Taylor and coauthors provide a noteworthy and comprehensive set of aircraft observations of aerosols, specifically those which serve as cloud condensation nuclei (CCN), over variable source regions during the COPE field campaign. Additionally, they evaluate air mass sources and predict ice nucleating particle (INP) concentrations based on a set of different ice nucleation parameterization models. Although this work represents a detailed account of observations necessary to improve climate model simulations, there are a few issues that need to be resolved prior to publication in ACP.

General comments:

In attempts to harmonize ice nucleation terminology, Gabor Vali and colleagues published a technical note to define terms used throughout the community. Please use ice nucleating particles (INPs) instead of IN, to align with Vali et al. (2015).

Vali, G., et al. Technical Note: A proposal for ice nucleation terminology. Atmospheric Chemistry and Physics 15.18 (2015): 10263-10270.

Information regarding the instrumentation used is missing. First, what aircraft inlet was used? I am assuming from a brief statement later on in the text regarding not being able to sample in-cloud that an isokinetic inlet was used, but please provide the details in the methods section. Also, what instruments were used to measure CO, vertical velocity, and ice concentration? What are the units for these? Observations of CO and vertical velocity are presented in the manuscript, but information on the instrumentation is not provided. This would be alleviated by providing a few quick sentences in the methods, perhaps in a supporting measurements paragraph.

The altitude of the measurements and flight duration are vital pieces of information that should be provided. The authors do state the flights occurred below ~500 m, but measurements nearest to the surface could vary significantly from 500 m, depending on stratification, emissions, winds, etc. Even if provided by Leon et al. (2015), that information should be provided again here. This would also provide vertical context to the air mass trajectories as trajectories ending at 100 versus 500 m could lead to disparate sources. The authors could address this by providing an image of the vertical sampling statistics, or even a range if sampling was only conducted at 500 m (this is not currently clear).

Along these lines, on page 5, lines 19-20, the authors suggest that aerosols from long-range transport were likely removed via precipitation, but that depends on how high the trajectories were back in time. For instance, the height of a trajectory endpoint 3-4 days back could be thousands of meters. This statement would be more valid if vertical profiles of the trajectories were shown, perhaps as separate panel(s) in Figure 1. Also, what about trajectories at the ground site, since the authors present data from this location? A connection between the ground and location of aircraft observations would result from also running trajectories where the WIBS measurements were acquired.

The N_{GA} and N_{UGA} data and discussion are not pertinent to the main themes of the manuscript. Further, the values are quite low; are these values of any significance compared to total CCN concentrations? I suggest eliminating this information as it is only discussed in one small paragraph and does not add any significance to the main conclusions. What the authors could instead do is provide mean size for each day, perhaps for each size distribution mode, to provide some sizing context to the CCN.

Are there enough sampling statistics to look at vertical profiles of the aerosol measurements? That can provide quite a bit of insight into the sources and transport of aerosols. At the very least, this can be done with the SMPS, PCASP, and CDP. Also, RH, mixing ratio, temperature profiles would be helpful to show.

Why is SEM-EDX introduced in the methods, but no results are provided? The single particle chemistry would serve as quite useful information for characterizing the aerosol.

Given the temperature range presented in the last section, I would expect the ice particle number concentration to equate to several orders of magnitude higher than INP concentrations (i.e., Hallett-Mossop). The authors should revise this section to include secondary ice formation as a plausible reason and tone down the element of surprise.

Specific comments:

The abstract could use a sentence or two regarding the broader impacts and motivation for the work. By adding some "big picture" material, the significance of this work is evident right off the bat.

Introduction: Similar to the abstract, end the first paragraph with a direct statement to segue into the next paragraph, i.e., something along the lines of, "The potential of flooding from persistent convective clouds along the peninsula demonstrate the importance to understand cloud formation in this region."

As I and potentially other readers are not familiar with this region, it would be helpful to point out Figures 1 and 2 in the first paragraph for geographical context.

Page 1, line 27: Define that the parameterizations are for prediction of ice nucleating particle concentrations. Parameterizations is somewhat vague.

Page 2, line 13: Can also inhibit cold precipitation by reducing riming efficiency of descending ice particles in a mixed-phase cloud system. Also, replace "lower" with "subzero".

Page 2, line 14: "..such as riming and the Hallett-Mossop rime-splintering processes." Also, the aerosols themselves do not "initiate" secondary ice formation processes, the conditions such as temperature, updrafts, etc. do.

Reference to Figure 2 at the beginning of Section 2.1 for examples of the flight plans.

Page 5, line 18: What synoptic charts? Either provide a reference (paper or website) or synoptic maps as a figure. More information would also support the statements on page 5, lines 24-25.

Page 7, line 21: This would be a good place to discuss the variability in the winds and why, say, for 18 Jul the coastal and marine sections were relatively polluted (relatively stagnant winds unlike other days where faster winds introduced marine-soured air to the coast). In general, it would be useful to directly link relationships between winds, chemistry, and size. This is done to some extent, but should be clearly highlighted.

Page 7, lines 29-30: For the non-AMS crowd, please provide information on what these fragments are and clearly highlight which indicated a more oxidized OA.

Page 11, line 32: Is the "polluted case" Jul 18? Please define.

Page 14, line 12: This is true, for mineral dust compared to purely biological particles. However, we do not yet know the extent to which biological material within or on dust contributed to the nucleation of ice, i.e., determining if the mineral or biological components are what is nucleating on a single dust particle.

Page 15, lines 18-24: What is this value compared to other days the WIBS was operational? I understand the authors was to use the data to extrapolate to what might have been observed at aircraft level and solely focus on the flight days, but this does not seem valid due to the fact that information regarding the time and height of the aircraft over the site is not provided (i.e., to demonstrate what was observed on the ground was potentially observed aloft and 2). It would be helpful if the authors could provide more information to justify the use of the ground-based fluorescence to compare to aircraft, otherwise the INP concentrations compared to those estimated from parameterizations used for the aircraft data do not seem comparable. Also, the WIBS is briefly discussed, yet what types were used to calculate the concentrations (Type ABC)? Is this information found in a different publication on COPE? If not, please provide more details on the measurement.

Figure 2: The black arrows for "other" are distracting. It would be useful to remove these arrows from the picture as they are unneeded information.

Figure 4: Can the authors adjust the y axes in panels a, c, d, e, and f to show the same range? Panel b has much higher concentrations thus can remain.

Figure 7: Add that the dashed and solid lines are SMPS and PCASP distributions, respectively.

Figure 8: Jul 05 should be Jul 18 (typo in figure). Also, adding values for the vertical updraft speeds and total CN since these are discussed in the text.