Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2021-129

Anonymous Referee #2

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-RC2, 2021

General Comments

The investigation of dry deposition is critical for constraining aerosol lifetime and impact. This work is addressing an important gap by analyzing the dependance of dry deposition model parameterizations on the land use. That being said, this work does not address the gap in a meaningful way or add to our understanding of dry deposition.

There are major problems with the comparison to measured values, both in terms of the measurement sets chosen and the limited number of land use categories investigated. The work only investigates three land use types and compares the schemes for these land use types to only one set of measured values – two of which did not have the data that the authors needed to operate their deposition schemes. Choices made in the proposed revised parameterization, while well described, are not well explained. The authors have introduced new parameterizations without fully explaining how those parameterizations were derived and have changed established variables without explaining the reasoning. The scheme intercomparisons are very well done and comprehensive, however, they lack meaningful context because of the limited comparison of the schemes to measured values.

For these reasons, I believe the manuscript should be rejected and undergo major revisions before resubmission. I believe that restructuring the manuscript by condensing the scheme intercomparisons into a smaller section – or moving them to the supplemental – and increasing the focus and discussion on the comparison of the schemes to measured values would improve the manuscript.

Specific Comments

LINE 74: I think its inaccurate to say that most only report for a single particle size – there is a decent amount of size-resolved particle dry-deposition data. And in the past 5 years there have been several review papers published on it that compile all this size-resolved data. This obviously varies by region, but for the land uses investigated there are many measurement sets to choose from.

LINE 92, Section 2.1: I don't understand why the authors are comparing to the Zhang 2001 model here. Those parameters have been shown previously to be quite inconsistent with recent measured data (Petroff and Zhang 2011, Emerson 2020) and the model parameters have been re-constrained very recently by Emerson 2020 to better fit measured data. Why not compare to the new parameterizations – or add it to the comparison? The Emerson 2020 model doesn't change any of the core equations of the model so it should be easy to incorporate.

LINE 209, equation 22: The authors haven't really updated the impaction term expression – rather, they are just using the impaction expression with an α value of 1 (which is what it is most of the time for the Zhang model) – which the authors should acknowledge. The authors also need to provide more justification for the use of the 400 vs 1 as the α value. How was 400 shown to be more representative? Why use 1?

I'm also very curious about the choice to hold α at 1 when originally in the Zhang 2001 paper, it is dependent on the land use category. Since a major point of the work is to address the dependence on land use for these schemes why not keep α as a function of the land use?

LINE 218, equation 24: Where does this parameterization of the leaf area index (LAI) factor come from? Did the authors derive it from measurements? Another piece of work? The authors need to give more detail about how LAI was derived, particularly given that it is a newly introduced parameterization.

LINE 221: I think the authors have explained what they have done in their modified schemes very well - just not why they have done it!

Section 2.2: The major issue I see here is that the authors say this will be applicable to "surfaces of varying types" but have only evaluated the model against grass, coniferous forest, and deciduous forest. The fact that this scheme is not evaluated against other land use types is problematic. At the very least they should see how these revised schemes compare to deposition over water. Ideally, they would do it for ice/snow as well, although I know there are fewer relevant datasets.

The other major issue I see in this section is the choice of direct measurements for comparison. It seems like the authors have relied heavily on the compilation of data from Khan and Perlinger, however, there have been several reviews that have more comprehensive lists of the available data for comparisons. The authors have also chosen studies that were listed for particle sizes that differ by an order of magnitude between the three studies (0.04 μm vs 0.5 μm), and the studies they chose didn't have the data needed – so the authors ended up making assumptions about the geometric standard deviation anyway. The manuscript stated a few times that the data was "prescreened from Khan and Perlinger (2017)" but the paper doesn't really address what this means. If there is a reason that the authors are only using data from that list, they need to be more specific about why. That is especially important because they only use one set of data for each land use type.

Comparing to only a single set of measurements for each land use is also problematic given the variability in deposition measurements for any given land type. Overall, I think that this work needs to expand the measurements used to compare the deposition schemes to make the validation and comparison of these schemes more robust.

LINE 278: For Table 2, the authors should have notes in the table to indicate which values were assumed and which values were calculated in this work vs which values were provided by the cited studies. For example, the density assumption of 1500 kg/m3 and the geometric standard deviation assumption.

LINE 356: The paper states that the data set is incomplete because "the measurement could not be perfectly considered to represent "deposition to the grass surfaces" because they have not been screened for either wind direction or the morning transition period". So why is this the data set that the authors are using? Why not use a data set that has had that filtering already applied? Comparing to multiple data sets would also help the authors if they think those trends are an artifact of the filtering either applied or not applied to that specific data set.

LINE 365: "Across three land-use types, PR11, OFF, and VGLAI show more consistent diurnal patterns as the measurements than Z01". I think this is a hard statement to make when the text just presented plots of diurnal cycles. Especially when in Table 3, the Z01 scheme consistently has fractional biases closer to zero. I think a plot either in the main text or supplemental, where the authors could show the diurnal fractional biases for the models, would be helpful. For example, the authors could show one plot for each land use for a selected, single σg. Otherwise I don't think the manuscript has actually provided enough evidence to make this claim.

LINE 426, Section 3.5: This section is arguably the most important piece of the manuscript's results – comparing modeled to measured values – and it only takes up a small portion of the discussion. The deposition scheme intercomparisons are interesting, but until this point do not have meaningful context behind them.

LINE 450: "…the results show that the scheme by Pleim and Ran (2011) modified to include vegetation dependence was best able to capture the magnitude and variability across all of the observation datasets." As a reviewer, I don't think that the authors have provided enough evidence to support this statement.

Technical Comments

LINE 56: What do the authors mean as a function of particle? Please be specific here and say function of particle size.

LINE 48, equation 1: define the vertical flux variable when vertical flux is first mentioned in the prior sentence, and then remove the identifier after the equation.

LINE 100, section 2.1.1: The authors have outlined the model operation very well, but this section is quite hard to read. It also doesn't help identify what the major differences are between the various schemes that the authors are using, which is really important. I suggest using table 1 to compare the equation parameterizations for the two models and then number and reference the equations in the table. More variable definitions could then be added as notes bellow the table.

Using the table would make it easier to directly compare these models and would free up space in the text for the authors to focus on their main points. I understand that this is a formatting preference on my end so if the authors choose to keep the outline as it is, I would at least suggest adding a section at the end where they highlight what the major assumptions and key points of the model are – essentially answer the question "in the context of this manuscript, what are the most important aspects of these different schemes?". That way, when the two schemes are contrasted, the reader is clear which aspects of the two schemes we should be focusing on.

I think this it key for this work because it would help explain why the authors chose to work off the PR11 scheme to develop the new parameterizations and not the Z01 scheme.

Additionally, the manuscript's final results really don't focus on the Zhang model. The authors could therefore put the larger outline of the Z01 model parameterizations in the supplemental to help focus the work on the PR11 scheme and the two developed schemes – since understanding those is key for the manuscript's results.

LINE 120: put Rs in parentheses

LINE 221: Final sentence is awkward "ð□□¸ð□□µ and ð□□¸ð□□¼ð□□□ both inherit implementation in PR11" Are they both implemented? Could rephrase to "Both EB and EIN are implemented in PR11"

LINE 331: Might be good to mention that for an ideal model the fractional bias would be 0. While this is a common performance metric it's helpful for a reader to define this metric, so we know what the authors are trying to achieve using the metrics outlined. This is especially important because in the next section, the manuscript jumps right into reporting fractional bias numbers.

LINE 373: Figure 4, please explain in the figure caption that the numbers in each box represent the fractional difference between each scheme and the PR11 scheme. The column should be labeled with that scheme instead of the calculation, and the calculation could then be moved to the label on the color bars. I think this will make it more clear that the color bar represents that fractional change for each of the schemes.

LINE 395: Figure 5 is not clear enough. I'm assuming that the blue line is supposed to separate the coarse mode data onto its own axis on the right, which has the higher scale, but this isn't explicitly stated. Please clearly state this in the figure caption.

LINE 400: Is figure 6 supposed to have the same kind of dual axis as figure 5? The blue line is there separating the results for ASO4(I+J) but everything is on the same axis. If the line is just a fine vs coarse mode separator then please state that and ensure that the notation in figure 6 is different from figure 5, where it was used to separate two different axis sets.