

Interactive comment on “Using different assumptions of aerosol mixing state and chemical composition to predict CCN concentrations based on field measurement in Beijing” by Jingye Ren et al.

Anonymous Referee #3

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This manuscript presents field measurement results in Beijing on size-resolved CCN activity. Closure study is carried out to investigate the effects of mixing state and chemical composition on the prediction of CCN concentration. The conclusion that the EIS assumption is the best way to predict CCN concentration is sound and could be useful for the treatment of aerosol mixing state in the climate models to evaluate the indirect forcing. The major issue with the manuscript is that it is poorly written. Sentences in the manuscript are always ambiguous, making it difficult to understand what point the authors want to make. In general, the manuscript can be published in ACP, as long as

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the written issue and some major concerns listed below are addressed:

Major concerns:

1. My main concern on the results of this manuscript is the size-resolved data. The size-resolved BC concentration in this study is retrieved combining bulk concentration measurement with BC profile from previous SP2 measurement. This method is practicable, but the authors need to understand and discuss the bias brought about by this method. Since SP2 measures BC core diameter instead of the diameter of the BC-containing particle, this method will overestimate the BC concentration in smaller particles and underestimate the BC concentration in larger particles. For example, a 400 nm particle with a 100 nm BC core will be recognized as a 100 nm BC particle by SP2, and thus by the data matrix of this study. Besides, the authors should make it clear how they got the size-resolved SOA and POA.

2. The abbreviations system used in this manuscript is not reader-friendly. It always takes a second through before one can understand what they stand for. Besides, some of the abbreviations are not defined in the text. For example, the “Ra(S)” in equation 6 is not defined.

3. Page 22, part 4.4. I have difficulty to understand this part. Why the volume fraction of organic needs to be assumed when dealing with field measurement data? What does the korg here refer to? SOA or POA?

4. The written issue. Here are some examples which could be improved.

Page 1, Title “. . . to predict CCN concentrations based on filed measurement in Beijing”. “filed” should be “field”.

Page 2, “. . .is crucial for determining CCN number concentration accurately” is suggested to be revised as “. . .is crucial for accurately predicting the CCN number concentration”.

Page 2, line 32, “with an assumption that sulfate, nitrate, and secondary organic

aerosols are internally mixed and that primary organic aerosols, POA, and black carbon, BC, are externally mixed; and the chemical composition is size dependent". This sentence could be misleading. I suppose that the authors want to express "with two assumptions: first, sulfate, nitrate and secondary organic aerosols are internally mixed with each other but externally mixed with primary organic aerosols (POA) and black carbon (BC); second, the chemical composition of aerosols is size dependent". Is that correct?

Page 4, line 77, "...and because the CCN properties of fresh and aged aerosols are different." is better as "...and the differences in the CCN properties between fresh and aged aerosols".

Page 4, line 87, "However, to our knowledge, no CCN closure test that considers not only the chemical composition but also the mixing state in such a polluted urban area has been done." This sentence is poorly organized. Please revise it.

Page 13, line 267, what do the authors mean by "have played a greater role in the particle size mode"?

Other comments:

1. Page 14, line 284, what do the authors mean by saying "...and the secondary transformation of POA with the secondary hygroscopic species"?
2. Page 15, Equ 7, what is $fNCCN/NCCN$ in this equation?
3. Table S1, there is no unit for the data the authors provided here.
4. Page 16, line 320, "This may indicate that particles became more internally mixed through nucleation and coagulation from the Aitken mode to the accumulation mode." I don't think nucleation has anything to do with particle "from the Aitken mode to the accumulation mode". Do the authors mean "condensation" here?
5. Page 15, line 296, I don't think the ref. (Mei et al., 2013) provides any information

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of the equations listed in the manuscript. The authors need to be more careful on their citation and double-check all the references.

6. Page 17, line 344, “At lower SS, the critical diameter on polluted days was larger than that obtained under clean conditions, suggesting that particles with D_p of ~ 40 nm were more difficult to activate under polluted conditions. ” I have difficulty to understand this sentence. Why the critical diameter at lower SS related to D_p of 40 nm? Also, the authors should consider the deviation of the calculation (as shown in Fig.3) before making any conclusion in this paragraph.

7. Page 20, I don't think the authors provides any explanation why EIS overestimate the CCN during rush hour. I would attribute this to the bias of the size-resolved POA and BC, if there is any.

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