The paper by Tencé shows 14 years of PSC observation from Dumont d'Urville. However, the title of the paper is misleading. From the title and introduction, one expected a more detailed presentation and analysis of the observed PSC above Dumont d'Urville. Since PSC observation by ground-based lidars are rare in Antarctica a more in-depth presentation of the dataset would be of interest for the community. The lidar at Dumont d'Urville is in operation since 1989, why do you only focus on the last 14 year? Did the occurrence of PSCs in general and of different PSC types changed since 1989? Do you observe a trend since 1989?

The paper need major revision and the authors need to add a more detailed analysis of the data set.

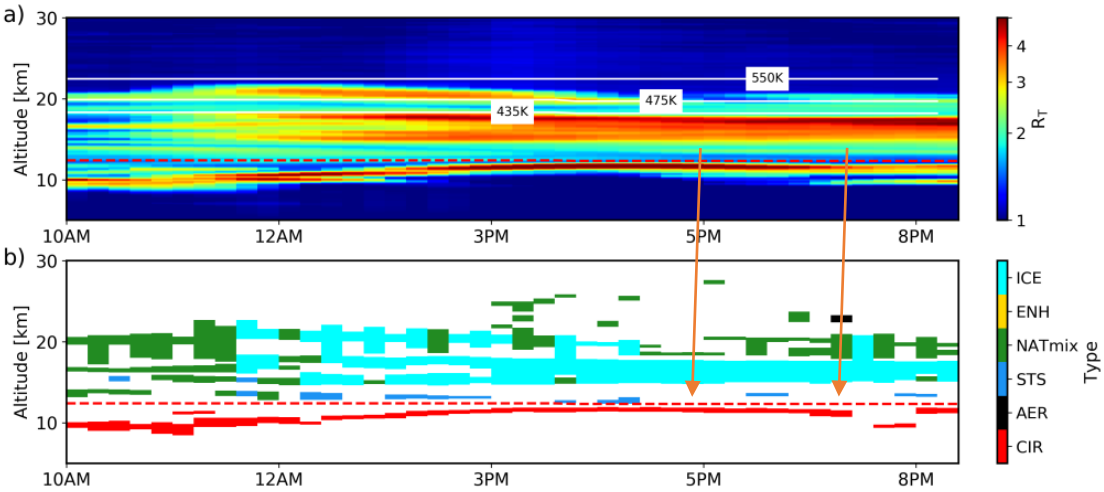
Major comments:

- It would be good if the authors could add a paragraph about the lidar measurements including some statistics, e.g. How many days per year the lidar was operational? How many PSC were observed per year? When were there observed? Are the PSC observation evenly distributed over the winter?...
- Please include a more detail analysis of the 14-year dataset: e.g. year to year variability, comparison to other ground-based stations and CALIPSO
- Section 4.1:
    - The difference in ICE PSC occurrence needs more discussion. Tesche et al. (2021) shows an ICE occurrence of around 15% compered to 3.7% shown with P18. Could you please provide a pie chart from DDU for the same time period used in Tesche et al. (2021).
    - Also, it would be good if you could provide a plot showing PSC occurrence per year (e.g. histogram). I would assume the ICE occurrence varies from year to year and that it was higher in the years 2015 and 2020, when the ozone hole set record sizes (see Stone et al. 2021).
- Section 4.4. Why do you decide to use NCEP here? In the section before you concluded that NCEP has a T bias from 2k and ER5 should be used. ER5 and IASI should be used here.

Minor comments:

- Line 279: What threshold is used for background aerosol?
- Line 280: Sentence starting with: *This might….. .* Not clear what is being referred to.
- Line 277: If you have a 3% crosstalk at DDU, would that not shift all the observation in Figure 3a slightly to the upper right corner?
- Line 282: *prominent share of NAT class among global* Please add citation.
- Line 285: not sure what is meant by that statement
- Line 284: not clear. B05 has a small occurrence of pure NAT clouds, but NAT is included in MIX. And MIX is quite high in B05.
- Figure 3. Add measurement date in the figure caption. Also, pleas add σ_{aer} to B05.
- Line 305. Misleading. MIX is mixture if different types (STS, ICE and/or NAT), not a completely different typ of PSC
- Figure 4.

    - Subplot a and b look the same, as well as d and e. Does that mean that STS and NAT+MIX between B05 and P11 agree very well?
    - The bimodal distribution for ICE in B05 is very interesting. Could you provide the mean d_{aer} for the peaks?
- Line 345: Really just the drift, not also the different resolution?
- Figure 5a: Looks very smooth. Too much interpolation? And the signal just above the tropopause is not classified. Why?



- Line 384: For the temperature comparison. What resolution were used? Did you interpolate the radiosonde profile on ER5, IASI, and NCEP?
- Section 4.4 and Figure 8. A discussion about the year to year variability of the ozone hole and PSC occurrence should be added here. For example, you could add a second y-axis showing the average ozone hole area for every year.
- Figure 9: What would be the thickness for your definition ($T-T_{nat} < -2k$). Would the model thickness agree than better with the observation?

General comments:

- Figures are to small and of low quality

Stone, K. A., Solomon, S., Kinnison, D. E., & Mills, M. J. (2021). On recent large Antarctic ozone holes and ozone recovery metrics. Geophysical Research Letters, 48, e2021GL095232. https://doi.org/10.1029/2021GL095232