

A mobile X-POL weather radar for hydrometeorological applications in the metropolitan area of Sao Paulo, Brazil

Comments

General comments

While this paper presents some interesting possibilities for the MXPOL radar, it appears to be incomplete and is not totally consistent with the title. I expected to see some results on rain rate, rainfall accumulation, vertically integrated liquid water...

From my perspective, the most interesting part is the detection of the sea breeze front with MXPOL and its relation with thunderstorm initiation (in section 4). In section 5, a microphysical study of a squall line is attempted but it is shown that severe attenuation affects MXPOL data with little added relative to earlier papers about X-band observations. Nevertheless, consistent with the aim of the paper, it would be good to have further comparisons with SPWR data. The paper would be much stronger also if the authors can more completely integrate the kinematics with the microphysics by means of 3-D retrieved fields because it is difficult to interpret microphysics (section 5) without knowing something about the magnitude of vertical motion within the convective cells. This represents the major weaknesses of the manuscript.

Major comments:

p. 180, l. 21: Since the MXPOL corrected reflectivity are available (p. 181, l. 13) why is the the focus of the present study on raw data? Furthermore, what does “corrected reflectivity” mean?

p.181, from l. 25 to l. 28: How are cloud tops, rainfall accumulation and vertically integrated liquid water determined? What is the difference between “cross-sections” and “specified horizontal and vertical products”? Is “storm motion forecast” different from “storm tracking and forecasting”?

p. 183, l. 22: The spectral width (Fig. 3d) does not allow to precisely discriminate the center and borders of MASP region! High (low) values are also observed in the center (borders) of MASP region. On the other hand, comparisons should be done at the same altitude which is not the case in Fig 3d relative to a PPI.

p. 183, from l. 27 to l. 29: Figs 3b to 3e are relative to observation in clear air and show that only ground echoes are detected (no weather echoe). Consequently, it is not clear how it is deduced that “low elevation polarimetric measurements allows to distinguish weather from ground echoes under clear air conditions over MASP”. Please explain. Furthermore, it would be very helpful to compare observations in clear air and cloudy weather to focus on ground echoes.

p. 184, l.16: Clouds tops correspond to a 5 dBZ reflectivity value and yet echo tops are estimated at 18 dBZ with SPWR. Please, explain this difference.

p.184, l. 20: How are the hydrometeors identified and discriminated? How to be sure that reflectivity values (~ 20 dBZ) above the melting layer are not significantly attenuated?

p.184, l. 24: Fig. 5b is relative to radial velocity not to terminal velocity! With vertically pointing cloud radars, the Doppler measurement is the sum of the reflectivity-weighted terminal fall speed and the vertical air velocity. Consequently, without information on vertical air motion, it is impossible to discuss about the terminal velocity in the present study.

p. 185, l. 1 : Since the present study is based on raw data, the biased value of Z_{DR} is probably due to strong attenuation (by rain and cloud). Could you please justify that it does not affect the relevance of the study ?

p. 185, l.10: Taking into account my previous comments, it is not clear how it can be deduced “the good quality of MXPOL polarimetric measurements”. I suggest to compare Fig. 5a with a new one relative to SPWR data.

p. 186, l. 16: The analyse of ZDR field about radar targets orientation is correct assuming Rayleigh scattering. But for insects the Rayleigh scatter approximation is inaccurate !

p. 187, from l. 10 to l. 12 : high reflectivity values and strong radial velocities are co-located near the center of Fig. 9a but not in the left corner of the figure. Please, explain. On the other hand, turbulence $SW \sim 3 \text{ ms}^{-1}$, lower $\Phi_{DP} < 100^\circ$ and $K_{DP} < 3^\circ \text{ km}^{-1}$ are not specific to the area of high reflectivities near the center of fig. 9a ! It is also to be noted that higher $\Phi_{DP} > 100^\circ$ and $K_{DP} > 3^\circ \text{ km}^{-1}$ can be co-located with high reflectivities...

p. 187, l.12 to 14 : The correlation coefficient is between 0.95 and 0.99 everywhere except at the edge of radar echoes. Consequently, this parameter can not indicate « the good quality of the polarimetric measurements » !

p.187, l. 27 : Fig 10b is relative to radial velocity of the radar target, not to vertical component of the wind. Furthermore, it is obvious that the lower the radar elevation (the lower altitude in Fig. 10b), the weaker the vertical component of the radial velocity. Consequently, can you explain how you identify « the strongest updrafts » ?

p. 188, l. 6 : What do you mean with « radial convergence » ? Is not radial convergence highest at 5 km altitude and at 28 km range ?

p. 188, l. 8 : Because of low variation of the spectral width inside the main part of the squall line, Fig. 10c does not clearly emphasize that the turbulence is higher near the main updraft.

p. 188, l. 12 : The « core of high negative value » may also result from strong attenuation as indicated by circle on Fig. 10a. Please discuss.

p. 188, l. 15 : Z_{DR} is zero in a large part of the squall line. Does it suggest the presence of hail everywhere... ?

p. 188, l. 18 to l. 22 : It is shown that severe attenuation affects MXPOL observations. Only K_{DP} seems to be weakly affected by attenuation. But what is new ? This is known since 1995 (Aydin et al., 1995).

Ref. : Aydin, K., V. N. Bringi, and L. Liu, 1995: Rain-rate estimation in the presence of hail using S-band specific differential phase and other radar parameters. *J. Appl. Meteor.*, **34**, 404–410.

p. 189, l. 15: Where is it shown in the present study that “the dual polarization measurements allow better rainfall estimation”? I suggest to delete this sentence.

p. 189, l. 21 and 22: Where are “boundary layer circulation” and “vorticity” discussed in the text? Furthermore, MXPOL does not directly detect convergence and divergence. It only measures radial velocity!

p. 189, l. 24: Could you, please, clarify what is “precursor to lightning”?

p. 189, l. 24 and 25: In order to better validate MXPOL reflectivity measurements it would be very helpful to rely on Fig. 5a and a similar one for SPWR.

.
Minor comments:

p.178, l. 17: The sentence “Heavy pollution...” is unnecessary. It is unrelated to the present study.

p. 178, l. 25: « Ferreira et al., **2010** » should be « Ferreira et al., **2011** »

p. 179, l. 1: “Fig. 1” should be “Fig. 3a”

p. 179, l. 6: “and” should be “an”

p. 179, l. 11: “establish” should be “established”

p. 179, l. 13: I suggest to add “In particular, MXPOL was designed and built to monitor and to nowcasting weather systems over MASP and the Coast region of Sao Paulo State”. Consequently, l. 24 “It was designed...State” can be deleted.

p. 179, l. 25: the sentence “Both regions...” can be deleted because this information is given in the first part of the introduction.

p. 179, l. 27: the sentence “The Hydrometeorological...” can be deleted because it is redundant with l. 7 to l. 13 (p. 179).

p. 179, l. 29: complete the sentence “... in 2007 **and 2008**” to be in agreement with Figs 6, 7 and 8.

p. 180, l. 17: “it is well know...” should be “it is well known that ...”.
Attenuation is not caused only by rainfall! Also, I propose: “It is well known that X-band radars suffer from attenuation caused not only by intervening rainfall (Berne et al., 2006) but also by cloud, i.e., the nonprecipitating component of condensed atmospheric water (Pujol et al., 2007) ».

ref.: Pujol O., J.F. Georgis, L. Féral, and H. Sauvageot, 2007: Degradation of radar reflectivity by cloud attenuation at microwave frequency. *J. Atmos. Oceanic Tech.*, **24**, 640-657 .

p. 180, l. 21: I suggest to add a sentence before “The focus...”: “On the other hand, few studies were proposed to estimate cloud attenuation and, consequently, to correct X-band radar observations (Georgis et al., 2006) »

ref. : J.F. Georgis, O. Pujol, and H. Sauvageot, 2006: A dual wavelength polarimetric method to identify cloud component in warm precipitating systems. *4th European Conference on Radar in Meteorology and Hydrology, Barcelona, Spain*, 276-278.

p.180, l.26: “are described” has to be deleted.

p.181, l.14: “W” should be “SW”

p. 183, l. 11: “Figure 3b to h” should be “Figures 3b to d”

p. 183, l. 12: How are “surface winds” retrieved?

p. 184, l. 3: Please precise the definition of “echo top” in terms of reflectivity value.

p.184, l. 6: It should be mentioned that 18 dBZ echo tops are below 3.5 km altitude in the close vicinity of MXPOL.

p. 184, l. 14: - What is “ Z_h ”? Is it Z or Z_T defined p. 181, l. 13?

- Z_{DR} and SW (not W!) have to be permuted

Fig. 5: - images (c) and (d) have to be permuted and the same goes for (e) and (f).

- in Fig. (b) “VT” should be “ V_R ”.

p. 184, l. 17: According to Fig. 5 (a), the melting layer is 280 m deep between **3180 m and 2900 m (not between 3940 m and 3660 m)**. Consequently, it would be very helpful to represent in Fig. 5 all the measured variables as a function of MSL altitude instead of “Height”.

p.184, l. 21: Change “ Z_h is 34 dBZ” by “ Z_h reaches a 34 dBZ maximum value”.

p. 184, l.29: Please, explain the “near-field effect”.

p. 185, l. 6: “differential phase” should be “specific differential phase”. Please, specify the interest of Fig. 5f.

p. 185, l. 13 (and after) : Is it 19:48 UTC or 19:46 UTC as indicated in the caption of fig. 6? Please, add the unity (dBZ) in the caption.

p. 185, l. 19: please, indicate the “very fine line of reflectivity” in Fig. 6.

p. 185, l. 22: the sentence is truncated: “the convective cells towards the..?”

p. 186, l. 13: It would be interesting to see the location of the severe thunderstorms on a new figure (as Fig. 6b).

p. 187, l. 25: According to Fig. 10a, Z attenuation should be the greatest in the anvil cloud behind the core of high reflectivity and not in the circle! On the other hand, it is well known that the specific differential phase is not affected by propagation attenuation. This is why it is an important parameter for meteorological applications. Then, it is not astonishing that « collocated K_{DP} measurements are not as much affected by large drops ».

Figures 9 and 10: please, add the unity for each parameter.