Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-129

Anonymous Referee #1

Referee comment on "Particle dry deposition algorithms in CMAQ version 5.3: characterization of critical parameters and land use dependence using DepoBoxTool version 1.0" by Qian Shu et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-129-RC1, 2021

General

This study compares a few formulations of particle dry deposition algorithms in both a stand-alone process model and as implemented in CMAQ. Although the topic of particle dry deposition is an important one, this manuscript fails in many ways to significantly advance our understanding of this subject, and falls far short of the quality of science and its presentation that this reviewer has come to expect from the CMAQ group at EPA. In light of these failures, I recommend that this manuscript be rejected, in the hope that both the science and the presentation can be improved for future resubmission.

Specific

Overall, the writing and presentation in this paper is confusing and poorly done. Given the complexity of the subject, it is vital that the writing and English usage not make it even more difficult for the reader to understand the work performed and the choices that were made in the study. It would be too time consuming to provide a detailed list of all instances of awkward wording or phrasing, but a particular example is lines 199-206 on p. 6, which is difficult, if not impossible, to comprehend. And, in fact, this description is extremely germaine to the focus of the entire paper! Thus, my first suggestion is a complete overhaul of the manuscript and detailed editing to make it comprehensible.

From a science perspective, I question many of the details of the study and a lack of justification for some of the choices made. In particular:

- Why did the authors bother to include the Zhang et al. scheme in their analysis? They

only compare it against the Pleim & Ran (PR) variations in the DepoBox process model and did not implement it into CMAQ. The only conclusion that can be drawn from this comparison, is that, yes, it produces very different deposition velocities than the PR scheme does. It seems clear that the real motivation for this work is to simply improve the PR scheme in CMAQ and not do any meaningful comparison between the Zhang and PR schemes, so why include it?

- In describing the current PR scheme in CMAQ (Section 2.1.2), they fail to explain many of the oddities of the scheme that have a large impact on the calculated deposition velocities. Some of these oddities include:
(i) the use of the 0.95 factor when calculating aerodynamic resistance (Eq. 16) - Why is this factor used?;
(ii) the use of the convective velocity scale, w*, is unique to the PR scheme - the value of this quantity has a huge impact on the calculated Vd, especially over a diurnal cycle, but the importance of this fact is not addressed (except for the assumed values presented in Figure S-2);
(iii) the value of 400 in the impaction efficiency formula is said to have been chosen "to better represent aerosol deposition to heavily vegetated regions" - Is this based on a previous (uncited) analysis or what?
(iv) the interception efficiency is ignored in the PR algorithm because "it is difficult to specify realistic estimates of these parameters over the area of typical grid cells" - Ignoring a term because it is difficult to estimate is a very weak justification;
(v) the use of Eq. 21 as the original impaction efficiency is said to have been "in order to facilitate incorporation of the integrated Stokes number" - It's not clear exactly what this means; was it done to make the modal integration easier? Or something else? As presented in the supplement, other choices for the impaction efficiency formula could have been made to make the integration simpler, but they all are somewhat arbitrary;

- In Section 2.1.3 Proposed Schemes, there are several issues with either insufficient (or unintelligible) discussion or unjustified model modifications:
(i) As already mentioned, lines 199-206 on p. 6 are nearly incomprehensible, but are key to understanding the results the paper presents. I have only a vague understanding of what was done with the impaction efficiency in the "OFF" scheme and why it was modified;
(ii) For the VGLAI scheme, yet another form of the impaction efficiency is given (Eq. 22), this one with a value of 1 instead of 400 in the denominator. No justification for this change is given;
(iii) A leaf area index factor is introduced into the formulation of the boundary layer resistance (Eq. 24) for VGLAI, but no justification for the form of this factor is given or discussed. Where did this come from and what is its intended effect?

- In line 223 on p. 7, it is laughable to state that "a _comprehensive_ evaluation of particle dry deposition schemes discussed in Section 2.1" has been conducted. Three observational studies over only three land use types is not "comprehensive" in my way of thinking.

- The comparisons in Figs. 1-3 of the DepoBox process model with observations do not inspire much confidence in the performance of _any_ of the schemes and make

comparisons of the CMAQ implementations with observations in Figure 9 rather questionable. Comparison of results from the PR-based schemes in CMAQ are potentially interesting, but without a more detailed (indeed, comprehensive!) evaluation against observations, it is hardly more than a numerical exercise to see what all of the arbitrary changes made to the algorithms result in changes to Vd. The authors' selection of the VGLAI scheme as the "most applicable for predicting particle dry deposition over grass and coniferous forests", while potentially true, is not justified by the results presented in this manuscript.