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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Physical properties of the sub-micrometer aerosol over the Amazon rainforest during the wet-to-dry season transition – comparison of modeled and measured CCN concentrations" *by* J. Rissler et al.

Anonymous Referee #1

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Physical properties of the sub-micrometer aerosol over the Amazon rainforest during the wet-to-dry season transition - comparison of modeled and measured CCN concentrations.

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General Comments

This is a fundamentally sound paper with respect to the research it presents, but the

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quality of the writing is such that only very diligent readers would be likely to work through the entire text. The entire text should be proofread very carefully before final publication, with a careful eye towards consistency in verb tense and punctuation. In some sentences, multiple readings are required for understanding. For example:

"Since organic surfactants are known to reduce the surface tension, in the sensitivity study also an investigation of the possible impact on the CCN concentrations of reducing the surface tension at the point of activation by 15%, was made." (p. 3187)

And:

"The hygroscopic diameter growth factor was measured with an H-TDMA and often showed a bimodal structure, although one hygroscopic group was clearly dominating. This group had growth factors that were relatively stable between the time periods, and with an average around 1.29 (for the almost hydrophobic mode 1.06) for 100 nm particles (growth factor of 1.7 for pure ammonium sulfate), with a slightly increasing growth as a function of dry size." (p. 3200)

Note that these are only examples; the authors are strongly encouraged to review the entire manuscript to make it more clear for the benefit of their readers.

Language issues aside, the strength of the comparison between the H-TDMA data and the CCN observations make this a worthwhile paper. It further solidifies our understanding of the dependence of droplet activation on particle size and composition. The manuscript should be accepted by Atmospheric Chemistry and Physics once the general language issues and the specific comments listed below are addressed.

Specific Comments

1. Introduction

Lines 9-10, page 3161 use the term “aerosol particles”, which is redundant. “Aerosols” is sufficient.

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Lines 13 and 25-26, page 3161: There are instances where value judgments (beyond the scope of the work) are indicated by the chosen language. The most striking examples are the last sentences of each of the first two paragraphs. Words like “suffering” and phrases like “protect the integrity of the system” are inappropriate for a research paper. To a lesser degree, the start on the fourth paragraph (“In spite of the great climatic importance of the Amazon region...”) implies the same bias (line 11 on page 3162). These are not judgments I disagree with, but the word choice should be more objective here.

2. The experiment

Lines 22-23, page 3166: This is an exceptionally long delay between the measurement period and the calibration period. Obviously this can’t be remedied now, but given the stated sensitivity of the CCN measurements to small changes in the system timely calibrations are rather important. Also, the last sentence should read “of the CCN counter”, rather than “at”.

Line 5 of page 3167: “was” should read “were”.

Lines 5 and 12, page 3167: The phrase “quality assure” is twice used as a verb. I understand the meaning, but I don’t think the phrase can be used in that way.

3. CCN calculation model

Line 16, page 3169: When describing the usefulness of the model, it would be helpful to indicate that the model is developed with H-TDMA data in mind. That is the source of the caveat about subsaturation solubility, but that point is not clear.

Lines 10-11, page 3170: e is better described as the equilibrium partial pressure over the droplet, while es(T) might indicate that it describes the equilibrium over a flat surface of pure water, to clarify the difference.

Line 10, page 3172: ds is better described as the dry particle size, to avoid confusion.

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Interactive Comment

Line 14 of page 3172: Why the non-ideal behavior of the model salt? No reason is given.

Lines 14-17, page 3172: These assertions are not immediately obvious from the given information. The authors may consider including Equation (6) from Swietlicki et al. (1999) to support the assertion.

Lines 17-18, page 3172: “Needless to say...” This should be removed.

4. Submicron physical properties

Lines 8-10, page 3175: "(Hours)" and "(days)" are ambiguous. The meanings of “recent biomass burning” and “aged biomass burning” can be defined once and then used consistently throughout.

Lines 21-22, page 3176: This sentence does not require parentheses.

Line 26, page 3179- line 3, page 3180: The error bars on figure 3 indicate that the differences between the two periods are not statistically significant. This should be noted, or the comparison should be removed.

Page 3180: The biomass plume is compared to other observations in the region, but the source is known to be different. How does the result compare to other measurements of small grass fires, both in number concentration and size distribution?

Lines 3-5, page 3181: Here the origin of the recent biomass burning is characterized as “probable”. This is stated definitively in section 4.1.2. The statements here and elsewhere in the paper should be consistent.

Line 12, page 3183: Figure 2c is not sufficiently time-resolved to illustrate the statement.

Lines 5-6, page 3184: It’s not clear to what the statement in parentheses is referring.

5. CCN prediction

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Lines 14-15, page 3188: Table 4 should be mentioned here.

Lines 15-17, page 3188: The authors should discuss the sources of the estimated instrument error. Issues with this type of instrument are discussed at length in the Nenes et al. reference.

Line 3, page 3189: “reducted”??

Line 15, page 3189: Here, the term hygroscopic is used without the previous used “more” or “less” qualifier. The terminology should be consistent throughout.

Line 19, page, 3190: Ni should be ni.

Tables and Figures

Table 4: The “young” biomass burning is split into two time periods, without explanation. The terminology has also changed from “recent”. Also, an additional supersaturation, S=0.12%, is described in the text but not included here. Either an explanation should be provided, or the data should be included.

Figure 2: This figure is too small to see the trends discussed in the text. In particular, it is difficult to distinguish between days, much less the hourly variations described.

Figure 3: The right axes need correction; the lower supersaturations are rounded down to zero. Also, the size distributions and the size/supersaturation have very little to do with each other. It would be more appropriate if they were separate figures.

Figure 4: This plot is the core of the analysis; it would be very nice to also see the trends at the other measured supersaturations.

Figure 7: The different groups of curves, corresponding to the different time periods, should be labeled.

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