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Interactive comment on "Development of a new semi-empirical parameterization for below-cloud scavenging of size-resolved aerosol particles by both rain and snow" *by* X. Wang et al.

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General Comments:

The authors use a compilation of theoretical aerosol scavenging coefficient formulations for below-cloud scavenging by both rain and snow to develop a size-resolved parameterization for these coefficients. The authors use numerous combinations of theoretical formulations for raindrop-aerosol and snow-aerosol collection efficiency, rain and snow size distribution, and fall velocity to generate 320 scavenging coefficient profiles for rain and 168 for snow for each of 100 aerosol size bins. The 90th percentile of these formulations is then used to develop a parameterization as a function of aerosol size and precipitation rate. The end result is a convenient parameterization that could be readily applied in chemical transport models. I found this study to be scientifically

worthwhile. As the authors point out, this is an important process related to aerosol removal from the atmosphere, but is associated with considerable uncertainty. However, as noted at the end of Section 3.1, the new parameterization is not associated with any less uncertainty than existing formulations. My major concerns with the manuscript in its present form are that 1) it does not give a reader a clear indication of the conditions when the new parameterization might be most suitable and 2) the use of the 90th percentile of coefficient values is not well justified, nor are alternatives examined. While theoretical and measurement based scavenging coefficients are expected to differ, it is not well established that theoretical values in the 90th percentile should give a more accurate representation of reality simply because they are closer in magnitude to the measurement based values, as outlined below. These concerns and below comments should be satisfactorily addressed prior to final publication.

Specific Comments:

1) The third paragraph of the introduction states that the upper range of available scavenging coefficient theoretical formulations are thought to be more realistic because they are closer to, while still smaller than the field derived estimates. While I concur that the theoretical formulations and field measurements for the scavenging coefficient are expected to differ, the magnitude of this difference in the case that the theoretical formulation was perfect is not well established since the measurement based values do include processes (including storm dynamics) not expected to be included in the theoretical formulations. Thus I am not convinced that the development of a parameterization based on the 90th percentile of theoretical formulations is fully justified. Are the authors able to provide any additional support for this approach, particularly for snow, or at least give a more thorough consideration of alternative assumptions?

2) Does the 90th percentile of theoretical formulations for both rain and snow correspond primarily to a certain combination of the existing theoretical formulations? This was not clear in the text but would be helpful to know the combination used.

3) The last paragraph of the methodology states that steps 1 and 2 of the parameterization development were only conducted for precipitation at 1 mm hr-1. If other precipitation rates were considered, would this change the values retained as 'realistic'?

4) The first paragraph of Section 3 states that a fixed ambient temperature and pressure was assumed. How might this influence the performance of the parameterization under different conditions?

5) The definition of the raindrop size (page 5907, line 19) seems unusual. Hydrometeors of 1 um in diameter would usually not be considered raindrops.

6) The first paragraph of Section 3.1 states that the component parameter formulas come from a wide range of rain types. Does the choice of the 90th percentile yield a selection of one rain type primarily? In that case, what rain type was primarily used on the parameterization development? This information could be helpful to the reader in understanding the parameterization.

7) The agreement of the 50th to 90th percentile of theoretical formulations with the experiment of Sparmacher et al. (1993) seems to suggest that a parameterization based on the 50th percentile might be equally reasonable. Do the 50th percentile values correspond to any certain rain type or combination of the input formulations that might lead to a preferential choice of the 50th percentile under certain conditions for alternative parameterization development?

8) The second paragraph of page 5912 discusses the abrupt change in the A(d) and B(d) values at particle sizes between 1 and 2 um. Are you able to provide any insight on the physical basis for the abrupt shift?

9) The last sentence of Section 3. 1 states that the uncertainties associated with the new scheme should not be larger than the uncertainties with the existing parameterizations (which are in the order of magnitude range). This is fair enough – the new

scheme does not accomplish any reduction in uncertainty in the scavenging coefficients but does provide a convenient parameterization as a function of quantities that are predicted by a global model. What needs to be clear to the user of such a parameterization is whether the parameterization favors a certain rain type or set of physical conditions. Could this information be presented more explicitly?

10) Section 3.2 deals with the parameterization for the snow scavenging. My previous point for rain applies also to the presentation of the snow scavenging parameterization. Is the parameterization (developed by choosing the 90th percentile values) most applicable for certain types of snow and habits?

11) Section 3.2 states that no unrealistic values were found and excluded from the ensemble of scavenging coefficients. Figure 4a does appear to show clusters of the lines around two distinct minima (similar to Fig 1a). Can the authors give explanation why all values were considered realistic for Fig. 4a but not for Fig. 1a?

12) I found the justification of the use of the 90th percentile to be weaker for snow than for rain since there is even less experimental data available (Fig 4b compared to Fig 1d) – thus additional information to characterize the conditions most applicable for the snow parameterization (point 10 above) would be particularly helpful.

13) Also, how does the exclusion of unrealistic values based only on one precipitation intensity for snow influence the results? Have you checked other precipitation intensities?

14) Section 4.1 gives a comparison with previous B(d) values – perhaps a table might helpful to see these values more readily. Some of these previous values are related to field measurements and others are related to theoretical formulations – would you expect to be able to compare equally between the two? Also, are you able to make any comparisons related to A(d)?

15) Related to the comparisons in Section 4, if you formulated the parameterization

based on the 50th percentile or any other percentile as opposed to the 90th percentile, would the agreement of B(d) with previous studies be any better or any worse?

16) In Section 4.2, the ratio between the snow and rain coefficients is noted to change with particle size. Are you able to add any comments on the physical mechanisms that drive these changes?

17) The conclusion starts by mentioning that the use of existing theoretical formulations for the scavenging coefficients requires somewhat arbitrary choices to be made. However as you note at page 5906 line 27, the choice of the 90th percentile is also somewhat arbitrary. This led me to think that in order for this new parameterization to be a step forward in any manner other than simply being a more convenient formulation – the text should give as much understanding as possible about the range of conditions when these rain and snow parameterizations are considered to be most applicable (e.g. rain and snow types, temperature and pressure).

18) Conclusion, line 4, I am not convinced that closer agreement with field derived values can necessarily be termed 'more realistic'. There are a variety of factors included in the field measurements that are not expected to be included in the theoretical formulations such that it is not clear whether a 'more realistic' agreement should be expected for the 90th percentile as opposed to any other percentile.

Technical corrections:

1) Page 5917, line 2: Should 0.53-0.86 be changed to 0.53-0.89?

2) Table 1 and other tables have rather lengthy footnotes. Could some of this text be reduced by a reference to the nomenclature table since you have already described the symbols there?

3) Figure 1: Perhaps add a note that the legend on panel 1d also applies to the colors on panel 1a.

4) Caption of Fig. 3 reads somewhat awkwardly. Could the caption state what quanti-

ties are shown in the panel 3a as opposed to use of the word 'comparison'? Also might be helpful to state explicitly that the solid line is the parameterization.

5) Figure 7 caption (and throughout the text): As opposed to stating 'particle size' consider explicitly stating 'particle diameter' to avoid confusion about whether the reference is to particle radius or diameter.

Interactive comment on Geosci. Model Dev. Discuss., 6, 5901, 2013.