

***Interactive comment on* “Evaluation of five dry particle deposition parameterizations for incorporation into atmospheric transport models” by Tanvir R. Khan and Judith A. Perlinger**

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I received the second review by e-mail as there was some issue with posting it. Therefore I use an Editor comment for this purpose. This is NOT my personal review but that of an independent reviewer.

Comments on mss by Khan and Perlinger

This is a well written paper, with conclusions that are important and with which I agree. The introduction gives one of the best overviews of the development of dry deposition formulations that I have come across. The text introduces a new (to me) method for

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ordering the sensitivity of a model output to the various input properties, and concludes (no surprise) that u^* is a key property. Since u^* is a measure of the level of turbulence affecting the eddy transport of all atmospheric properties, this is an expected result. However, it leads us further down a path that seems (to me) to have no foreseeable end, since u^* is among the most difficult of all atmospheric properties to quantify, both instrumentally and even more so by using available models or even tabulations. The following observations arose during my reading of the mss.

1. The first sentence of the introduction could be omitted.
2. Line 33. Suggest omit “accurate.”
3. Line 36. “particles” – plural.
4. Line 41. The time scale as stated is that which relates to gravitational settling. It is not appropriate for small particles, for which the relevant time scale needs to take into account the depth of the layer in which the particles are mixed and the various resistances associated with their deposition. In any case, I do not see the need to refer to the time scale here.
5. Line 49. Terrain complexity is a central factor that none of the existing formulations address.
6. Line 57. “. . . there remain . . . “ singular.
7. Line 66. There is a recent paper in JGR (lead author – Hicks) that might shed some light on the way in which data have or have not agreed with model predictions.
8. Lines 83-85. My immediate reaction is to wonder why the benefits of multi-variable partial correlation are not mentioned here. This attributes the variance among the contributing factors so that an ordering becomes obvious. The implication of the text is that a single correlation analysis is not appropriate because of the various covariances that could contribute. To my mind, this is precisely what a multiple regression is intended to resolve.

9. Line 96 and onwards. This is the best description of what the models actually assume that I have come across. Congratulations to whoever it was who did the grunt work. Well done! The detail elevates my own fear that the modelers are constructing simulations that reflect articles of faith rather than of evidence. I appreciate that it is comparative examinations like that of the present text that might well shed some informed light on the benefits of model complexity. However, I have yet to see a reason not to start with something simple and add complexities as observations then warranted.

10. I recommend using readily apparent subheadings for each of the model descriptions.

11. Section 3 – Methods. I note that the selection of data requires that each dataset contains measurements of all of the variables whose relevance is IMAGINED by the modelers. I consider this to be rather limiting. Surely, the selection of variables measured in field studies is a measure of what the experimentalists thought to be important factors. Examination of this might constitute a Delphic approach to determining what is important. I note, also, that requiring measurements of such things as leaf area index (LAI) assumes an application to situations in which a vegetated canopy of some specific kind dominates. For example, it has long been known that the hairs on leaves can play a role. How are these considered in the context of a LAI?

12. The definitions and roles of such properties as the zero plane displacement and the roughness length remain subjects of sometimes heated discussion in meteorological circles. Using these concepts rather cavalierly in dry deposition models seems a poorly justified extrapolation of a very poor basic understanding. I would welcome a more kindly-phrased reflection of this theme somewhere in the present text.

13. Line 345. The wind tunnel studies CANNOT be considered in the same context as open-water deposition. One cannot get 2-m white-capped waves in a wind tunnel.

14. Line 359. “. . . were used . . .”

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15. Equations (63) and (64) are no more than expressions that quantify how much the averages differ. Why go into all of the mathematics when the description is so simple. And why not simply call it the amount by which the ratio of the averages differs from unity. (Defining BNMBF seems rather extreme. In practice, the distributions involved are likely to be log-normal or close to it, and hence the arithmetic average is an incorrect concept.)

16. At this point, I encountered Tables 1 and 2. Some comments . . .

- What time of day are the numbers meant to represent?
- What do the uncertainty ranges mean? If they are meant to refer to long-term ensemble averages based on some other variable, then I would agree with some of them.
- The humidity assumption of 80
- However, I note that nighttime is indeed assumed: the value of L is positive. A value of 50 m is commonly VERY stable.
- I could argue with all of the other “base values,” all of which represent the environment that some investigator saw out his window. For example, grass 50 cm tall seems very unlikely. At that stage, it should have been harvested, grazed or mowed.
- The use of a one-sided LAI worries me a little. Particles will deposit to both sides of leaves. Why use the one-sided value?
- In Table 2, I hope that the separate listing of ranges for u and u^* does not mean that these are allowed to vary independently. The friction velocity is usually from 4
- The variation of L from 10 to 100 m is very constraining, since once again the analysis represents conditions that would often be considered uncommonly stable.
- The roughness length quoted as the lower limit for snow/ice is 0.02 mm. As far as I know, this is less than aerodynamically smooth, and is therefore unlikely (some would say impossible). Please be careful.

- In Table 3, I note that all of the field programs are for daytime conditions, yet the Base Values tabulated indicate an assumption of stable (nighttime) conditions. Something must be wrong. A suitable explanation would be that the sign for L is incorrect

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