General comments

The paper by Grazioli et al "Measurements of precipitation in Dumont d'Urville, Terre Adélie, East Antarctica" presents analysis of new and unique in that region observations of precipitation at DDU station located in the coastal region of East Antarctica. The measurements include both in-situ (Pluvio, weather station) and remote sensing measurements (MRR and MxPol radars, lidar), which are compared to ERA-Interim reanalysis data. Such comprehensive measurements approach in order to quantify precipitation and also separate it from blowing snow on a long-term basis is much needed in Antarctica. This work contributes to an important question of quantification of precipitation over the Antarctic ice sheet and its representation by ERA-Interim reanalysis, which is commonly used for both analysis and as model forcing.

However, several important issues must be addressed before publication. The methodology has to be described in more details. The authors should not overstate their conclusions by saying that they have constrained estimates of precipitation in the region by using Z-S relationship derived from MRR and Pluvio during summer. A detailed description of calculating this local relationship is required in order to assess the validity of the results. Summer snowfall microphysical properties influencing radar Z can differ drastically from the rest of the year. Thus, using summer Z-S relationship can simply introduce a bias or a preference towards particle shapes and sizes observed in summer.

Methodology of data post-processing for each instrument and also their synergistic use has to be more detailed. Please see my specific comments.

Specific comments:

Abstract:
line 10 : "Climatological data..." - in what sense the term "climatological" is used? Are ERA-Interim data compared to observations for the year of measurements? Then "climatological" is inappropriate

Methods:
p. 3, line 18: "to name a few" - please name all relevant variables and their details

p. 3, line 26: MRR is not sensitive to clouds and sees only precipitation (it can be also precipitation not reaching the surface, i.e. "virga"). The authors should emphasize that MXPol is the only "weather radar" sensitive to both clouds and precipitation, while MRR should be rather called "precipitation radar". MRR will not only miss supercooled liquid clouds but also the ice clouds - except for those that are precipitating and thus can be classified as virga rather than clouds. These aspects have to be discussed when presenting the radars products.

p. 4, Fig. 1: add DDU location to the map

p. 4, line 35: What is the first useful range of MXPol radar? How is it influenced by the ground clutter?

p. 5, line 56: MASC camera has to be described in more details - what is the size range of detected particles? Are there any bias estimates due to the under-catch? What is the raw data processing methodology?

p. 5, line 59: details of the weather station? location, instruments, accuracy...

p. 7, line 81: "we optimized a local relation..." - this local Z-S relation is one of the most important milestones of this paper thus the authors have to explain in more details how it was calculated. The authors are saying that MRR and Pluvio measurements were used for calculating this Z-S relation. Why not using MASC which gives information about particle size and shape?

p. 8, Table 2: Please introduce all abbreviations from Matrosov 2009 reference.

The authors should justify why Matrosov 2009 relationships are applicable to DDU location. There are numerous other publications with various Z-S relationships.

p. 8, line 94-97: I would use MRR-detected precipitation as absolute truth with caution. MRR can easily miss very light ice precipitation to the ground, which in turn can be detected by Pluvio gauge. I recommend to do the following test: cluster Pluvio data by wind speeds with and without MRR-detected precip (precipitation at DDU is always accompanied by strong wind speeds thus is difficult to separate from blowing snow), and also check if "phantom" precip occurs just BEFORE MRR-detected precipitation.

p. 9: ERA-Interim data - is the grid closest to DDU location used or a mean of surrounding grids? Please give details

p. 10, line 15: the method of particle classification using MASC should be described in more details

p. 10, line 20: similarly, the classification method using MXPol should be described

p. 10, line 19: "The instrument being close to the ground level..." - at what height exactly agl MASC is located? This is important in interpretation of precip/blowing/drifting snow contribution

p. 11, fig 7: x-axis should be "fraction of occurrence, %"

p. 11: I don't agree with the statement that the majority of small particles can be assumed to be blowing snow particles picked up from the surface. Precipitating particles of small sizes are also common in Antarctica. And I expected the

observations described here to shed more light into this issue rather than using common assumptions.

Contribution of blowing snow to the small particles can be estimated by separating MASC-analyzed particles for clear-sky blowing snow events and precipitation events.

p 12, Fig 8: How is the riming index obtained? Please add units and values to the y-axis. All abbreviations used in naming snow particles in the pie-chart must be explained. "The riming index is undefined for "small" particles" - what is the threshold for defining "small" particles?

p. 12, line 53: "...though the curves do not co-fluctuate well". Please specify what do you mean. What is the correlation coefficient?

p. 12, line 54: "this optimized Z-S relation provides estimate that are close to the B90A relation.." Specify what is the B90A relation of Matrosov 2009. Why is the local Z-S relation close particularly to B90A? In Table 2, the reference is Matrosov et al 2009, while here Matrosov 2009. Please correct.

To my opinion, the authors cannot say that they have reduced the uncertainty in quantifying year-long snowfall rates by applying local optimized relationship calculated using only summer measurements. In order to justify this, seasonal evolution of cloud particles has to be analyzed. While this is a subject of future analysis, the authors should discuss limitations of their method.

p. 13, fig 9: Caption should be completed and self-explanatory

p. 16, line 45 and section 4.2: MRR cannot capture blowing snow... I am asking the authors to clarify this issue and show more clearly using other measurements (Pluvio, AWS) that MRR sees nothing during clear-sky blowing snow events

Technical corrections:

p. 2, line 31: e.g., Konig-Langlo et al.. (as this is not the only paper about human meteorological measurements)

p. 2, line 37: "for the medium..." - delete 'for'

p. 12 and throughout the text: "cumulated" -> either accumulated or cumulative

p. 12, lines 4-5: "within what could be observed...." - the whole sentence should be rephrased

p. 14, Table3: units? mm we?