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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of global simulations of aerosol particle number and cloud condensation nuclei, and implications for cloud droplet formation" *by* George S. Fanourgakis et al.

Anonymous Referee #2

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Fanourgakis et al. perform a large model intercomparison study focusing on the evaluation of aerosol size distributions, CCN, and Nd with surface observations as a reference. The main conclusion is that models have substantial problems in simulating the concentrations of CCN, with on average strong underestimations. In contrast, Nd as derived applying an off-line parameterisation with prescribed vertical wind is much better correlated to observations-tied estimates.

The study is a very large effort and worth publishing. It is also useful for upcoming assessments such as the IPCC AR6. It is, however, a pity that it was impossible to point to specific parameterisation shortcomings, since obviously neither in the multi-model

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Discussion paper

ensemble nor in the PPE, specific parameterisation choices and/or specific models seemed to systematically correlate better to the observations than others. The only hint is that organic aerosols seem to be more difficult than other types.

I think the authors should better explore the Nd result in a revision. The first thing that would be very useful and probably not difficult to do is to compare the Nd the models actually compute to the idealised ones computed here off-line. It would be very interesting to see how large the potential biases in the parameterisations of droplet activation are in comparison to the aerosol biases. I also wonder why the deviations in Nd are in some cases qualitatively different from the deviations in CCN. Some explanation is needed.

Specific comments

p4 l12: Is “OA” really defined as a fraction here? - contradiction to p8 l8

p5 l13: really opposite, i.e. of different sign?

p6 l30: Modal aerosol module

p7 l2: Kirkevåg

p7 l11: “a few models account for melting and sublimation of ice crystals” - I have the impression this sentence is incomplete.

p10 l15: why this very coarse resolution, and not just the resolution of the coarsest-resolved model?

p11 l6: the conclusion that the correlation is captured “satisfactorily” needs quantification: what can be considered “satisfactory”? How do the authors quantify these relative differences? - perhaps a figure like Fig. 3 but for the spatial variability would be useful?

p11 l7: The index of agreement, since it is not a conventional metric, would need to be defined in the main text. The authors further should motivate why this IoA provides extra information that substantially goes beyond NMB and NME.

p13 l1: “satisfactorily” - again a quantification would be helpful. Is this significantly better than for CCN0.2?

p14 l5: the + for NME is unnecessary

p14 l10-12: Why would the authors call an NME of 97% “reasonable”?

p15 l6: I rather have the impression that it is completely unclear: in 5/9 station, it is indeed longer in winter, but in 4/9 the opposite.

p15 l7: in 6/9 cases, the MMM is consistent with the summer-winter change as the obs show, in 3/9 cases, opposite. Is that “qualitatively capturing” the change?

p15 l32: it doesn’t hinder an eventual decrease in smax, it implies it.

p16 l24: The competition effect cannot explain why the ratio observed/simulated for Cabauw and Vavihill turns from substantial overestimation to underestimation, and why the opposite is found for Finokalia.

p31 Fig. 2 – I don’t see the dashed lines for the observations.

p34 Fig. 5 – Would it be useful to show the MMM as well?

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