuscript also submitted; Loubet, 2003, personal communication). Finally, we have taken the approach of making the best estimations possible despite known limitations and uncertainties. For example, Wesely et al. (1984, JGR) empirical formula for sulphate deposition was only based on one siteąŕs data (grass) and this formula was then used for all land types and even for other fine particle species in many air-quality models. Our approach is a major advancement.

We hope that the foregoing points make a compelling argument for the publication of this paper.

Specific comments:

P1779: We will correct the statement by adding ąőfor some speciesąŕ into the sentence to make it clear based on the reviewerąŕs comment.

P1779: We agree with the reviewer that any models that consider in-canopy turbulence have considered meteorological effects, but weąŕd like to point out most of these mod-

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els are multi-layer models, not big-leaf models. A few big-leaf models also include some meteorological effects, but limited to in-canopy aerodynamic resistance (e.g., Erisman et al., 1994, cited in the paper). We have some discussions on aqueous-phase chemistry in Zhang et al. (2002b), and we will mention more in the revised paper based on the reviewerąŕs comments. Since we have a lot of discussions on SO2-NH3 co-deposition in Zhang et al. (2002a) (Page 544), we will just mention it in the revised paper based on the reviewerąŕs comment.

P1780: We will mention aqueous-phase chemistry involved in wet surface deposition in the revised paper.

P1780: These parameters are reference values for resistance components. We will revise the text to make it clear.

P1780: Deposition velocity is shown in the Figures. We will make this clear in the revised paper.

P1780: Because this Land Use Scheme is based on BATS, a widely-used LUS in North America, related parameters as well as a high resolution data base all make it more easily adoptable to different air-quality and climate models. We will discuss this thoroughly in the revised paper.

P1781: Since this reviewer refers to an other reviewerąŕs comments on Ra, we offer the same answer provided to the other reviewer. We have some discussions on this issue in Zhang et al. (2003, A.E., 37, 2941-2947) and we will not repeat in this paper.

P1782: Equation (4) is not developed in this paper, but first appeared in Zhang et al. (2002b). In that paper, we stated in many places that the formula is similar to Wesely (1989), and the basis of choosing two scaling parameters is exactly the same as the one used in Wesely (1989). The only difference is that we use a slightly different form and we gave the reason why we used a slightly different formula. We also acknowledged our very helpful discussions with Wesely on this issue and we cited Wesely

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(1989) at the very beginning of the introduction of this paper. We think that this is sufficient and would not like to trace back all original references for most equations.

P1782/83, As stated by the reviewer, we provided some discussion on stomatal blocking in Zhang et al. (2002b) which we do not want to repeat here. We do not think that the rainfall rate need to be included since the rain intensity is only important at the very beginning of the rain event. Once the leaves are wet, the rainfall rate will probably have little effect.

P1783: We agree that formulas have been presented somewhere else. That is why we only listed these equations here without any detailed discussion. We think it is necessary to have these equations here so we can explain all the necessary parameters presented in Table 1. Additionally, there is one typographical error in one equation of our earlier paper where most of these equations were first discussed (Brook et al., 1999, see more discussions in Reply to Reviewer #3) and would like to have the correct version printed here.

P1784: Since the model for stomatal uptake is based on previous studies (e.g., Sellers et al., 1996, Brook et al., 1999), all the important information can be found in the references we cited, and we chose not to repeat information that is already published. However, we will add some discussion based on this reviewerąŕ s comment. This reviewer (see previous comment) suggests omitting all equations and to refer them to other references. We chose to avoid detailed discussions about these equations, including the dependence of leaf water potential on solar radiation, since we are not modifying them. But, we think it is necessary to list important equations so we can present necessary parameters and readers can have a complete picture of the model. Researchers who would like to do further investigation will certainly find more details by looking at references cited here.

P1784: We cited two references (Erisman et al., 1994; Wesely, 1989) when we first presented the big-leaf model in Zhang et al. (2002b). Both of these references are

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also cited in the present paper, though not at the place of discussing Rac. To our knowledge, Rac was used in several of Erismanąŕs papers including the one pointed by this reviewer. If we remember correctly, the formula was not first developed in the paper suggested by this reviewer, nor was it in the paper we cited. Since all these papers have this formula discussed, we chose to cite one reference to Erisman that not only has Rac discussed, but also discussed many other parameters (e.g., sonw resistance) that has been used in our study. Again, as mentioned above, we would not like to trace back all original references and cite every one of them. We chose most important one so the readers can find more information quickly if they like to do further research.

The effect of canopy height is implicitly included in friction velocity, roughness length and more importantly, the reference Rac0 values in the present study. As can be seen, Rac values are larger for higher canopies than that for lower canopies. Since both in-canopy (Rac) and cuticle resistance (Rcut) are affected by the intensity of turbulence, or u*, which is certainly affected by canopy height and other factors, we think it is a better way to implicitly include u* , rather than canopy height, in both Rac and Rcut.

P1785: We thank the reviewer for this detailed explanation. We chose input parameters first based on measurements available, then based on theory. Although we had some information on this topic, the reviewerąŕs comments provide a clearer picture. We will add some discussion based on the reviewerąŕs comments in the revised paper.

P1786: We admit that the soil resistances for SO2 are somewhat arbitrary, but these values are based on a review of available measurements. If we do not have information on soil type, pH, etc, we cannot provide more accurate values than those presented here. The soil resistance for forest includes the overall uptake of soil and the litter layer. We would also like to point out that the overall uptake is not very sensitive to the soil resistance input parameters for forests mainly due to the large in-canopy aerodynamic resistance. However, for very short canopies and bare soil, soil resistance had to be chosen carefully and reflect published measured values.

P1786: We mean RH in the range (0.0-1.0), not in percentage.

P1787: We will add these references.

P1787: The reviewer is right in that snow fraction might be available in some meteorological models. The sow fraction provided by meteorological models represent grid-averaged snow fraction. Here we provided formulae that can estimation snow cover fraction for both leaves and underlying surface, and we think it is better than using grid averaged value. We realized that there are uncertainties in these formulae.

P1788: For canopies that change LAI and height, z0 values should be changed with season; for evergreen forests, z0 might not change much with season. We will add some discussion based on this reviewerąŕs recommendation.

P1789: One other reviewer has raised a similar question. Since most data have been discussed in our two earlier papers (Zhang et al., 2002b, 2003), we do not want to repeat too much on model comparison with measurements. We will add some more information on the local meteorological data used in the model comparison in the revised paper.

P1789: We will do some sensitivity tests (though not planned to be included in the paper) so we can add some discussions. Vapor pressure deficit seems to play a role for the different diurnal cycles of forests and crops.

P1790: The reviewer is right in that these values represent ranges of published values for different land types and several common species (SO_2, O_3, HNO_3, NO_2, PAN as discussed in the paper).

P 1790: These 9 species are most commonly considered in air-quality models, especially models developed at an earlier time (e.g., ADOM, RADM), though more and more species have been added in. We can certainly extend the table to include more species in the revised paper.

P1791: We will add some discussion on compensation point based on the reviewerąŕs

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comment.

P1792: Rc is small for HNO3 most of the time, but not negligible. 10-20 sm-1 is a reasonable estimate for vegetated canopies. So, under stable or close to neutral conditions, Ra does dominate. However, during unstable conditions, Ra can be quite small (e.g., $<$ 20 sm-1), thus changes in Rc can still be significant for increasing Vd. We will modify the test to make this clearer.

P1793: We will add some comparison results with our previous model version for wet canopies. We would also appreciate any colleagues evaluating this model, especially the non-stomatal parameterization, using their data at locations different from our data, so improvements can be made. We have a stand-alone version that can be made available to interested colleagues who want to evaluate this model.

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

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