Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-185-RC2, 2018

Geoscientific Model Development Discussions
Open Access
EGU

Interactive comment on "Global aerosol modeling with MADE3 (v3.0) in EMAC (based on v2.53): model description and evaluation" *by* J. Christopher Kaiser et al.

Anonymous Referee #2

Received and published: 4 September 2018

The paper presents a new aerosol model, MADE3, extending the earlier MADE models to include an explicit representation of multiple mixing states within the modal aerosol framework by separating purely-soluble modes from those also containing insoluble material such as black carbon. This is quite a novel approach compared to most modal schemes, with the potential for improved representation of the variability in aerosol composition, aerosol-cloud interactions and comparability to in-situ observations. MADE3 is presented as a proposed component of the ECHAM/MESSy Atmospheric Chemistry (EMAC) model. A comprehensive evaluation is presented against both remote-sensing and in-situ measurements, with apparent model deficiencies highlighted rather than glossed over, but performance generally on par with other global

Printer-friendly version

Discussion paper

aerosol models.

The model and results are generally well presented, with further detail in the appendix rather than cluttering the main text. The paper represents a significant contribution to the existing range of aerosol models used within the community, and I would recommend publication in GMD subject to the following minor comments:

p.5, lines 3–4. "To avoid interpretation of..." is a bit confusing. I would suggest "To avoid convoluting the results with feedbacks from..."

p.5, line 7. If the "big leaf" approach is mentioned here, a reference should be provided.

p.6, lines 12–20. This paragraph is a little confusing. The criterion for transfer to mixed modes is stated as "a liquid coating of a critical size" (size presumably meaning thickness here?), but with aerosol water neglected. Surely the thickness of a liquid coating would be largely determined by the amount of water, however? Or does this rather mean a *soluble* coating of a critical thickness, e.g. a layer of sulphate n molecules thick, ignoring any water? Such a coating-thickness criterion would require an amount of soluble material proportional to the surface area of the insoluble core, while the following text describes a criterion as a proportion of the mass or volume. Please clarify.

p.6, line 21. Please explain the rescaling which is applied here.

p.7, lines 3–5. This sentence seems to suggest the Bergeron-Findeisen process represents the freezing of cloud droplets, with the evaporation of droplets as an alternative. I assume this is a mistake in the phrasing, since (as the authors are no doubt aware) the Bergeron-Findeisen process represents the latter (evaporation of droplets, releasing their aerosol, with deposition of the resulting vapour onto existing ice particles) rather than the former (freezing of existing droplets). Please clarify the sentence.

p.8, line 20. What about impaction by cloud droplets (not by precipitation, which is described separately)? Both activation and impaction are mentioned earlier as separate processes by which aerosol particles can be taken up by cloud droplets, but it is unclear whether insoluble particles are subject to impaction only, or to neither process, in the model.

p.12, Table 3. There are a lot of dense numbers in this table, which would be clearer visualised in chart form.

p.14, lines 3–4. This seems to put quite a positive spin on what looks from the figure like a general disagreement between model and observations. I would re-phrase this to more clearly acknowledge the large overall error, while still pointing out the agreement in the spatial pattern.

p.22, lines 2–3. Please clarify which phase of the AeroCom project is referred to here, as the models have evolved significantly since Phase I.

p.27, Figure 9. Do these observations really show more than 50% of the aerosol in the 46–167nm range being dust? That's a surprisingly large fraction at such small sizes, and if correct represents the major disagreement with the model which should be discussed in the text.

p.31, Table 5. There are a lot of dense numbers in this table, which would be clearer visualised in chart form.

In addition, there are a few typos and grammatical points that should be corrected:

p.3, line 31. Delete commas in "both, gases and aerosol particles, . . . "

p.11, line 6. "of several 10%" doesn't read well. I'd suggest "of severel tens of percent".

p.11, line 18. CASTNET and IMPROVE are mentioned here, but the acronyms are not defined until their next use on lines 31–33.

p.11, line 26. Delete comma after “both”.

p.16, lines 15–16. Change “statistics of. . . is“ to “statistics of. . . are”.

p.17, line 29. Delete commas in “both, the simulation, and. . . ”

p.22, line 5. Delete commas in “both, spatial and temporal coverage, . . . ”

p.24, lines 24–25. “up to several 10%" doesn’t read well. I would suggest “up to several tens of percent”.

p.28, line 22. “lead to several 10% different. . . ” doesn’t read well. I would suggest “lead to several tens of percent difference in. . . ”

p.30, line 4. Delete “shows” in “. . . algorithm shows significantly overestimates AOD. . . ”

p.31, line 13. “several ten percent" doesn’t read well. I would suggest “several tens of percent”.

p.32, line 7. Change “size distributions, . . . , was. . . ” to “size distributions, . . . , were. . . ”

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-185, 2018.

Printer-friendly version

Discussion paper