Review of the manuscript revision by G. Zhang et al.

Overall, the authors have done well to address some of the referee comments, but not all. There are several major issues that remain, which prevent me from endorsing the manuscript for publication. For the comments that have not been addressed sufficiently, I have the original referee comment below in quotes, followed by my assessment of the changes and additional comments.

Original comment: "For NH_3, a positive correlation is observed between the number fraction of particles with NH_4^+ and NOC, while a negative correlation is observed between the relative peak areas of these compound classes. There is not a reasonable explanation given for this surprising and apparently contradictory behavior."

New comment: The authors' explanation for this observation still does not make sense. The authors state that ammonium is necessary for NOC formation, but that ammonium also inhibits NOC formation. Various factors contributing to this counterintuitive result are hypothesized (e.g., acidity, competition for gas-phase ammonia), but none are consistent with the results in Figure 3c and 3d. The discussion in the revised manuscript (revised manuscript with track changes lines 205 - 214) actually contradicts their finding, since ammonium does not inhibit NOC formation in any of the references cited in this added paragraph. Based on the other referee's comments, it seems there is a possibility that the results in Figures 3c and 3d stem from a measurement artifact (ionization efficiency changing with composition), rather than an actual physical/chemical process occurring in the atmosphere. The authors need to present a detailed, logical argument for this observation.

Original comment: "NOx is completely ruled out as a contributor to NOC formation on the basis of poor (or no) correlations between NOC and NOx. However, this is a misinterpretation of the data. Many factors (different removal processes and lifetimes of particles vs. gasses, primary vs. secondary species, etc.) could contribute to a lack of correlation even if NOx did contribute to NOC formation."

New comment: First of all, the authors used the referee's comment word-for-word in their revised manuscript (lines 460-462 in the track changes version). This is inappropriate, and constitutes plagiarism of this sentence. Clearly, this should be changed.

The above issue aside, I still do not believe this comment was adequately addressed in the revision. There is too much emphasis on simple linear correlations, when that is not expected for the chemistry in this system. For example, NOx controls the branching of VOC reactions, which will in turn affect product distributions, including NOCs; however, this will not (in most cases) result in a linear correlation between NOx and NOCs. Likewise, NOx affects nitrate radical formation, which can form NOCs, but a linear relationship between NOx and NOCs will not necessarily occur even if this is the predominant pathway for NOC production. Further, the references cited in lines 451-453 (track changes version) to support their position do not show linear correlations between NOx and NOC formation in systems representative of a polluted urban atmosphere. Therefore, the possible role of NOx in NOC formation is not accurately described in the manuscript revision.

Finally, the revised manuscript needs editing for grammar and language.