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Interactive comment on "Exploring the potential of utilizing high resolution X-band radar for urban rainfall estimation" *by* Wen-Yu Yang et al.

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Response to Reviewer 1

We deeply appreciate the reviewer for his/her very insightful and constructive comments.

We would note that we decided to change our topic to "Utilizing X-band radar monitoring fast-moving rainfall events" considering the nature of the revision. Such change is motivated by following reasons: The urban hydrologic simulations are very sensitive to the spatiotemporal variability of rainfall (Schilling, 1991; Emmanuel,et al., 2012) and thus require rainfall inputs of high spatiotemporal resolution. Although X-band radars can provide rainfall products of high spatial resolution (Chen and Chandrasekar, 2015), they still lack the ability to provide products of high temporal resolution. The radar-

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Discussion paper

rainfall accumulations generated from periodic sampling often poorly represent the actual rain fields due to the coarse temporal resolution of the radar rainfall product. This error will be amplified for fast moving storms and fine spatial resolution data (Seo and Krajewski, 2015). In the revised manuscript, we monitor the fast-moving rainfall events with downscaled X-band radar product using the extrapolation technique. First, we quantitatively evaluate the "common error" correction approach to assess the quality of the coarse temporal resolution product. Then, we investigate the impacts of advection correction on the radar QPE. We also examine impacts of the physical factors on the correction accuracy.

The connection between the previous and revised manuscripts are: Same observations from the Beijing X-band radar system, including an X-band radar and a disdrometer; Same QPE algorithm to retrieve rainfall from radar measurement.

However, due to the unexpected amount of work in the revision, we are unable to finish the revision in time even though one extension had been kindly granted by the editor. As such, we first address the specific concerns of the reviewer as best as we can; meanwhile we are working on the revision with more thorough analysis.

Below we detail how we addressed the specific concerns of the reviewer: Major comments: Radar calibration: calibration using a nearby disdrometer is actually a reasonable option, especially for longer wavelength radars (in the cited article, Lee and Zawadski used S-band data). Indeed, at shorter wavelength such as X-band, in addition to path attenuation, the attenuation caused by the wet radome can induce serious underestimation of the reflectivity factor, up to several dB, e.g. Schneebeli and Berne (2012), Gorgucci et al. (2013), Frasier et al. (2013). Considering that the disdrometer in this study is very close to the radar, most of the measurements analyzed are likely coming from situations with rain over the radar also. This may explain the reported underestimation for higher reflectivity (>35 dBZ). Only qualitative results are reported in the manuscript, with figure 3 representing observations from a single event during a one year period (by the way, I would exchange the x and y axes, since the disdrometer is the reference here). What about the other events and an overall quantitative evaluation of the calibration? Response: We thank the reviewer for the suggestion. The reviewer's analysis of underestimation is very insightful and we will add a detailed analysis in the revised manuscript. Also, we agree with reviewer that a single event comparison is not comprehensive. In the revised manuscript, four fast-moving events are selected to evaluate the performance of X-band radar QPE. Therefore, the evaluation will be done for the four selected fast-moving events to make the calibration more convincing.

Beam integration: what is illustrated in this section appears to be a simple elevation selection, depending on the visibility. There is no mention of correction for partial beam blocking. If this is the case I think it may be simply called "beam selection", and should not be considered a correction procedure. Response: Corrected as suggested.

Local Z-R relations: the authors cite Steiner et al. (1995) work to differentiate rainfall type (convective/stratiform) based on a reflectivity threshold of 39 dBZ. However, the cited paper presents a more complex procedure based on the spatial structure of the reflectivity (intensity, peakedness). Steiner et al. report an overlap region between 20-35 dBZ, highlighting that "a simple reflectivity threshold method to separate convective from stratiform precipitation is insufficient". So, where does the 39 dBZ value comes from? Why do you need a different convective/stratiform partition method for the disdrometer data? Would it be possible to use the radar-based LWC method to select the corresponding disdrometer data for the separate Z-R retrievals? This may be more consistent, since in the end you need the Z-R relations for application to the radar observations.. Response: We thank the reviewer for pointing out our incorrect citation of Steiner et al., (1995). The 39 dBZ threshold was used just as a first order estimation. In the previous manuscript, we used this simple threshold method due to its computational efficiency compared with the radar-based LWC method. In the revised manuscript, as we now focus on only four rainfall events, it is feasible to use the radar-based LWC method.

Wind drift: the authors seem to confuse the motion vectors (advection of reflectivity patterns) and the wind vectors. At line 330 it is stated that “the advection velocity of a rainy pixel (equal to the background wind velocity)”. This is not true: the advection velocity is not the same as the wind velocity. Although a correlation may exist between storm advection and mid-tropospheric winds (e.g. Johns and Doswell, 1992; Kyznarova and Novak, 2005), the lower layers’ winds (0-2 km) may actually dramatically differ from the advection motion. In addition, the low-level shear cannot be simply attributed to a velocity change (with constant direction), as reported in section 3.2. This is an over-simplification, not supported by neither theoretical arguments nor experimental evidence. It is also not clear why this “wind drift” correction is only shown for a single event, while the other corrections are applied to a bigger dataset. I’d rather suggest to carefully check the time synchronization between the radar and the gauge observations. In particular, which time was considered for the radar observations, since these are coming from different elevations (different scan time) depending on the azimuth sectors? Response: We thank the reviewer for pointing out our confusion between the motion vectors and the wind vectors. In fact we intended to investigate the temporal sampling bias caused by advection rather than the wind drift effects (Thorndahl et al., 2017). It has been acknowledged that radar-rainfall accumulations generated by weather radars from periodic sampling often incorrectly represent actual rain fields. Coarse temporal resolution radar product suffers spatially discontinuous patterns that were caused by the intermittent radar scanning frequency. This error will be amplified for fast moving storms and fine spatial resolution data (Seo and Krajewski, 2015). Therefore, in the revised manuscript, we use an extrapolation techniques to downscale the radar product to very fine temporal resolution (1 min). The effect of temporal sampling error on the results will be further discussed for four fast moving events.

Minor comments: L. 319 and 323:: the reference to Caroline (2015) is missing. Response: We apologize that the Caroline (2015) should be Sandford (2015). The reference is in line 577

L.82-93: I'm not convinced that the wind drift effect should be considered an issue specific for X-band systems. While it is true that X-band have higher spatial resolution, due to the short range the height of the radar beam is in general lower, with a reduced impact of wind drift. Response: We confused the wind velocity and advection velocity in the previous manuscript. Now in the revised manuscript, we have removed the part on wind drift effect and instead investigate the temporal sampling bias caused by advection.

L. 176: which kind of "prior knowledge" do you need for VPR? This is unclear Response: The prior knowledge is the variability of vertical structure of precipitation over Beijing. Based on the vertical structure, we can convert the reflectivity at a high elevation to relatively lower altitude. The vertical structure of precipitating system can be well resolved by the remote sensing instruments. Compared to ground-based radar, space-borne radar has great advantages in measuring the vertical structure of storm thanks to the less interference from the earth curvature, mountain blockage, and beam broadening.

"real-time atmospheric temperature profiles that is commonly used for convective-stratiform classification". Do you have a reference for this statement (convective-stratiform classification from temperature profiles)? Response: This statement can be found in Qi et al., (2013). They use the reflectivity at the altitude of -10° for the convective-stratiform classification.

L. 318-327: the notation Delta_x may be confusing, since this usually indicates the zonal displacement. Response: The notation Δx has been modifed as Δs.

Fig. 5: the mustard-colored and red lines have the same exponent (1.2) but different slopes in the plot. On the other hand, the blue and red lines show different exponents but seem to have the same slope. Looks like the coefficients are switched somewhere Response: We thank the reviewer for pointing out our mistake in the plot. We confuse the coefficients of "all" Z-R relation and Stratiform Z-R relation in this figure.

The revised figure is shown as fig 1. Despite the above analysis, since our convective/stratiform partition method is insufficient, the Z-R relation for both stratiform and convective rainfall is inappropriate, that means we still need to replot the figure in our revised manuscript.

Fig. 8: the result in panel (e) appears a bit counter-intuitive, since the “all” Z-R relation should over-estimate always respect the convective relation and also respect to the stratiform relation, for R higher than approx.. 1 mm/h. The scatterplot shows the opposite. This might be related with the Z-R coefficients issue (previous point). Response: We have double-checked that the plot is correct based on the Z-R relationships used in the previous manuscript. The reviewer suggested that “all” Z-R relation should overestimate both respect the convective relation and also respect to the stratiform relation. However, from the fig 2, we can clearly see that “all” Z-R underestimate the respect the convective relation. Therefore, using the “all” relation will underestimate the convective events like what is shown in our fig 8. Despite the above reason, since our convective/stratiform partition method is insufficient, the Z-R relation for both stratiform and convective rainfall is inappropriate, that means we still need to replot the figure in our revised manuscript.

All the other minor comments have been corrected as suggested

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Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/amt-2016-388/amt-2016-388-AC1-supplement.pdf

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