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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Strong sensitivity of aerosol concentrations to convective wet scavenging parameterizations in a global model" *by* B. Croft et al.

Anonymous Referee #2

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This manuscript presents an exercise in which a variety of wet removal schemes are applied to a GCM to investigate associated changes in aerosol optical depth. The new schemes show very clearly that "aerosol concentrations and wet deposition predicted in a global model are strongly sensitive to the assumptions made regarding the wet scavenging of aerosols in convective clouds". This fact is indisputable and after reading and re-reading this manuscript numerous times I arrived at the conclusion that this message doesn't warrant a scientific publication. It is the kind of work that one expects to find in an Appendix: one or two simple plots justifying changes in a numerical scheme. I recognize that significant effort has gone into these simulations but that does not bring it closer to the threshold for what I would regard as publishable in a scientific

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journal.

Within the world of climate modeling I found the paper typical of GCM studies of clouds, aerosols and precipitation. It jumps between discussion of detailed effects of entrainment on supersaturation, suggesting an attempt to represent these processes (although they are unresolved by GCMs) and coarse treatment of drop nucleation, clouds, scavenging and precipitation. I suppose the excuse is that this is the best that climate models can do. But my numerous readings left me with a profound sense that if papers like this continue to be published we will simply be flooding the journals with excess technical material that does not further scientific understanding, and that raises a new generation of young scientists to believe that climate model clouds are real clouds.

In its current state, perhaps it has a place in ACP's technical notes. Following one potential avenue of enquiry below, it might potentially reach a level which would make it worthy of publication. That would require *major* changes and *major* effort.

Other major points

1) The connection between aerosol scavenging and precipitation may be a way to salvage this paper. These are two tightly coupled issues. Why not strengthen this connection *significantly*. If more aerosols are being removed how does this affect subsequent precipitation rates/amounts? This may require better treatment of coalescence scavenging and sub-cloud removal.(See points 2,4,5). A rigorous comparison with observed rainfall/aerosol distribution at shorter timescales than 5-year averages, and at selected regions might be useful. Extend yourselves! One more technical paper is clutter; one more good paper could be thought-provoking and useful. 2) How is autoconversion affected by changes in scavenging schemes. Is CDNC influenced by autoconversion and accretion? (It should be, although the paper makes no mention of this.) 3) The section comparing to profiles from Koch is so superficial that it has no place in the paper. This kind of work does our field a disservice. If the authors want to compare to observations, please do this with rigorous scientific method, backed by

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statistical analysis. The fact that one of the schemes compares better with MODIS than others is not particularly useful since so many other processes could be tuned to achieve better results. (See points 2,4,5.) 4) Aerosols seem to be released as a result of B-F. How about through drop evaporation or ice sublimation? 5) We know that sub-cloud impaction scavenging can be responsible for $\sim$15% of mass removal. This is brushed off as something to look at in the future (!) 6) Huge changes in CDNC (doubled in the mid troposphere) should provide hints that something is wrong. These are not real clouds!

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 1687, 2012.