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Geoscientific Model Development Discussions

Interactive comment on "Improving dust simulations in WRF-Chem model v4.1.3 coupled with GOCART aerosol module" *by* Alexander Ukhov et al.

Anonymous Referee #2

Received and published: 24 June 2020

This manuscript identifies a number of inconsistencies in the treatment of dust in previous versions of WRF-Chem+GOCART, quantifying their impact with a focus on simulations over the Middle East, and documents the fixes introduced into v4.1.3. It also presents an interpolation tool called Merra2BC for generating initial and boundary conditions for the model.

In documenting clear improvements to certain aspects of the model, the manuscript is appropriate for publication in GMD; however there are a number of issues that should be attended to before publication:

General comments

There are a very large number of appendices, some of them very short, which results in a disjointed manuscript that doesn't flow very well. I would consider reformulating these so that the overall paper flows better (possibly merging those which are fundamental to the paper, e.g. the non-technical description of Merra2BC, into the body). Appendix F doesn't even appear to be referenced anywhere in the manuscript.

There are several places (particularly in the introduction, but also in section 2.1.1) where an excessively long list of citations is given to exemplify a point – please consider whether *all* of these are necessary and if not cite the most pertinent examples.

In a number of places, configuration parameters of WRF-Chem are referred to without explaining their meaning. While the manuscript is obviously of most interest to those familiar with this model, it should be understandable more widely.

Finally, the experiments carried out should be described prior to the results section.

Specific comments

p.3, lines 29–31: please include a table explaining what these options are.

p.4, line 14: please state what "chem_opt=300" means.

p.4, line 24: is AFWA an acronym? If so, please expand on first use.

p.6, lines 28–29: should this be ERA-Interim (not ECMWF Interim)?

p.7, lines 9–12: a little more explanation of Merra2BC is warranted in the main body of the paper, especially given that its introduction is later highlighted in the conclusions suggesting it's more than a minor element.

p.7, line 14: this is confusing, because it refers to "all inconsistencies" when these haven't yet been enumerated in the text. Please reformulate so that the inconsistencies, changes made, and experiments carried out are introduced prior to the results section.

p.7, line 30: is it documented that a log-based distribution is the one on which the parameterisation is based, and that therefore this is an inconsistency? Or is it the author's assumption/assertion that such a distribution is universally the correct one to assume, whatever the parameterisation?

p.8, line 6: "overestimated" sounds like it is a comparison to observation, but I think this means only in comparison to the modified model? Please make this clear, and if possible illustrate if this is an improvement against relevant observations. (It is not given that a model which appears to be more theoretically correct actually improves results.)

p.8, line 19: it's not quite true that these are not accounted for - all of $DUST_1$ is included if you add up the coefficients; it's merely that some of it is treated as larger than it should be (and thus less optically effective). Please clarify this in the text.

p.10, lines 3–4: please clarify what you mean here. A "function of natural logarithm of radius" is also a "function of radius". Do you mean specifically a *linear* function of each?

p.14, lines 20, 23, 27: again, "overestimation" suggests this is by reference to some actual measurement rather than to the modefied model - please clarify if this is just "one model version is higher than the other" or if the change is an improvement or degradation compared to measurements.

p.14, line 29–p.15, line 3: Section 3.4 is very brief – to be meaningful, this needs to show to what extent the contribution from both initial and boundary conditions is significant relative to one another and to sources within the domain.

p.16, line 9: as above, please clarify that submicron particles aren't omitted as such, but treated incorrectly.

p.18, line 9: is it really correct that MERRA-2 has a globally-uniform constant surface pressure of 1000hPa? That seems highly unlikely in a meteorological reanalysis – please check and clarify, as this is what the current text suggests.

p.19, Figure A2: both singular and plural should be "species" (not "specie").

p.21, lines 7–11: please state the AERONET wavelength(s) from which these calculations are performed to generate the 550nm value.

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