Geosci. Model Dev. Discuss., 3, C437–C439, 2010
www.geosci-model-dev-discuss.net/3/C437/2010/

Geoscientific
Model Development
Discussions

Interactive comment on "Development and validation of a size-resolved particle dry deposition scheme for applications in aerosol transport models" *by* A. Petroff and L. Zhang

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Received and published: 13 October 2010

The authors are presenting a size-resolved aerosol particle dry deposition model simple enough for application in large-scale numerical models. Clearly, as indicated also in the paper, such a model with sufficient simplicity and capability of capturing most significant features such as dependencies on surface type as well as particle size is currently missing. Additional source of uncertainty in dry deposition modelling to vegetative canopies is dependence on atmospheric stability. The presented model takes into account the influence of stability indirectly through dependence of aerodynamic properties above the canopy. Considering current (limited) knowledge and experimental

evidence on aerosol dry deposition into vegetative canopies and canopy flows during different stratification conditions the constructed model combines up to date theoretical knowledge with relative simplicity. The ability of the model to represent deposition to ground surface in the form of its asymptotic limit is an additional nice feature of the model. Thus the article deserves certainly publication in GMD but could be improved by considering following suggestions.

The presented model is based on simplification of more complex deposition model into vegetative canopies with the aim to achieve the same descriptive power by introducing coefficient values, that aim to fit the results. Such coefficients and therefore model "versions" are derived for 26 land use classes. A reader being not familiar with global scale modelling deserves short explanation why exactly 26 LUC's were used and/or are there other standards of land use classification in global modelling communities. In addition, treatment of urban environment as canopy may look weird (again for those not being familiar with global modelling). Although the authors recognize that such treatment is open to criticism (P1333 line 7-10), it deserves a short explanation of what is meant by "urban trees with LAI 2" and how the concept of GEM model combines emissions (that usually dominate over deposition except at larger vegetative areas such as parks) with modeled deposition.

P1323 lines 25-26 and eq. (7) P1324. What boundary conditions have resulted in such functional form of the extinction coefficient as a function of stability length above canopy? Presumably different assumptions can lead to different functional form. Either reference or explanation needed.

Figure 1 is little informative in my opinion i.e. different lines do not carry information separately and the same information would be given also by presenting only variation boundaries for each color. Instead, consider presenting deposition velocity size dependence separately for each deposition mechanism and deviation (relative error) between current model and 1D-model also separately for different mechanisms (for some chosen configuration(s) of particular interest). Such presentation would be probably more

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illustrative and helpful in understanding. Fig. 1 in present form could be summarized in text by a few sentences.

Interactive comment on Geosci. Model Dev. Discuss., 3, 1317, 2010.

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