Comment on gmd-2021-397
Anonymous Referee #2

Referee comment on "Description and evaluation of the community aerosol dynamics model MAFOR v2.0" by Matthias Karl et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-397-RC2, 2022

Review of Description and evaluation of the community aerosol dynamics model MAFOR v2.0

The manuscript describes an open-source aerosol model, which includes all the basic aerosol microphysical processes as well as multiphase chemistry. It is an updated and extended version of an earlier published version MAFOR1.0. Such a model package can be useful to the aerosol modeling community, as such open-source models are still rare. I have some remarks that have to be addressed. Especially, I am concerned that the main improvements of the model (compared with the earlier version) might not be tested in the evaluation part.

- As a major advantage of the described model, “consistent treatment of both the mass and number-based concentrations of particulate matter” is listed (already in the abstract). If I have understood the details of MAFOR1.0 correctly, the choice of solution was already similar, which means that this feature is not a novelty in 2.0? In addition, it is stated that this is a feature that is an advantage “compared to the other sectional aerosol process models” (Summary, page 56, lines 28-30). Does this mean the models that were compared against here (AEROFOR, SALSA), or more generally? In either case, this is a strong statement, which needs more careful justification. E.g., in SALSA, you state that “SALSA outputs volume size distributions of particle components, which at known density can be translated to mass concentration” (page 53, lines 22-23). What is the inconsistency? And, further-furthermore, how about two-moment sectional models such as TOMAS (e.g. Lee and Adams, 2012) or GLOMAP (Spracklen et al., 2005)? Are they also prone to inconsistency regarding both mass and number-based concentrations?
- Page 4, lines 7-10, statement about integrating aerosol dynamics with gas phase chemistry. It is true that many of the aerosol dynamics models do not contain gas phase chemistry. However, many of them are designed to be coupled with a separate gas phase chemistry model. One example is the HAM-model (Bergman et al., 2021), which includes SALSA, and has been implemented into the ECHAM5 climate model. Would it thus be more ‘fair’ to compare the contents of MAFOR to HAM (and other such
models) instead of the pure aerosol dynamic packages, when considering what they contain (Table1)?
- I really appreciate the clarity by which the differences between versions 2.0 and 1.0 are explained (section 2, page 5). However, what remains unclear is what is the role of these new developments in improving the results presented in this manuscript (section 4), where dilution and aerosol dry deposition seem to dominate the dynamics, and, e.g. the new nucleation models seem to play no role. It feels like very little of the listed novelties are actually tested here???
- Table 5, page 40: As dry deposition is stated as the other major process (in addition to dilution) affecting the size distribution, and each of the models compared have a different dry deposition parameterization, is it possible that it is these differences (as well as how the particles are introduced into the beginning of the simulation) that explain much of the differences in the results.
- Page 30, discussion about numerical diffusion: The authors acknowledge that the fixed sectional discretization chosen is prone to numerical diffusion. They also state that this is circumvented by using enough bins. However, if the model is to be used in 3D atmospheric, or even climate models, one can typically not afford to use a very big number of bins, and this is why other methods, such as the moving center method (which is mentioned) has been proven useful. This, I believe, needs more clarification, especially since in the summary (page 58) MAFOR2.0 is envisioned “a state-of-the-art benchmark model” to be also implemented into earth system models.
- The theory in Appendix B is a bit hard to follow. Is B2 a time-differenced result of B1? (together with B4?) And, where does B3 come from? For the reader, it would be clearer to first show the differential equation, the mention how it is discretized, and finally show the discretized equation.

