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Interactive comment on "On the effects of hydrocarbon and sulphur-containing compounds on the CCN activation of combustion particles" *by* A. Petzold et al.

Anonymous Referee #5

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General comments:

This paper presents some very interesting and useful results from a set of difficult measurements on jet combustor aerosol particles. Most of the article concerns enhancement of CCN activation properties of carbonaceous particles due to the sulfur content of fuel and the production of sulfuric acid. Strong conclusions made in the abstract regarding the role of organic carbon content on water uptake and CCN activation are not equally well supported by the data provided. Further, the data are presented without any statistical qualification. The result is that some conclusions require a lot of faith in trends versus clear quantitative significance. The authors do admit to the qualitative nature of some of the results, but only in the concluding section. The general discussion in the paper does not match this qualification. I therefore recommend that the strength of the conclusions be softened and/or that some uncertainties are specified.

Specific comments:

1. Abstract: I find most of the statements made in the last few sentences to reflect speculation rather than strength of evidential results. These statements therefore need to be qualified as such. Most of the data that serve as their basis appear in two plots and two paragraphs of discussion.

2. Introduction: I find the statement that ends page 2601 and starts 2602 to be inaccurate. Although CCN activation may be a key process with respect to the indirect effects of combustion-related particles on global climate, there remains serious question regarding the potential role of ice formation processes on combustion-related particles. While this may not be an issue in contrail conditions where high water saturation ratios are achieved, it is an issue in the absence of contrail formation or after contrails evaporate and the particle trails remain as potential cloud forming nuclei.

3. Section 2.2: The use of a 20 nm diameter for normalizing CCN concentrations to the concentrations of carbonaceous particles seems quite arbitrary. I believe there is a basis for this assumption in previous papers from this group, but I did not see it clearly stated here. Also, why were CCN spectra not obtained? The selected value does not seem much relevant for the mode sizes of particles or the activation diameters of coated particles. Obviously a value needed to be chosen, but it is not clear why a 1.006 saturation ratio was chosen or why other set point values could not be used.

4. Results:

a. Section 3.1: Figure 3 does not present a very convincing argument for an increase of OC fraction of TC with combustion temperature. Is it possible to place error bars on the data points?

b. Section 3.1: Do any of the previous publications show electron microscope imagery of the particles to demonstrate their irregular structure? This would help in understanding how uncertain an estimate of their surface area or volume could be.

c. Section 3.2, p. 2609: What is the meaning of the "CCN relevant size range of 100 nm"? Many sizes of CCN can be relevant, depending on the cloud type and particle compositions.

d. Section 3.3, p. 2611: Why are there no error bars on growth factor values in Figure 10? I assume from various papers in the literature that this is quite well known and typically on the order of 0.02. Could a single one be shown, so as not to confuse the plot? I also note that some of the data in this figure is previously published by Gysel et al. (2003), which perhaps should be mentioned.

e. Section 3.3, Figure 11. For the comparison made in the bottom panel of Figure 11, is an inherent assumption made that all larger particles possess the same soluble volume fraction as measured for 100 nm particles? Alternately, is some relationship derived based on the observed variation of soluble volume fraction versus size shown by Gysel et al. (2003)?

f. Section 3.3, Page 2612, lines 20-21: Remove or modify this sentence, which is irrelevant or otherwise misleading since the actual measurement of saturation ratio in natural clouds has only been demonstrated on a few select occasions and there is no reliable measurement capability for all clouds. The actual values in clouds can only be inferred to range from quite low values to in excess of 2

g. Section 3.3, p. 2613, lines 20-24: I am not sure how this brief discussion of ice particle activation in contrails is relevant to the present discussion? Please clarify.

h. Section 3.3, p. 2614: Is this newly reported research on this page or a summary of published information?

i. Section 3.3, p. 2615 and Figure 12: Figure 12 does not display a clear and impressive relationship between the plotted quantities and the discussion of it focuses only on the maximum deviation noted at high FSC. Please apply error bars and other evidence or modify the strength of the discussion.

j. Section 3.3, p. 2615 and Figure 13: The issue in this figure is the same as for Figure 10. These data provide inferential conclusions, but I am not convinced that there could be any significance to the two highlighted data points if experimental uncertainty is considered.

Technical notes:

Figure 2 caption: Should note that m is simply the linear regression slope.

Page 2608: gaseous is misspelled.

Page 2610, line 19: Suggest use of "used" rather than "deployed".

Page 2612: The sentence following Eq. 5 refers to Eq. 4 and I believe it should refer to Eq. 5.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 2599, 2005.