Comments to "RANGE AS A FUNCTION OF DUAL-POLARIZED QUANTITATIVE PRECIPITATION ESTIMATION"

Edouard Goudenhoofdt

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1 General comments

I would like to thank the authors for considering my comments and their responses. Unfortunately most of the major comments have not been addressed in a satisfactory manner. While clarifications are appreciated, some of the responses are missing, irrelevant or too short. There are no clear references to the revised manuscript, which does not contain the major changes expected. Additions to the text are made but limited to a few sentences which are often not well integrated. The new figure comparing the performance of all algorithms reveals the potential of the paper which is still not fully exploited. Below is a summary of the most important remaining issues.

2 Major comments

2.1 Does the paper address relevant scientific questions within the scope of HESS?

The paper could provide a long-term verification of dual-pol QPE algorithms which is relevant for hydrology. The authors stress that they focus on the range effect but this is in contradiction with the extended list of objectives in the introduction and the limited amount of results related to range in the conclusions.

2.2 Does the paper present novel concepts, ideas, tools, or data?

The number of data is limited. Why only one year? Why only 46 days of precipitation are available when the normal is around 100 days?

2.3 Are substantial conclusions reached?

The conclusions are short and do not summarize clearly the main findings (i.e. the algorithm's relative performance in function of the range). A proper discussion on the validity and possible cause of the different results is missing.

2.4 Are the scientific methods and assumptions valid and clearly outlined?

The information on the data and their quality is still limited while it seems some observation errors affect the results. Which type of quality control is effectively performed by WDSS-II on the radar data? Why not using the one-hour precipitation product of NOAA as reference? Why using the Mesonet network when the higher resolution CoCoRaHS is considered as better by the authors? The data selection criteria and choice of statistics are not sufficiently discussed.

2.5 Are the results sufficient to support the interpretations and conclusions?

In Figure 2, the results vary a lot between the algorithm's and the radars making interpretations difficult. I am surprised by the bad performance of KDP (did you check the cause visually?). The tentative explanations of radar issues for specific gauges (e.g. bright band effect) are not robust. In Figure 3-8, only the overall best and worst algorithm's are shown, which is too limited (I would present the best of each type). It is often unclear for which algorithm an interpretation is valid. There are inconsistencies in the results : the NSE is sometimes different between Figure 2 and the other figures ; the overall results are not always equal to the sum of the cool and warm seasons results.

2.6 Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

The results of similar studies (including from the authors) are not properly reviewed. Is there a connection with your recently submitted article on X-Band?

2.7 Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

The description of the statistical analyses needs to be much more clear and precise (proper definition and interpretation, thresholds used for zeros, selection of hit only data).

2.8 Does the title clearly reflect the contents of the paper?

The new title sounds a bit odd to me.

2.9 Does the abstract provide a concise and complete summary?

The abstract has not been improved as suggested and is not consistent with the conclusions.

2.10 Is the overall presentation well structured and clear?

The comments have not been taken into account. There is still part of the methodology in the "results" section.

2.11 Is the language fluent and precise?

No significant efforts have been made to improve the text structure, terminology and style. There are annoying editing errors at this stage (e.g. a repeated sentence on line 212).

2.12 Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Some definitions are still incorrect or imprecise.

2.13 Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

The results section is still not clear nor concise. There are too much points in Figure 2. There are too much plots in the figures. I would show only NMB, NME, PoFD, PoD. Paragraphs over the different radars could be combined. What is exactly on figures 2-8 : best at each point (your response) or only R(Z,ZDR) (figure caption)?

2.14 Are the number and quality of references appropriate?

The number and quality of the references are acceptable but they are often cited for anecdotal reasons (e.g. Figueras et al. on line 381). They are best used for discussion in the introduction and conclusions sections.