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Interactive comment

Interactive comment on "Multi-radar performance in the Midwestern United States at large ranges" *by* Micheal J. Simpson and Neil I. Fox

Anonymous Referee #2

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Title: Multi radar performance in the Midwestern United States at large ranges Authors: M.J. Simpson and N.I. Fox

The current work presents the performance of 15 difference dual-polarimetric radar algorithms for 46 days of weather radar observations obtained by three radars in Missouri. Even though different algorithms perform differently for different events and different periods of the year, no clear conclusion can be drawn on which algorithm performs best. Even though some do seem to perform better than others. The general idea behind this study is very interesting and I enjoyed reading the presented results. However, I have the feeling this study is not finished yet and as such not ready for publication. The limited number of gauges as well as limited number of days make it difficult to draw any firm conclusions. Therefore, I would suggest that the authors would

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do a number of additional analyses and incorporate a number of suggested changes to the manuscript. Once these are done, this manuscript will become very interesting. A complete description of my comments is given below.

Overall comments

Currently, only 46 days of precipitation are analyzed for a single year. I would suggest that the authors would extend this analysis covering multiple years to improve the robustness of the obtained statistics.

The current manuscript only makes use of a very limited number of rain gauges (15), which makes it difficult draw conclusions in general (especially for 46 days only). Many of the results presented in this work (Figures 2 – 8) present maxima and minima on solid lines, which gives the impression as if the radar performance is specific at a given range. Instead these maxima and minima are obtained for a given rain gauge only. I would suggest that the authors present the individual gauges in single points in these figures instead of solid lines. Next, in the presentation of the results, please be careful with generalizing certain phenomena that only hold for a single gauge.

No clear distinction was made between convective and stratiform precipitation. Even though the manuscript does indicate that convective precipitation is identifies (although using a very poor algorithm). It would be interesting if the authors could present there results for precipitation type especially in case the size of the dataset is extended (see previous point).

The authors currently present the results for each individual radar even though at the national scale these observations are merged into a single product. Therefore, besides presenting results as a function of distance from the radar, it would be valuable if the authors would generated a combined radar grid on which the performance of each algorithm would be calculated.

This manuscript misses a clear discussion about the impact of the results presented here as well as the limitation of the applied methods. I would suggest that the authors would add this.

Specific comments:

Lines 46-48: The paper discusses the operational dual-polarimetric NEXRAD product. As the such, the X-band radars discusses in these lines fall outside the scope of the manuscript (as the operational S-band is not affected by attenuation) and should be removed as they do not add anything.

Lines 56-58: These lines are unclear. I would suggest that these are rewritten.

Lines 59-64: I would suggest to add the following references here: Kirstetter P.E., et al., 2013, JAMC, 52, 1645-1663, doi: 10.1175/JAMC-D-12-0228.1 and Hazenberg P., et al. 2013, JGR, 118, 10243-10261, 10.1002/jgrd.50726.

Lines 65-72: Quite a number of papers have looked during the last decade to the long-term performance of the operational weather radar. Though noted, these other papers might have not looked at a similar number of algorithms, which is something that makes this manuscript very interesting. I would therefore suggest that the authors rewrite this section given a bit more credit to work performed by others.

Lines 79-83: How does this work go beyond work presented by these authors in previous work? Even though 46 days is a nice amount, it only covers 1.5 month for a single year. Therefore, it is difficult to draw any firm overall conclusions especially when it comes to the impact of seasonality.

Lines 112-114: It is stated that 46 days of radar data were analyzed. Of the 46 days, how many hours did actually contain rainfall? As I suspect that it was not raining all day. If a considerable number of hours contained zero rainfall, what is the effect of this on the presented statistics?

Lines 118-120: This is a very classical approach which has been shown to be too simplistic. I would suggest that the authors make use of the method presented by Steiner M., et al., 1995, JAM, 34, 1978-2007, doi: 10.1175/1520-0450(1995)034<1978:CCOTDS>2.0.CO;2

Lines 141-143: How is the conversion from polar to Cartisian performed? Using the nearest point, or a weighted integration?

Lines 143-145: While integrating the 5-minute precipitation product to hourly intervals, was there any spatial interpolations between individual images performed? Especially, for summertime convective summer events, not accounting for the propagation speed of a precipitation cell between individual 5-minute scan can have a serious effect on the hourly accumulations (see e.g. Fabry et al., 1994, JoH, 161, 415-428, doi: 10.1016/0022-1694(94)90138-4). In case this was not taken into account, how would this potentially affect the obtained results presented here?

Lines 145-147: Why would it not be possible to use the nearest polar pixel for comparison with the rain gauge?

Line 75 and lines 154-155 state that a total of 55 algorithms were applied while in lines 156-160, a total of 15 methods are briefly presented. Please clarify this difference.

Lines 217-222: The authors suggest that the overestimation by the radar at around 150 km might be due to the bright band. First it is not clear what is meant with a “second bright-band”. Instead of suggesting the possibility of bright-band contamination, I would suggest that the authors analyze local sounding/weather model observations for the different days analyzed to obtain a proper estimate of the location of the zero-degree isotherm and at which distance the radar beam interaction with the layer just below this. This will help to clarify whether the maximum was indeed related to bright band. Next, I would suggest also to carefully look at the convective/non-convective data, as the former should not be affected by the bright-band.

Lines 322-326: What about the fact that winter precipitation is generally more frontal and widespread with spatially variabilities much smaller. As compared to summer pre-

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cipitation with convection triggering small-scale variations. As such, it is easier for the radar to see the proper evolution of precipitation in winter as compared to the summer, although correlation coefficient does not indicate the performance to estimate the exact amount.

Lines 337-349: Given the limited number of gauges used in this study, I would suggest that the authors would be careful to make any subdivisions with respect to distance.

Figure 9: Which radar product is being used here?

Figure 10: It makes statistically to identify the impact of a given radar algorithm while looking at an individual gauge. Especially in case you only have 15 gauges.

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