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Interactive Comment

Interactive comment on "Development of a parameterization scheme for calculating dry deposition velocity of fine, coarse and giant particles" *by* L. Zhang and Z. He

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Thank you for your interest in this paper and for providing your valuable insights. Please find our responses below.

RC – Original Comments; AC – Authors Comments

RC - The discussion paper presents an attempt to make a simple parameterization for particle dry deposition in atmospheric transport models. Unfortunately, we faced several confusing points in the paper, which seem to question its value.

AC: We welcome criticisms from the community and hope our explanation can clear your confusion. We also hope you could further advance the science in the near future on this important scientific topic.

RC - 1. The Eq.1, which underlies the considerations, is wrong and has been disproven in several publications. Already Slinn and Slinn (1980) derived a more appropriate equation for dry deposition velocity with an artificial virtual resistance, which, unlike the Eq.1, satisfies the mass conservation requirement between the surface and reference height. However, it did not resolve the principal problem demonstrated by Venkatram and Pleim (1999). The essence is that the electrical analogy is not applicable to particles with noticeable sedimentation and cannot be made to comply by any means. This paradigm is valid exclusively for gases. With correct solutions available from recent publications, partly quoted by the authors, reviving the obsolete approach seems confusing.

AC: We do not think Eq. 1 is wrong; it is just a simple empirical approach for a very complex scientific issue. Particle dry deposition involves many complex processes which are difficult to describe accurately using exact mathematical formulas. This simple approach can describe the air-surface exchange flux for aerosol particles as a first-order approximation, as was also admitted in your recent publication (Kouznetsov and Sofiev, 2012, JGR, 117, D01202). A first-order approximation is good enough considering that too many uncertainties still exist in the handling of this process. You mentioned that Slinn and Slinn (1980) derived a more appropriate equation than Eq. 1, but that equation only applies to water surfaces. In fact, W. Slinn himself further developed Eq.1 for vegetated surfaces (Slinn, 1982, Atmospheric Environment, 16, 1785-1794). The model described in Zhang et al. (2001) was an extension of Slinn (1982). Despite that Venkatram and Pleim (1999) raised the principle issue, they continued using the resistance analogy approach in their chemical transport models (e.g., CMAQ and many other models).

We appreciate the effort you have committed to the development of a more sophisticated model (JGR, 2012). We agree that your model could be used in 3D atmospheric

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dispersion transport models. However, researchers in the monitoring community need a much simplified algorithm, such as the one presented here. It should also be realized that a more complex model, while is useful for sensitivity studies identifying dominant factors and for further development of more practically-applicable models, does not warrant more accurate results due to more input parameters involved (which had potential of introducing more uncertainties). For example, early intercomparison studies showed that detailed multi-layer dry deposition models had similar uncertainties to the very simplified big-leaf models (Petroff and Zhang, 2010; Schmede et al., 2012, AE).

RC – 2. The new parameterization is shown to reproduce the results of old Zhang (2001) approach. However, that scheme has about as many fitting parameters (4 parameters x 15 landuse categories x 5 seasons, with some omissions) as there are experimental data points generally available from wind tunnels and field studies. How can that number of parameters be verified? Moreover, comparison made against a few wind-tunnel observations suggested strong over-estimation of dry deposition velocity – see Kouznetsov Sofiev (2012), quoted by the authors. Unfortunately, we are not aware about any comprehensive evaluation of Zhang et al (2001) parameterization. Thus the agreement of the new parameterization with it can hardly be a justification. Measurements have to be used instead.

AC: The original scheme of Zhang et al (2001) defined 4 parameters x 15 landuse categories x 5 seasons. The scheme was later extended to 26 LUCs using the approach described in Zhang et al. (2003, ACP, 3, 2067–2082). One major modification is to remove the definition of seasonal category. The four major LUC-dependent input parameters were interpolated from their respective minimum-maximum values and LUC-dependent Leaf area index values (LAI). When generating Vds data which was the basis for the new empirical formula, the mode was run for 365 days (for each u* value) which cover all different LAI conditions. That is why LAI is also a parameter in the new algorithm.

When the model of Zhang et al. (2001) was first developed, it was deliberately parameterized to produce larger Vd than those generated from wind-tunnel measurements. The rationale was that more recent field flux measurements suggested much higher Vd, especially over rough tall surfaces (e.g., forests),tan those from the wind-tunnel or chamber studies. This was described in detail in Zhang et al. (2001). It should be noted that earlier flux data were for bulk fine particles.

With the increase of flux data for specific particle sizes, some comparison have been done for existing size-resolved models. For example, Petroff and Zhang (2010) suggested that the model of Zhang et al. (2001) produce reasonable Vd for all particle sizes over tall vegetated canopies, but over predict Vd for ultrafine particles over smooth surfaces. Since ultrafine particles contribute a very small fraction to the total of bulk mass, this overestimation in Vd should have minimum impact in the parameterized bulk Vd. One thing we like to stress is that wind-tunnel measurements my not represent the real-world situation. Please also refer to our replies to the two reviewers regarding the point of model evaluation using flux data.

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