General Comments:

This is a very extensive observational study of cirrus clouds having near-global coverage, based on aircraft flights funded through NSF (7 campaigns) and NASA (5 campaigns). In particular, the smallest ice particles of the ice particle size distribution (PSD) are sampled, down to 1 μm, thus providing more useful information regarding the ice formation pathways (i.e., heterogeneous vs. homogeneous ice nucleation; henceforth het and hom). These ice PSD measurements are unique in that they have complementary measurements of relative humidity (RHi), aerosol particle PSDs, and vertical velocities (w). These complementary measurements are related to the ice PSD properties of ice water content (IWC), mean maximum dimension (Di), and number concentration (Ni) using a delta-delta method that correlates their fluctuations with those of the complementary measurements. Lastly, machine learning techniques are applied to better understand how IWC is affected by the complementary measurements.

I share the concerns expressed by Reviewer 1 regarding the practice of using in-cloud aerosol measurements since Na(500) (i.e., aerosol concentration between 0.5 μm and 1.0 μm) may be mostly small ice crystals. Why should fluctuations in Na(500) be so strongly correlated with fluctuations in IWC? What plausible physical process would explain these strong correlations? Using aerosol measurements just below cloud base could remedy this concern. Alternatively, if it could be shown that N_i(1-3μm) (i.e., the ice crystal number concentration between 1 and 3 microns as measured by the FCDP) is orders of magnitude less than Na(500), then it might be argued that ice crystals are a minor component of Na(500).

Our recent research shows that IWC and Ni track each other very closely when hom occurs. If Na for D > 0.5 μm here is strongly affected by Ni (as suggested by Reviewer 1), then these strong correlations may be partly due to fluctuations in hom (associated with higher IWC) correlated with fluctuations in Ni (associated with hom). Alternatively, one could argue that ISSRs (ice supersaturated regions) are common with higher INP (ice nucleating particle) concentrations impacting the RHi within these ISSRs to various degrees, resulting in correlations between IWC and Na(500) fluctuations. Such physical interpretations of these results are needed, even if they are only working hypotheses.

Specific Comments:

1. Lines 25 – 26: I don't see the justification for this statement (cirrus coverage of 20% to 40%). Sassen et al. (2009) estimates that global coverage for cirrus is 17%, while in Mace and Wrenn (2013), I could not find any mention of coverage.

2. Lines 48 – 50: Please support this statement with a reference. Cziczo et al. 2013 seems appropriate.

3. Line 107: Jensen et al. 2017b is cited for POSIDON but POSIDON is never mentioned in that article, which is concerned only with ATTREX. A good POSIDON reference is Schoeberl et al. (2019, JGR).

4. Lines 129 – 131: These PSD properties are defined in the abstract but not the text. Is that consistent with ACP policy? Also, Di is defined as number-weighted mean diameter, but diameter applies only to spheres. Please provide a more accurate definition, such as mean maximum dimension.

5. Lines 171 – 182: Regarding the NSF data, the Fast-2DC has a physical measurement range from 62.5 um to 1600 um, with 25 um bin widths (as stated here). Throwing out the first 3 bins would then limit the sampling range to 137.5 um to 1600 um. The measurement range of the CDP is from 1 to 50 um. This leaves a 87.5 um gap between 50 and 137.5 um. How is this gap addressed?

6. Line 247 – 250: At the bottom of Sect. 5.1.1 in Kramer et al. (2020) is the statement: "Because of the dangerous nature of measurements under such conditions, the frequency of convective – and also orographic wave cirrus – is underrepresented in the entire in situ climatology." Could this be an issue in this dataset as well? Moreover, in Sect. 4.2.2 of this article, we find "the higher (Ni) values at warmer temperatures in the Krämer et al. (2009) data set (Fig. S6, Supplement) were caused by flights where lee wave cirrus behind the Norwegian mountains were probed". To summarize, higher Ni values are associated with orographic gravity wave (OGW) cirrus clouds, and OGW cirrus are characterized by higher updrafts, more conducive to hom. In the satellite remote sensing studies of Gryspeerdt et al. (2018) and Mitchell et al. (2018), there are large regions of elevated Ni over and downwind of mountain barriers. Figure 1 of this paper does not show much sampling of such regions. Is it fair to say that OGW cirrus may have been under-sampled in these datasets leading to an underestimation of hom in the midlatitudes and polar regions? If so, please indicate this in the article.

7. Lines 272-275: Perhaps it is worth mentioning that the NSF data exhibits median Ni values near log(1.5), or 32 L-1, which is similar to median Ni in Kramer et al. (2020).

8. Lines 436 – 441: After studying Table 3, the "take-home message" for me is the following: At scales of 50-s or greater, dT + dRHi appears to be the most influential IWC predictor for quiescent cirrus. The 250-s scale appears to be the best IWC

predictor for non-quiescent cirrus (regarding dT + dRHi), with dlogNa(500) also having an impact in addition to dT + dRHi. Should something along these lines be stated here?

9. Lines 444 – 448: For quiescent cirrus, it is true that IWC peaks ~ 110% RHi, but for non-quiescent cirrus, IWC peaks for RHi > 150%. Please consider mentioning this important finding.

10. Lines 455 – 459: In Fig. 10, what is responsible for the differences between panels a-b-c in the 1st row and panels d-e-f in the 2nd row?

11. Lines 513 -514: For a broader perspective, please mention the cirrus climatology of Kramer et al. (2020) and the number of field campaigns employed and their latitudes, surface types (land vs. ocean), and other relevant factors.

12. Lines 514 – 516: As mentioned, the properties of orographic gravity wave (OGW) cirrus may differ considerably from other cirrus clouds and be widespread in coverage as noted in Joos et al. (2008, JGR), Barahona et al. (2017, Nature), Kramer et al. (2020, ACP), Mitchell et al. (2018, ACP), Gryspeerdt et al. (2018, ACP), Lyu et al. (2023, JGR) and other studies. In Kramer et al. (2020), it was stated that OGW cirrus clouds were under-sampled due to dangerous flight conditions resulting from the higher updrafts. The same is likely true of this study. Please mention here this need to sample OGW cirrus clouds.

Technical Comments:

1. Lines 159 – 161: The FCAS, being an aerosol probe, must have a measurement range of 70 - 1000 nm, not microns.

2. Line 433: Possible typo: 50 km => 250 km?