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Interactive comment on "Evaluation of CloudSat snowfall rate profiles by a comparison with in-situ micro rain radars observations in East Antarctica" *by* Florentin Lemonnier et al.

Anonymous Referee #1

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The present work compares and evaluates vertical profiles of CloudSat precipitation product 2C-SNOW-PROFILE with profiles of MRR vertical pointing radars located at Dumont d'Urville and Princess Elisabeth stations in Antarctica. The four considered cases show very good and encouraging results on the comparison (correlation 0.99). This is a really promising result since right now, CPR onboard CloudSat is the only instrument able to give precipitation information over most of Antarctica continent with a reasonable time resolution. The manuscript is very well written, data and methodology are thoroughly described and results are presented in a systematic and logical way. I want to congratulate with the authors because nowadays, as a reviewer, it is very hard to find good work well presented.

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I suggest the publication of the manuscript with minor revisions, though I see some points that I would like the authors to clarify.

(1) 4 concurrently snowfall events are considered over 77 actual overflights of CloudSat over the two considered stations. What was the problem of those 77 "events"? probably either MRR or CPR did not detect snow (or both of them). I understand that the detection problem is probably out of the scope of this manuscript, but I would suggest to mention it and explain why those events are not considered, to give to the reader the idea that the problem of snowfall estimate over Antarctica (and in general over the Globe) is not just to quantify it, but we have to deal with detection first of all.

(2) P.5 l.12: the authors provide the Souverijns et al. (2017) Z-S relationship for PE station MRR. As far as I know, the MRR2 have been calibrated with CloudSat, doesn't this introduce a bias in the results of the present work?

(3) P.9 l.26: "in comparison with the quantiles of the vertical structure of precipitation": this should be better explained. I guess you are referring to the black and grey lines in fig.3 that are the 20th, 50th and 80th quantiles and the average precipitation profiles, but also in the figure caption, there is just a reference to Duran-Alarcon et al. I suggest adding some more information both in the text and in the caption to explain better where those plots come from, if they are an average calculated over the station over a certain time period. Moreover, if I am correct, Duran-Alarcon et al. provided reflectivities, how did you get to the snowrates?

(4) P.9 l.31: "also a systematic difference in the CloudSat calibration": this sounds a bit tricky since MRR2 has been calibrated with CloudSat, correct? I guess it is more a sensitivity issue since W- band radars can detect much lighter snowfall than K-band ones.

(5) P.11 l.4: "this precipitation event is representative of the climatology of PE": again, as for p.9 l.18 and l.26 make clear why from the comparison with Duran-Alarcon et al. the event is representative of the climatology (clarify the black and grey lines on the

plots).

(6) P.12 l.5: what do you mean in this case with “higher dispersion”?

(7) P.12 l.22: “by applying to CloudSat profiles the calibration difference estimated in the previous section..”: in this case I don’t actually understand the procedure you are adopting. You are comparing CloudSat to MRR to evaluate CloudSat, so you are considering MRR as your “truth”. But it is known that k-band radar has issues with the detection of light snowrates, so the correction applied doesn’t seem to be fair. I would rather look for a minimum detectability threshold for MRR and compare just the rates that both of the sensors are actually able to detect. The comparison of snowfall between different sensors is an hot topic right now and for sure not an easy manageable one, we need to be really careful on the conclusions we take from it.

(8) P.12 sec.4.4: since CloudSat product comes with its own uncertainties, why not consider also them in the analysis and give some advice to the final users of the products that most likely will use that values for their analysis?

Minor comments:

(1) p.3 l.1: use capital H for HYDRological.

(2) p.6 l.5: what do you mean with CloudSat “phase”?

(3) p.6 l.7: “corresponds to a distance”: at a first glance this could be confused with the distance from the station, I would suggest adding “covers a distance” or something similar.

(4) p.6 equation: I would suggest using “Vwind” or “Vw” for wind velocity, seems more intuitive.

(5) Fig. 2: in fig. 2g and j include the north direction as you did for the previous two maps.

(6) Fig. 2: here you mention the grey plane disk, in fig.1 was the white disk, be consis-

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tent.

(7) Fig. 3: as mentioned on a previous comment, clarify the quantiles information.

(8) Fig. 3: The 80th quantile line in fig.3c became for some reason orange over the shaded orange area instead of gray.

(9) Fig. 4: on the legend use station name and date instead of day number.

(10) Fig. 6: it is not clear from the caption if you are considering each vertical bin of each profile of each overpass (for the 4 considered cases) and then the average value of all of them or if for each overpass and each vertical bin you consider their own average and calculate the deviation from that.

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