A review of "IMPROVED ANALYSIS OF SOLAR SIGNALS FOR DIFFERENTIAL REFLECTIVITY MONITORING" by Asko H. et al., submitted to AMT (amt-2016-52)

The paper describes a number of analysis methods for monitoring the residual differential bias between horizontal and vertical reflectivities of solar signals. Personally, I find it interesting and stimulating. I also believe that is of interest not only for the academic world, but also for the operational meteorological radar community, since dual-polarization receivers are now available in several Countries. The paper shows nice, intuitive and informative picture such as Fig. 2, 3, 6, 8 and 9. Maybe it could be made a bit shorter and more concise? I am confident that by doing so, it will attract more readers. Aiming at this, I would suggest, for instance, to shorten in particular Sec. 4.1, by deleting also Figures 4 and 5. Finally, it is probably worthwhile warning the readers about the (relatively weak) intensity of the Solar(+Noise) Signal: ~10 dB SNR with high-gain and high-sensitivity radar as long as the beam axis is hitting the center of the solar disk; but this is not the case for most of the hits acquired during operational monitoring …

Several other specific comments and suggestions to be considered by the authors are listed below.

Locarno Monti, 31.3.2016

Yours faithfully,
Marco Gabella

Fundamental, interesting, "hot" topics [from my (biased!) viewpoint]

Page 3 Eq. (1), term $-2ar$, which is two-way gaseous attenuation: is the term a in your Radar Signal Processor set to a constant values, independently of altitude? Curiosity: what value of a (in dB/km) at the sea level do you use?

Page 7, just after Eq. (16); I would re-write the sentence in a completely different way; I mean, your great idea of using solar hits during operational scan for monitoring weather radar performances, has been indeed useful for many national weather services … Hence, I would go along that tradition by changing the sentence into a recommendation. For instance: unfortunately some radar software systems do not provide both H and V reflectivities, but instead only the H reflectivity and the differential one. In this way, when deriving Z_V from the two previously-mentioned quantity, one has to assume that C_H=C_V, which is not necessarily always the case. Or something similar; with this kind of formulation, future dual-polarimetric radar users will always deal with Z_H, Z_V and Z_{DR} and not only with two of them; there are other advantages associated to this choice: for instance, Z_{DR} can be set to 0 dB by introducing an appropriate offset, while leaving Z_H and Z_V untouched and calibrated at their best according to the Probert Jones Equation (nominal values of Tx, Rx Losses and antenna Gain).

Page 8: paradoxically, you just briefly mention the important issue of Signal-to-Noise ratio in Sec. 3.4 ("Calibration of Z_{dr} during rain"), which deals with "RELATIVELY STRONG" hydrometeor signals and you do not mention it when dealing with "much WEAKER" solar signals. On the one hand, please, consider a 10 dBZ echo say at 10 km altitude (when vertically pointing): SNR is of the order of ~20 dB for the Finnish high sensitivity radars. On the other hand, even when the antenna beam axis is hitting the center of the solar disk, the Sun+Noise "signal" is just ~10 dB stronger than the Noise; even worse, during the operational scan, the beam axis rarely hits the center of the Sun, so that typical SNR is probably around ~8 dB or less … I ask to authors to mention this important aspect and warn the readers about it: maybe you could anticipate it in the introductory part? Another important point: here at MeteoSwiss, ZH and ZV values are derived after Noise subtraction, so that solar flux estimates "back-retrieved" from reflectivity values in the PPI are "intrinsically noise subtracted". Probably, the same is at FMI and it is worthwhile to inform the readers about it.

Eq. (17) and Fig. 6 (linked with Fig. 8 and page 15, see my next comment): I would plot data only from Aug. 1 to Aug 20, before the discontinuity that is only subsequently described at page 15, line7-8. Alternatively, I would anticipate here such sentence, maybe even in the caption. More important: am I missing something? By simply looking at the picture, I would conclude that Zh(5par fit)-Zv(5par fit) [CYAN curve] is BIASED of approximately +0.1 dB with respect to the median (GREEN curve), which in turns look unbiased. Why? Curiosity: what is the st. dev. of these first 20 points? (it looks of the order of 0.05 dB or less…) Adding up, sorry to say, from Fig. 6, I do not conclude that 5-param-fit is better than median. Why do you conclude that the median is not recommendable? Finally, do you have explanation for the mean having a negative BIAS with respect to the median?

Page 14 (text related to Fig. 8); line 3: dB instead of degree; line 5: yes, MeteoSwiss figures (0.05 dB for Albis, 0.06 dB for Lema and Dole, Gabella et al. 2015) were obtained using both wet and dry days; however, as stated at page 51, Sec. 2, of Gabella et al. 2015, "In the MeteoSwiss approach, only the last 5 km (60 gates) of each radar bin are averaged for Sun power retrieval"; this means that our solar echoes are at stratospheric and mesospheric altitude … Probably here it is worthwhile to emphasize that MeteoSwiss figures have been obtained: A) by neglecting atmospheric attenuation (affecting in a different way the 15 different elevations used); B) by simply using the median, instead of Zh(5par fit)-Zv(5par fit).

Fig. 8: could you please add $\mu\pm\sigma$ also for March data? (e.g. in the upper left part of each picture) Could you please add a picture for UTA radar?

Fig. 9: could you please add $\mu\pm\sigma$ also for March data? (e.g. in the upper left part of each picture) Could you please add a picture for UTA radar?

Page 15, line 12-13. This is indeed an important and interesting statement and a key point of the paper, I think; you could be less concise and try an interpretation of your results. In terms of reproducibility and repeatability, I am confident that $\mu\pm\sigma$ for March data will help the interpretation. (You could even add more months…) However, in order to make conclusions regarding this interesting point you should also provide a table with H and V Tx Losses and antenna Gain.

Conclusion, page 16, line 4-6. Here, I would list fact and figures in a more neutral and specific way: instead of "statistical accuracy … is better than 0.02 for most radars" I would write "the standard deviation of differential reflectivity for 30 daily samples (April 2015) is 0.02 dB for two radars, 0.04 and 0.05 dB for the other two radars" or something similar … [will you add also the 5th UTA radar, please?]. "This is significantly better of what reported earlier, namely 0.2 dB for the MeteoFrance Trappes radar [and then please specify 90 days; by the way, was it Zh(5par fit)-Zv(5par fit)? Maybe you can split the Trappes 90 days in three monthly subsets and tell us it st. Dev. is ~0.2 dB for all three subsets or whether it varies (I hope and I am confident it does not…)

Similarly, you can explicitly mention the DWD figure (Frech, 2013): what are the number of days and constraint used? … (5-par? Or median? atmospheric attenuation correction included?) and MeteoSwiss ones. As stated at the top of the page (no atmospheric correction, simply using the median, not the 5-par fit …) The number of days used for the 3 MeteoSwiss radar was larger than two hundreds (see Table 4, Gabella et al. 2015).

Finally, I think it is worth mentioning other examples; for instance, as reported in Gabella, M.; Huuskonen, A.; Sartori, M.; Holleman, I.; Boscacci, M. The solar slowly varying component as detected by dual-and single-polarization C-band weather radars in Europe, Electromagnetic Wave Propagation and Scattering in microwave Remote Sensing, Technische Universität Chemnitz Publikationen, submitted on September 29, 2015

during a common observation period of 100 days in 2014, the dispersion of the difference between H and V, namely the standard deviation of 100 values of 10 Log(H/V), was 0.06 dB for Albis and **0.08 dB for ANJ** (see Table 3 in the above-mentioned paper). While using the **median** instead of **5-par fit** for

ANJ, it was **0.12 dB** ; also this other example shows that using 5-par-fit gives better results than simply the median.

Other Specific Comments

page 1, line 20: are based on the solar signals; line 25: you cite examples with French and German radar(s); I hope you do not mind to cite here the Swiss network, too (although, our analysis, is simply based on the median, I know, as stated in several other points …)

page 2 line 12: … which is based on the difference (on the Log-transformed, decibel scale) of the daily median H and V reflectivities of the strongest 21 hits in each days [that means the 11th value …]

page 3, I would remove Fig. 1; part of the corresponding space could be used to show the symmetric form of the MeteoFrance Trappes radar in color (while in Holleman et al. 2010b, Fig. 2, page 883, it is in black and white and consequently, more difficult to be read and interpreted); furthermore, the reader would have these nice and informative pictures for two radars in the same paper. (Maybe you have it of other radars? Detailed co-polar and cross-polar info regarding radar antennas are rare in peer reviewed literature and one has often to look for info in technical reports of PhD dissertations, e.g. Reimann (2013)

page 4,5,6, from Eq.(2) to (15): could/would the authors reduce the number of Equations, for instance by citing (and not writing again) the ones that have already been published?

Page 5, Table 1: why do you list radar KUO (never used) and you do not list radar UTA? (which is used in Fig.3); please, substitute KUO with UTA.

Page, Fig. 3: To what day does it refer? Could you provide examples for ANJ and LUO, too?

Page 8, line 5-6, "… is in addition affected by the differences in the Tx power between the channels". Are you referring to the magic-Tee ? If so, I find difficult to imagine the power in not split properly; I would write " … could be in addiction affected by …

Page 8, line 8-9, "… because …to their normal operation"; are you referring to the T/R Limiter?

Page 9-10: as stated, I would make it considerably shorten and delete Fig. 4 and 5.

Page 12, figure 7, two bottom pictures: I would set y-axis with the same limit for both ELEV and AZIMUTH, e.g. from 0.9° to 1.3° in order to better emphasize different (apparent) widths because of azimuth scanning (PPI instead of RHI)

By the way, Page 13, text commenting figure 7: do not you think that the effective beamwidth in azimuth is larger as a consequence of the operational azimuthal scan? (I mean, PPI). I think it is worth commenting this effect and also citing Zrnic and Doviak (1976).

Suggested references:

Zrnic, D.S. and R.J. Doviak, 1976: Effective antenna pattern of scanning radars, *IEEE Trans. Aer. Electronic Systems*, **12**, 551-555

Reimann J., 2013: On Fast, Polarimetric Non-Reciprocal Calibration and Multi-polarization Measurements on Weather Radars, *PhD dissertation*, University of Chemintz, ISSN 1434-8454, DLR Forschungsbericht 2013-36, 161 pages