This 'brief communication' paper presents a comparison of Antarctic precipitation rates as derived by CloudSat, and as simulated by ERA5 and CMIP5 and CMIP6 climate models. The results indicate that there are substantial biases in ERA5 and CMIP models, and show no clear improvement from CMIP5 to CMIP6. While this is relevant work and a potentially interesting work, I am not sure why the authors chose to turn it into a 'brief communication' paper, given that I think many results are somewhat unsubstantiated and/or incomplete, and important details are missing. Below, I'd like to highlight two crucial items that I consider prohibitive for publication of this work, and would need to be worked on in case the authors would like to revise the paper. I will not focus on minor/textual items at this point.

CloudSat quality: While the authors present CloudSat as a benchmark data set, its performance over the Antarctic interior is highly doubtful. Across the ice sheet interior, a large fraction of annual the snowfall is generated within the shallow (stable) boundary layer by vapor (re)deposition and/or diamond dust. Since the depth of this inversion layer is typically (much) shallower than 1200 m, CloudSat fails to detect much of the precipitation in the interior. Recent studies have focused comparison with in-situ radars and other products in the coastal or escarpment areas (e.g. Lemonnier et al., 2019; Souverijns et al., 2018). The fair agreement of CloudSat with ERA-Interim as presented in Palerme et al., 2014 (TC) is associated with the well-documented dry bias in ERA-Interim in the interior (e.g. Medley and Thomas, 2019 (Nature Clim. Change)), which is confirmed here to some extent with ERA5. Based on this, I think the authors cannot claim that CloudSat is reliable in areas >2250 m (or even lower), and this analysis should be removed. The authors could instead focus on coastal areas only, where CloudSat is likely performing better, although uncertainties are likely still substantial where topography is complex and where sublimation of suspended snow particles can play a role between 0 and 1200 m (Grazioli et al., 2017 (PNAS)).

Temperature and precipitation: the authors aim to relate the bias in precipitation to temperature biases, suggesting that although the temperature is improved in CMIP6 relative to CMIP5, precipitation is not. I think this analysis falls short in multiple aspects:

(a) As the authors surely can confirm, there are many levels of complexity involved with precipitation formation, and only focusing on near-surface temperature definitely understates these complexities. While there might be some sort of relation between the two, the authors should focus on many other aspects of precipitation process (e.g. cloud microphysics, atmospheric (thermo)dynamic structure, humidity, etc. etc.). I simply don't think it is acceptable to argue that model performance in near-surface temperature and precipitation are related.

(b) The statistical analysis of near-surface temperature needs to be expanded. Only the RMSE is currently shown, but that fails to represent the mean bias (i.e. does the mean bias improve between CMIP5 and CMIP6?). Moreover, there is no analysis of statistical significance whatsoever, which is clearly necessary if the authors want to claim that temperature representation has improved in CMIP6.

One smaller issue is that Figure 1 should mask out regions south of 82 South. What are the white areas.