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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 #1

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This study presents results form a model intercomparison of 16 global models to compare predicted aerosol and cloud condensation nuclei number concentrations. Model results are compared to observations at nine locations representative for differently polluted regions. Based on predicted and observed CCN concentrations, cloud drop number concentrations are calculated for each model output and location. This intercomparison is accompanied by performing a perturbed parameter ensemble study that explores the sensitivities of model/observation biases to numerous aerosol parameters. This comprehensive study is extremely useful as it highlights uncertainties in current model predictions of aerosol parameters that translate into uncertainties in cloud drop number concentrations. Given the large uncertainties in current estimates of aerosol-

cloud-interactions, such studies will help to refine future models. The manuscript is extremely well written and I only have some minor comments that should be addressed prior to publication.

Main comments

In large parts, the discussion of the model results is merely a description of the figures. I missed at several places, a more thorough discussion of the underlying potential reasons of discrepancies and comparison to prior model studies. Examples are listed in the following:

p. 10, l. 32: The sensitivity of CCN prediction at low supersaturations has been discussed in many previous studies and been ascribed to the sensitivity of N(CCN) if the critical diameter is within the 'steep' section of the size distribution. Some references should be added here.

p. 13, 20-26: Is it expected that PPE simulations show this qualitative agreement with the MMM? What would it mean if there were no agreement?

p. 14, l. 5-7: Is this underprediction of total aerosol particle number a known bias in global models? Wouldn't it be then more useful to report CCN fractions, i.e. N(CCN)/ Na?

p. 15, l. 20: How do the parameterizations of wet deposition differ in the various models? I am quite certain that such descriptions have been discussed in previous model intercomparison and been identified as major causes of discrepancies. Such references should be added here. p. 16, l. 20-25: The relationship between N(CCN) and Nd as a function of w has been explored in many theiretical studies (e.g. by Nenes, Feingold, Reutter and others). Can your 'qualitative example' here be related to them?

p. 17, l. 20/21: What are the different assumptions on emission heights in the various models? How much of a discrepancy did the study by Daskalakis find?

p. 18, l. 11: Is there an explanation of the much highe rconcentraiton as found by

Spracklen et al.?

p. 21, l. 2: Your findings are in agreement with the concept of aerosol-‘vs ‘updraft-limited’ regimes. That should be repeated here.

Minor comments

Title: I think the title should be reworded and ‘number’ should be moved: Evaluation of global simulations of aerosol particle and cloud condensation nuclei number, and implications for cloud droplet formation’

p. 4, l. 9: This sentence implies that particle size also determines hygroscopicity – which is not true as hygroscopicity is a mass-related parameter. Please reword.

p. 6, l. 14: ‘one of the different binary homogenous nucleation’ sounds weird. Please reword.

p. 6, l. 29-p. 7, l. 3: It might be easier to list this information in a table.

p. 8, l. 4, l. 9 and later: In the reference list, Schmale 2017a and 2017b are listed whereas in the text Schmale et al., 2017 and 2018 are cited. Please correct.

p. 8, l. 20: ‘the chemical composition. . .’: There seems to be a verb missing in this fragment.

p. 10, l. 16: why ‘would no doubt affect’? I think it is safe to say that ‘no doubt affects. . .’

p. 11, l. 29: higher summer time values. . .

p. 12, l. 27: I suggest rewording to ‘an insufficient number of small particles are predicted to activate in the model’

p. 12, l. 29: remove ‘the’ (for all stations. . .)

p. 14, l. 19: Where does the parenthesis close?

p. 14, l. 19: remove ‘the’ (by nine of the models. . .)

p. 14, l. 19: I do not understand the fragment ‘not systematically the same models at all stations’

p. 15, l. 2: Has ‘persistence’ been defined anywhere earlier? (I might have missed it). Is it the time a CCN spends in the atmosphere?

p. 18, l. 23/4: Given that the referenced figures are in supplement, the ‘significant differences’ should be more explicitly discussed here.

p. 19, l. 3-21: It would help a lot to connect this text to Figure 11, if the abbreviations from the figure legend are added to the text. Some of them are not intuitive.

p. 19, l. 18: ‘in-cloud oxidation of SO2’ is not explicitly included in the parameters in Figure 11. Is it connected to uncertainties in pH?

p. 26, l. 15 and l. 17: The second of these two references seems wrong and redundant.

Figures 2 and 6, caption: ‘dots and dashed lines’ and ‘bold points and dashed lines’, respectively, is confusing as the dashed lines are model results. I think it should be sufficient to say ‘observational data is shown with symbols’ (unless I misunderstand something here). Figure 7: It would be much easier to understand the figure if you added a legend with ‘observation winter’, ‘observation summer’, ‘model winter’, ‘model summer’

Figure 8: Is Smax shown in %?

Figure 9, caption: ‘blue colored values’ sounds strange (values don’t have colors). I suggest rewording to ‘all NCCN at smax (blue symbols) have been divided by 2’

Figure 11: a) I understand that details about the analysis will be presented by Yoshioka et al. However, given that this paper is still in preparation, I suggest adding a list of all parameters, their meaning and ranges. b) Sum of the bars do not add up to 100%. Is the rest then the sum of the last four parameters, i.e. Carb_BB_diam, carb_res_diam, Sig_w, and Dust?

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