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Atmospheric Measurement Techniques Discussions
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Interactive comment on "A variational technique to estimate snowfall rate from coincident radar, snowflake, and fallspeed observations" *by* Steven J. Cooper et al.

Anonymous Referee #2

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Review of "A variational technique to estimate snowfall . . ." by S.J. Cooper et al.

The paper describes how observations from the innovative in situ sensor called Multi-Angle Snowfall Camera (MASC) can be used to constraint a snowfall retrieval scheme from non-Rayleigh echoes detected by a vertically pointing radar working at the attenuating frequency of 35 GHz. The retrieved snowfall amounts are then compared with reference observations during only 5 events, 3 of them weak (total amounts of these 3 events corresponds 3.8 mm snowfall liquid equivalent).

Personally, I feel that the paper has some major points that need to be addressed before it can be considered for publication. I was somehow surprised that the authors do not describe neither the in situ snowfall measurements device nor the remotely

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sensed observations (the radar). I would suggest

1] to complement the Mean Error with (at least one or two) complementary additional score, for instance, the standard deviation of the error and the correlation coefficient among hourly amounts. Indeed, it is interesting and relevant to have an idea of the temporal evolution of the recorded snowfall amounts during a period of time shorter than the whole event.

2] Three events out of five are indeed weak. Would it be possible to please add more events?

3] Would you please supply more information regarding the reference in situ observations? Furthermore, please write a few sentences characterizing the mountainous site at hand. In addition to snowfall amounts and water equivalent, do you have information regarding the wind intensity (and direction)? All considered, after having read the interesting the cited paper by Wolff et al. (2014) and other "grey" literature (extended abstracts) mentioned below (see more details at point #3]), you will see that additional scores like correlation and standard deviation of the error (see point #1]) are as important as the mean error.

4] I think that a very large majority (if not all) radar meteorologists agree with the authors statement "As such, use of traditional Z-S relationships . . . cannot be expected to produce accurate results . . ." However, a simple Z-S relationship can be used as benchmark. By way of example, in point #4] below, we suggest a possible Z-S to be used to derive as benchmark for the mean and standard deviation of the error as well as for correlation.

5] Please, provide a table with main characteristics of the Ka vertically pointing radar (Antenna type: is it Cassegrain parabolic? 3 or 2 m diameter? Half Power Beam Width?, presence of radome and type, minimum detectable signal in dBm and/or dBZ (including Tx and Rx Losses), Noise Figure, Bandwidth, dynamic range, Tx type: is it a 2 kW traveling wave tube as reported in the radar tutorial? , . . .

6] (see list of suggested references at the end)

Please find below more detailed suggestions/considerations regarding the 6 above-lsited major critical points

1] The mean “error” (actually it is the mean difference between the retrieval and the reference) is presented for 2 events and eventually for a larger pool of 5 events all together (including the first 2 event); in these way counterbalancing error effects are neglected. Please present not only the mean difference, but also the standard deviation of the difference, using hourly (or 3 or 4 hours, I let the authors decide; I see that one problem is also that events are weak . . .) In addition to the standard deviation of the differences, I suggest another score such as the correlation coefficient.

2] As you wrote at page 14, the data from Barrow site is limited. As you wrote, there is a clear need for a larger data set. Cannot you please add some other events with simultaneous radar, snowfall microphysical and snow gage observations?

3] Estimation of solid precipitation at the ground retrieved from remotely sensed measurements is indeed a challenge (see next point #4]), as the authors say in the paper. However, they should emphasize that even in situ point measurements at the ground show sometime huge disagreement depending on wind conditions, absence or single or double fences, . . . A suitable reference for this topic would certainly be the WMO Solid Precipitation Inter-Comparison Experiment (SPICE), which is unfortunately not yet ready (it will soon be) Meanwhile the authors may have a look at the extended abstracts of the WMO TECO and in particular at the extended abstract listed as K3A, O3(1), O3(2), O3(3), O3(4).

http://www.wmo.int/pages/prog/www/IMOP/publications/IOM-125_TECO_2016/TECO-2016_session_3.html

4] The authors emphasize several time the well-known problem (unfortunately, the problem is well known, while the solution is not known!) of non-uniqueness of snowfall retrievals from remotely sensed observations. [see also point 3]. They could use a single Z-S for the 5 events and show the corresponding scores (e.g. mean and st. dev. of the error, correlation, ...) as benchmark. For instance, they could use the one that is implemented operationally in the Finnish network (Saltikoff et al. 2015) ...

5] Figure 4 and 6: you show a dynamic of 74 dB, from -50 dBZ to +24 dBZ. Is it necessary? Is it reasonable? Is the sensitivity @ 2km range of the Ka Zenith Radar really -50 dBZ?

When you describe Fig. 5, could you please also provide a Table of correlation coefficients among the different curves (if I got it right, 3 values are enough, since LH74 Dendritic and Graupel are perfectly correlated are not they?)

6] SUGGESTED REFERENCES

K3A: Nitu et al. 2016: WMO SPICE: Intercomparison of Instruments and methods for the measurement of Solid Precipitation and Snow on the Ground, Overall results and recommendations, CIMO TECO 2016 extended abstracts, see link above at the end of #3].

O3(1): Roulet et al. 2016: Non-catchment type instruments for snowfall measurement and reporting of precipitation type: General considerations and issues encountered during the WMO CIMO SPICE experiment, and derived recommendations, CIMO TECO 2016 extended abstracts.

O3(2): Lee et al. 2016: Quantitative evaluation of weighing gauges with different wind shields through error modelling, CIMO TECO 2016 extended abstracts.

O3(3): Kochendorfer et al. 2016: Errors, Biases, and Corrections for Weighing Gauge Precipitation Measurements from the WMO Solid Precipitation Intercomparison Experiment, CIMO TECO 2016 extended abstracts.

O3(4): Smith et al. 2016: The WMO SPICE Snow-on-Ground Intercomparison: An Overview of sensor assessment and recommendations on best practices, CIMO TECO

2016 extended abstracts.

Furukawa, K., T. Nio, T. Konishi, R. Oki, T. Masaki, T. Kubota, T. Iguchi, and H. Hanado, 2015: Current status of the dual-frequency precipitation radar on the global precipitation measurement core spacecraft. Proc. SPIE, 9639, 96 390

Iguchi, T., S. Seto, R. Meneghini, N. Yoshida, J. Awaka, M. Le, V. Chandrasekar, and T. Kubota, 2015: GPM/DPR Level-2 Algorithm Theoretical Basis Document. Tech. rep., NASA/JAXA.

Matrasov, S.Y. 2007: Modeling backscatter properties of snowfall at millimiter wavelengths, J. Atmos. Sci, 64, 1727-1736

Saltikoff et al. 2015: Comparison of quantitative snowfall estimates from weather radar, rain gauges and a numerical weather prediction model, BOREAL ENV. RES., 20, 667–678

Speirs et al. 2017: A Comparison between the GPM Dual-Frequency Precipitation Radar and Ground-Based Radar Precipitation Rate Estimates in the SwissAlps and Plateau, DOI: 10.1175/JHM-D-16-0085.1

Minor points

Page 7 Line 24: . . . must then be left large ?? Line 28: . . . rates are based only the lowest few . . .

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