Atmos. Meas. Tech. Discuss., 5, C360–C365, 2012
www.atmos-meas-tech-discuss.net/5/C360/2012/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Observations of tropical rain with a polarimetric X-band radar: first results from the CHUVA campaign" *by* M. Schneebeli et al.

Anonymous Referee #1

Received and published: 26 March 2012

General Comments

The subject of this paper is actually the validation of a method for polarimetric attenuation correction of radar reflectivities at X-band and not "first results from the CHUVA campaign" as it is stated in the title. In the referenced very recent paper (Schneebeli and Berne 2012) of the method development that method was tested with radar and disdrometer data collected during 2010 in Alps. The current paper presents a re-evaluation of this method using a different data set collect in Brazil. Even though it is mentioned in the abstract (and the introduction) that a Ka-band radar and three disdrometers were available during the new experimental campaign and they can be used to thoroughly evaluate the method only one disdrometer is actually used. Thus, the current work looks like a repetition of the first paper with a different dataset. The paper will have a good scientific significance and merit publication if the results from the two different geographical areas and types of rain are compared for possible differences (for example, in the coefficients of the model-method in Table 2). Note however that the main aim of polarization diversity in modern radars is among others to remove the dependency of the formulation of rainfall estimate and the relevant methods (like the attenuation correction) on geographical area and type of rain. T-matrix simulations should lead to the same model coefficients if the range of input parameters is the same. I propose that the authors should make major revisions to the paper taking into account the above comments as well as the specific comments below, where the most important issue is the validity of Eqs. (13)-(14) which estimate the specific differential propagation phase shift from reflectivity.

Specific Comments

Page 4, line 18: The disdrometer is probably an OTT Parsivel, which should be mentioned.

Page 7, lines 10-17: Which is the range of the median volume diameter, the intercept parameter, and the shape parameter of the droplet size distribution (DSD) as well as the assumptions for the orientation (canting angle) of the droplets used in the T-matrix simulations? Which axis ratio model from the three mentioned was finally used and why? As the authors indicate disdrometer measurements from the experiment were used for the DSD instead of assuming a theoretical DSD (typically a normalized Gamma distribution). If the range of the DSD parameters is limited then the simulations and the coefficients of the method and models Eqs. (7)-(16) are specific only to the rain events examined. This is not the desirable behavior for polarimetric algorithms.

Page 10, Eqs. (9)-(16): The basis of the attenuation correction method is the models used like a reflectivity attenuation proportional to differential propagation phase shift Eqs. (11)-(12), a linear model between reflectivity and the logarithm of the specific differential propagation phase shift Eqs. (13)-(14), and the non-linear power model between differential reflectivity and backscatter phase Eq. (15). First, it should be noted that there are some obvious errors in Eqs. (9)-(16). Are these equations the same with the ones in the original paper (Schneebeli and Berne 2012)? In Eq. (10) the last term should be δhv(i+1) instead of δhv(i), else combining Eqs. (9) and (10) we get dΨdp/2dr=dΦdp/2dr=Kdp, which is wrong if δhv is non zero and has a gradient. Next, in Eqs. (11) and (12) μh,vΦdp should be replaced by -μh,vΦdp, else measured (i.e., attenuated) reflectivities are larger than the real reflectivities (μh,v is positive). Also, the right hand side of Eqs. (13)-(14) should be Zh,v-λh,v10*log10(Kdp), else at zero 10*log10(Kdp) the Zh,v becomes –κh,v which is a large negative value opposite to the results of Fig. 5.

Kalman filter on the other hand is just a method to filter (i.e., reduce) the measurement noise. Equations (9)-(16) should be given in the usual formulation of Kalman filter (i.e., state equation and measurement equation) so that the reader better understands how the the filter works. Kalman filters require exact models and a good knowledge of noise covariances else their performance can be seriously degraded or become unstable. There are various methods to estimate noise covariances from data. However, none of the models in Eqs. (11)-(15) is an exact (or accurate enough) model of reality but crude approximations with different average coefficients for different rain types or even rain events. Especially Eqs. (13)-(14) imply that Kdp is not needed because it is a function of reflectivity, which also turns the relation rain-Kdp in Eq. (7) to simply another relation rain-reflectivity. Furthermore, using the logarithm of Kdp instead of the linear Kdp in Eqs. (9)-(16) in addition to the non-linear model Eq. (15) makes the Kalman filter more non-linear (extended Kalman filter). The usual approach is to linearize the system by using partial derivatives (Jacobian) which increases the approximation error of the real system in addition to the approximation error made by the models used. Kalman filter will treat any difference of the measurements from the selected models probably as noise forcing measurements to models, which is not a correct method.

Page 11, lines 12-20, Fig. 3: This approach to estimate the radar calibration bias can be applied to any self-consistent scheme which depends on radar calibration. However, the accuracy of the estimation of the calibration bias depends on the validity of the models used in the scheme like Eqs. (13)-(14) in this paper. The results of Fig. 3a show that the model of Eqs. (13)-(14) does not fit well the observed Φdp. For ranges larger than 20 km, which is shown, the differences are probably bigger. Furthermore, the minimum in Fig. 3b is not well defined because the changes around the minimum are of the order of the Φdp (or Ψdp) measurement noise. I don't expect such a method to give better accuracy than a couple of dB. If a different minimization criterion (for example the mean square error or the maximum absolute error) instead of the mean error is used what is the change of the estimated reflectivity bias?

Page 11, lines 21-29, Fig. 4: In Fig. 4b the abbreviation HB in the figure refers to Hubbert and Bringi (1995) and should be defined in the text or the legend of the figure. The smoothing of HB curve seems quite higher than the filter length used in HB (about 1.5 km). The EKF estimated Kdp shows high variations and values close to zero (for example at about 17 km) while Φdp in Fig. 4d shows a constant increase in these ranges (11-20 km). Observing reflectivity variations in Fig. 4a it seems that this behavior of estimated Kdp is due to the crude model Eqs. (13)-(14) which estimates Kdp from reflectivity. The estimation of Φdp and δhv also clearly fails in ranges 5 to 10 km, where there is 5o bump in Ψdp due to δhv which remains also in Φdp. The negative gradient Φdp at the right side of this bump is inconsistent with the positive estimated Kdp (practically by Zh) in that area.

Page 12, Eq. (18): In the original ZPHI method the proportionality factor α includes the intercept parameter of the DSD, which is assumed to be constant in each rain cell along the range profile. However, in different cells it may be different and ZPHI approach can be applied to separate rain cells with significant different results. Thus, the ZPHI method used in this paper is a simplification of the original full method assuming one rain cell in the range profile. This fact is implied by the authors by mentioning that i1 is the last gate of the range profile but it has to be mentioned explicitly in the manuscript.

AMTD

5, C360–C365, 2012

Furthermore, because the proportionality factor α includes the intercept parameter of the DSD it may differ among rain events even if there is only one rain cell. In this case of one rain cell only the attenuation correction of Zdr is degraded (while Zh correction is not affected) if α is assumed to be constant in different rain events like it is done in this paper.

Page 13, lines 13-15: In Gorgucci et al. (1999) it is stated that is important to average Zdr from vertical pointing antenna in full 360o cycles to avoid the effect of azimuth dependency due to ground clutter variability, but they do not say that it makes difficult the application of the method as the authors imply. The significant problems that the authors faced with this method are probably due to their radome problems and not to the method itself.

Page 14, lines 5-6, Fig. 6: Differential reflectivity is usually very noisy at that range of reflectivity values (0 to 10 dBZ). A range of values like 15 to 25 dBZ would be preferable with a theoretical reflectivity estimated from a Zh-Zdr average relation and for small attenuation correction (i.e., small Ψdp) instead of a small sum of measured reflectivities along the ray profile.

Page 15, lines 8-9, Fig. 5: Fig. 5a that shows a “close” relation between reflectivity and Kdp is misleading because the logarithm of Kdp is used, which reduces the apparent scatter of data points. If the linear Kdp was used the scatter would be a lot larger. For Rayleigh scattering and qualitatively for non-Rayleigh scattering in rain the ratio Kdp/Zh depends among others (like DSD shape parameter and droplets axis ratio) mainly on the median volume diameter of the droplets. Thus, there are cases when Kdp may decrease while Zh is steady or increasing. Kdp and Zh will look close related (even though they are not interdependent in general) for data with narrow range of median volume diameter values. In this respect, it is important to show the distribution of DSD parameters for the specific dataset.

Page 15, lines 9-16: The EKF method to estimate the Zh calibration bias is actually a filtered version of the "Kdp method" because both are based on Eqs. (13)-(14). Thus, they are not two different methods to compare and the only expected difference between them is the accuracy of the bias estimation due to noise in the "Kdp method".

Pages 17-18: In addition to reflectivity offset due to radome wetting in heavy rain differential reflectivity is also affected due to the formation of streaks of water on the radome. Have the authors examined this effect on the behavior of the estimated differential reflectivity bias in heavy rain?

Page 19, lines 21-22: It is wrong that ZPHI would be degraded by partial blockage. The attenuation correction of Zh with ZPHI does not depend on calibration bias. The partial beam blockage that starts from small ranges (like in Fig. 10) and the attenuation by radome wetting appear like a change in calibration bias. The attenuation correction of Zdr with ZPHI will also be independent of the calibration bias in the case that the proportionality factor α in Eq. (18) is assumed (erroneously) to be a constant as the authors do in their simplified version of ZPHI.

Page 19, lines 26-28: Add the calibration bias (including the part due to radome wetting) to Zh in Fig. 11a so that it does not look like a very large attenuation correction due to rain even in areas with low Zh when it is compared with Fig. 11b.

Page 21, lines 2-4: As mentioned in a previous comment ZPHI correction is independent of radome attenuation and, thus, its improved performance compared to EKF under these conditions is expected.

Page 21, lines 24-25, Fig. 14: In Fig. 14 the Kdp estimated with T-matrix by Parsivel ranges from 10 dB down to -25 dB (and Parsivel ZÂňh goes down to 10 dBZ in Fig. 13), while the same Parsivel Kdp in Fig. 5a ranges from 15 dB down to only -10 dB. What will be the Zh-Kdp relation in Fig. 5a if the missing data are added?

Page 23, line 17: "Table 1" should be corrected to "Table A1".

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 1717, 2012.