Authors: We would like to thank the two referees for the reports.

I appreciate the authors' efforts in making significant improvements to the manuscript in response to my comments. I'd think the manuscript be ready for publication after a few minor revisions.
Authors categorized the current CCN parameterization methods into four groups. The study by Nakajima et al, 2001 only established the relationship between aerosol number concentration and AI, rather than the relationship between CCN concentration and AI. Although there is tight relationship between Na and CCN, a short description is necessary to clarify.

Authors: "(rather than CCN) " has been inserted after "Nakajima et al. (2001) hypothesize that this product, now commonly called aerosol index (AI; not the TOMS/OMI aerosol index), is approximately proportional to the column aerosol number concentration".

In addition, the study by Liu et al (2011, JGR) examined the CCN-AI relationship directly using the observation over the polluted site in China.

Authors: We now say "Liu et al. (2011) examine the CCN-AI relationship directly using the observation over a polluted site in China."

Page 5. The study by Liu and Li actually not only accounts for the size and aerosol vertical distribution effects, but also accounts for the influence of swelling effect by considering the RH for AOD, dry and ambient aerosol scattering coefficients, as well as the potential aerosol composition effect, to some degree, by analyzing the aerosol SSA.

Authors: We have added ", though they provide assessments on the changes in light scattering upon humidity changes and on single scattering albedo (SSA)" after "The impact of hygroscopicity is not directly accounted for".

The studies by Ghan and Collins, 2004 and Ghan et al., 2006, should also be mentioned as these studies addressed the influence of aerosol vertical distribution and swelling effect here.

Authors: Ghan and Collins (2004) is now cited along with Ghan et al. (2006) as related studies using lidar. We do not include these studies in any of the four categories, primarily because their approach is fundamentally different. They do not start with aerosol optical properties. They start with CCN measurements at the surface and scale them by vertical profile of optical properties.

The study of aerosol vertical distribution and swelling effect is certainly related to the present study. We refer to Ghan et al. (2006) in the discussion of vertical profile (Section 4.2).

The CCN concentrations used are for SS of 0.3-0.5%, does it refer to the mean CCN value for the SS range or for all of individual samples from 0.3% to 0.5% SS? The CCN concentration at 0.3% SS and 0.5% SS is different, which indicates that the same AOD may correspond to different CCN concentration. What is the uncertainty in the CCN parameterization due to the difference in CCN concentration at different SS. It should be clarified in the manuscript.

Authors: The CCN concentration at 0.3-0.5% refers to all measurements in this range of supersautration without averaging or adjustment. The only exception is the ARCTAS data that are adjusted to 0.4% supersaturation. "Adjustment is discouraged by the lack of supporting observations (e.g., size distribution) in a statistically significant volume.", as we say in Section 3.2. We discuss the impact of the supersaturation range. "This range is wide

enough to allow sufficient data for regression analysis. But it results in an isolated group of data points for a handful of cases, such as ~10% of the Black Forest data. This effect is evident despite the fact that data points up to one minute after each change in pre-set supersaturation are excluded. This is because the instrument supersaturation at the ARM ground sites, once recalculated for the actual instrument temperature, occasionally takes steps within the range, for example from just above 0.3% to just below 0.5%, rapidly changing the CCN concentration. The rate of this change varies with supersaturation and location. It is relatively high near 0.4% for Black Forest where the aerosol was highly variable with pollution from Stuttgart, organics from agriculture and nearby forest and heavy nitrate fertilization. Some of the isolated data points may be attributable to irregular instrument performance.“ Though difficult to quantify, this effect must be among the reasons for the variance in the estimated slope, which we describe two paragraphs later.

Page 11. I am surprised by your finding that the CCN-AOD relationship is insensitive to the choice of wavelength of the AOD. As suggested in the first round of review, this should be explained more clearly in the manuscript, for it is hard to believe/understand.

Authors: We have added a new paragraph in Section 4.1, immediately after explaining the variability in terms of critical diameter and normalized (by extinction) number distribution. “Of the two elements of the variability, the normalized size distribution is expected to depend partly on the choice of wavelength. But this dependence may be insignificant, because the particle sizes important for the number and the extinction are so far apart. The same calculation for a wavelength of 350 nm instead of 500 nm would lower the extinction peak diameter from ~300 nm to ~210 nm, narrowing the difference from the number peak diameter (~100 nm) but not closing it. And the variability in the critical diameter would remain unchanged. This view makes it less surprising that the ARCTAS CCN-AOD relationship appears insensitive to the choice of wavelength of the AOD (Section 3.1). We need more extinction/AOD data that are spectrally wide and coincident with CCN measurements to study the impact of wavelength.”

Page 17. The seasonal cycles and variability of the CCN-extinction relationship should be caused by the seasonal changes in aerosol types dominated in different seasons.

Authors: We agree. We discuss this in terms of emissions rather than aerosol types, essentially to express the same idea in this context.