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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Parameterising Cloud Condensation Nuclei concentrations during HOPE" *by* Luke B. Hande et al.

Anonymous Referee #2

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The authors present a study on aerosol and CCN concentrations during the HOPE campaign. The model COSOMO-MUSCAT is used to predict particle concentrations and CCN number concentrations. In addition, the CCN concentrations are parameterized as a function of updraft velocity (w) and pressure (p). While parameterizations of CCN in regional or global models are urgently needed in order to improve predictions of clouds and their radiative properties, the results and conclusions of the current study are not quite convincing to meet this goal. Some underlying processes and assumptions have to be explained and justified better. My main concerns are listed below, together with some minor and technical comments.

Major comments

1) p. 4, l. 15ff: What is the fraction of organic aerosol to total PM2.5? Does the

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underestimate of a factor of 2 mean that half of PM2.5 could be organic aerosol? How does this bias affect results, given that SOA has been shown to contribute significantly to CCN number?

2) In general, the CCN number concentration is a function of the supersaturation. The supersaturation is a function of the updraft velocity (source term) and the surface of the aerosol particles (sink term). If the total aerosol concentration is underestimated, I do not see how the CCN number can be calculated correctly in the model, only based on w.

3) How well can w be constrained by the model? And by observations? If CCN are parameterized such that they match observations, while there are uncertainties in both updraft velocity and total aerosol number (mass), it seems to be just an empirical fit where several effects might fortuitously cancel.

4) At low updraft velocities, the supersaturation and therefore the CCN number concentration is the lowest. What is the reason for the largest uncertainty at these conditions? The scarcity of measurements, uncertainty in measurements or the fact that at low number concentrations of activated particles any bias translates into a larger error?

5) What is the underlying reason that CCN concentrations are parameterized as a function of pressure? Is pressure only used here as a proxy of altitude?

Minor comments

p. 1, l. 24: Reword. The hygroscopicity parameter implies some chemical composition, therefore composition is not neglected.

p. 2, l. 6: Twomey's power law does not describe the size of aerosols but the total number of CCN.

p. 3, l. 7: Table 2 is referenced before Table 1 (Is Table 1 referred to at all in the text?)

p. 3, l. 9: Reword. – Results of the study by Ghan et al. (2001) were used (or similar)

p. 5, l. 6/7: The difference between ammonium sulfate and sulfate should be explained here (and not later in the manuscript). Is there no mechanism in the model that converts sulfate from nucleation into ammonium sulfate? Is this realistic?

p. 6, l. 7: Related to my comment 2) above, the supersaturation is not only determined by the updraft velocity but also by the total aerosol surface – not only the aerosol species.

p. 9, l. 2: Is it surprising that above continents the anthropogenic fraction is greater than globally, given that the majority of surfaces are oceans?

Technical comments

p. 1, l. 1: transportation –> transport

p. 2, l. 19: introduces –> introduce

p. 2, l. 26 and 27: ICON-LES and HOPE should be defined here (and not in Section 2)

p. 2, l. 31: A essential –> An essential

p. 3, l. 32: Black Carbon –> black carbon

p. 3, l. 33: compisition –> composition

p. 3, l. 34: shouldn't –> should not

p. 3, 34: shows –> show

p. 4, l. 7: What does the T stand for in TDMPS?

p. 5, l. 9: aitken –> Aitken

p. 5, l. 10: show –> shown

p. 6, l. 27: dependence –> dependence

Figure 3: 1) The axes are very blurry; the exponents are hard to read; 2) The green

dotted lines should be labeled; 3) add a), b) c) to the panels.

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