Tatzelt et al. present results from detailed measurements of CCN and INP over the Southern High Latitudes during the Antarctic Circumnavigation Expedition (ACE). These measurements are extremely valuable to the field of polar aerosol-cloud interactions. The authors include a discussion of how these measurements compare with previous measurements of CCN and INPs and also perform correlation analyses with co-located aerosol metrics in aims of determining the source origin of measured CCN and INP abundances and variability. Overall, there is massive value in these measurements and the manuscript is well written with few typos. However, there are many studies in the literature that have not be considered in the interpretation of the INP data that I think will add significant scientific value to the discussion. Additionally, I found several statements that were not adequately supported with data. As such, I recommend publication in ACP after the authors address the major concerns identified below.

Major Comments:
1. The reported INP number concentrations were compared to the Bigg 1973 measurements. However, more recent measurements reported by McCluskey et al., (2018) indicated up to 100 times lower INPs compared to the Bigg 1973 survey. Many, perhaps the majority, of INP number concentrations in this study are significantly higher than those reported by McCluskey et al. (2018) and more recently McFarquhar et al. (2021), which is interesting. The more recent data is only briefly mentioned and discussed in comparison to the Bigg survey. These ACE measurements should be discussed in the context of both Bigg (1973) and McCluskey et al. (2018) datasets for completeness.
2. The authors indicate that an assessment of air mass origin is not possible for the ACE observations due to a lack of a tracer measurement (lines 403-404). Given the difference in these measurements and those reported in McCluskey et al. (2018) and more recently McFarquhar et al. (2021), it would be extremely valuable to determine if the air masses originated from open ocean waters, the Antarctic coast, or one of the surrounding land regions. While dust concentrations are extremely low over the Southern Ocean, it is widely recognized that the ice nucleation ability of dust is significantly greater than sea spray aerosol (DeMott et al., 2016) and so even small amounts can significantly influence the measured INP. Dust sources have been identified for Antarctica (e.g., Neff et al., 2015). Have the authors considered using something like HYSPLIT back trajectories to determine
3. The authors reference the DeMott et al. (2010) INP parameterization briefly. However, studies (e.g., Vergara-Temprado et al., 2017) have found limitations of DeMott et al. (2010) for marine regions due to the fact that the data used to develop the DeMott et al. (2010) parameterization did not include marine data. Why have the authors not included a comparison against existing marine INP parameterizations based on total aerosol surface area (McCluskey et al., 2018) aerosol volume (Mitts et al., 2021), which could be tested against the measured aerosol distributions from the merged size distributions?

4. Why are values above or below the INP detection limit included in figures and analysis? I find this very misleading and distracting. By including the below (above) detection limit values, the impression will be that the INP number concentrations are higher (lower) than what they may actually be. Because you are unable to measure, these values should simple be excluded (NaN).

**Introduction**
- Lines 83-85 - I do not believe the CAPRICORN II INP measurements have been published other than in the McFarquhar et al. (2021) overview paper; McCluskey et al. (2018) only includes CAPRICORN I. (see also Lines 90-92)
- Note that Bigg also hypothesized a decade decline in southern ocean INP concentrations (see Bigg 1990).
- Line 95: "In consequence, a dominance of sea spray on INP was concluded." - this should specify that the Southern Ocean *marine boundary layer* was dominated by sea spray INPs.
- Figure 1 - Can the locations of the key ports and locations be added for the reader to more easily follow along?

**Methods**
- What size range is measured with the CCN instrument and how does this compare to the PM10 ionic composition measurements used in the correlation analysis? Can the authors provide some insight into the size ranges when introducing the correlation analysis in Section 3.4?
- Line 180 - "sampling time (<1 to 1437 min) dependent on the automatic shut-down mechanism." - are the sampling volumes available or perhaps a mean/median sampling volume with standard deviation/IQR would help inform the reader of the typical sampling volume, since this statement suggests the sampling time ranged from 1 minute to ~24 hours.
- Lines 176- was the automatic shut-down mechanisms used for both LV and HV? Please clarify.
- Line 233 - should this be "specific depths down to 200 m" ?

**Results:**
- Figure 2 - The caption mentions data were removed for instrument availability and ship exhaust filtering. There are many instances where CCN data are available, but the size distribution measurements are missing; are all of these times when the SMPS & APS were down?
- Figure 3 - It's quite challenging to see the differ points. Suggest to widen the size of this figure such that the space between the supersaturation bins are wider?
- Line 296 - ".. cancel each other out so that no clear latitudinal" - should this be longitudinal?
- Line 307 - Can the authors expand on the second reason for differences (number of
measurements during Austral summer is higher (tree years)? Why would this lead one to expect differences?
- Line 308 - I found the wording of this confusing; suggest change to "Disagreement of CCN concentrations at lower SS between Herenz et al. (2019) and this study suggests that differences arise from larger particles that are typical of activation at lower SS"?
- Line 310-316 - I think the authors are trying to describe two sets of data that are included in Figure 3, but I think the labels may be inconsistent with the text - The "all data" I think refers to the first portion of this discussion and the "baseline" data refers to the later. However, the text refers to these as "baseline" (Line 313) and then mentions baseline values removed but reference the triangle right (line 316), which is labeled as "CGBS baseline" in Figure 3. Can the authors clarify?
- Line 317 - "We conclude that terrestrial influence on our average values is small" should specify average values of *CCN_0.5*, since data are unavailable for other SS.
- Line 322 - suggest to include definition of BSO again here.
- Lines 300-330 - I appreciate the thorough review of previous measurements - I do find the discussion challenging to follow with the different seasons, years, SS, etc. I suggest adding a table that summarizes these aspects. Also, are all of these studies evaluating the same aerosol size range?
- Figure 4 - Should the hygroscopicity parameter values be on a linear scale (as in Figure 3B)? Also, could points be added to these PDFs (and to Figure 7) to provide an idea of the bin sizes used?
- Figure 5 – Are these data from the LV or HV filters? Please clarify. (same for Figures 6&7)
- Line 398 - Values above the upper detection limit should be excluded, see Major Comment 4.
- Line 403 - "assessment of air mass origin is not possible for ACE" - See Major Comment 2.
- Line 411 - "the derivation of N_INP was fundamentally different" - how was it different?
- Lines 411-415 - McCluskey et al. (2018) also discussed the hypothesis put forth by Bigg (1990) that a decadal decline in N_INP was possible over the Southern Ocean. INP concentrations reported in McCluskey et al. (2018) are also consistent with more recent measurements (McFarquar et al. 2021); It is not clear to me why so much focus is on comparing to Bigg (1973) without also considering more recent data. See Major Comment 1.
- Line 419 - See Major Comment 4.
- Line 440 - The Tobo et al. (2013) study was specific to terrestrial primary biological aerosol particles (PBAP) and I am unaware of marine INP studies that have identified a relationship between PBAP and INPs in a marine environment.
- Line 459 - "In other words, the PDF given is representative for the marine environment" I do not think the analysis supports this statement. It is also confusing to follow this up with "Note that INP active at this temperature range can be either of mineral nature and long-range transported from terrestrial sources, or originating from marine sources."
- Figure 8 - Are all samples included here, even ones with expected terrestrial influence from the ports (Grey shaded periods from Figure 5)? Suggest to segregate data using the land proximity data (i.e., data within grey shade versus not shaded from Figure 5).
- Line 479 - Note that a similar range of INP number concentrations is reported in McFarquhar et al. (2021) that expands the seasonal and spatial representation.
- Section 3.2 - I find it unclear if the authors think INP observations are thought to be representative of "open ocean" or were influenced by terrestrial air masses, or both throughout this discussion. I think it’s both (based on abstract), but overall I suggest reorganizing this to have a clear consistent message for the reader.
- Line 560 - What wind speeds were typical? I do not see windspeeds included anywhere. Is the relationship between sea salt or SSA and windspeed expected to be linear?
- Line 565 - Can the authors clarify how the PM10 measured size range compares to the CCN measured size range (from Line 345: "For the size range between roughly 37 and 123 nm probed with our SS (Tab S1)") and what this would mean for comparing mass
concentrations from the full size range from the HV filters?
- Line 591 - What is meant by a "delayed connection"?
- Lines 591-598 - Are the authors aware of the existing marine INP parameterizations based on total aerosol surface area (McCluskey et al., 2018) aerosol volume (Mitts et al., 2021) - see Major Comment 3. Aerosol type-specific parameterizations may provide a lot of insight as to the INP type.

Conclusions:
- Line 632 - "INP spectra (Fig 8) for the most part of the cruise feature similar levels and temperature range between -12 and -24degC." - What is meant by "similar levels"? I see 3 orders of magnitude variability in N_INP for any given temperature between -20 and -7degC in Figure 8. From the abstract (Lines 12-13) "[INP] concentrations spanned up to 3 order of magnitude, e.g., at -16degC from 0.2 to 100 m-3."
- Lines 634 - "We interpret this signal [correlations between N_INP in the temperature range between -12 and -24degC] as indication that mixed long-range-transported populations of INPs of biogenic origin (T>-20 deg C) and mineral dust (T <. -20degC) were present. " - Again, it is not clear to me that this statement is supported by evidence available in this study.

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