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Interactive comment on "Evaluation of Doppler radar and GTS Data Assimilation for NWP Rainfall Prediction of an Extreme Summer Storm in Northern China: from the Hydrological Perspective" *by* Jia Liu et al.

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This is a very interesting study which gives a relatively detailed account of how rainfall prediction can be improved using various combination of data assimilation in a widely used model WRF. Some of the findings are certainly of great practical use that may help practitioners to choose proper approach in dealing with severe storms in the context of hydrological forecasting. I also see that the paper is well organised and is easy to read in general.

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I do however think more in-depth discussions/findings are needed for the paper to differentiate itself from yet another case study paper. I wish to see that author address my following observations

1. The novelty. I understand the paper is very much a case study on an extreme event which certainly very useful in its own right. I do however think that paper like this should offer certain in-depth findings that will help model development community. I feel that the paper limits itself to present what has come out of the analysis without giving further reasoning on why. For example, the way of using the GTS data is vague and I don't think the author/or the reader have been able to answer why GTS has contributed to the improvement. For example, the location of the observations that have been assimilated, and what kind of variables are used etc. This will help explain the result with deeper understanding.

2. Technical details. There are many combinations in WRF settings that can affect rainfall prediction. A new scheme would have changed the overall conclusion. It would be helpful to discuss this in more details as to why certain schemes are chosen and whether that would affect the final conclusions. Being set as a limited area model, WRF is prone to the impact from the boundary condition. NCEP might be a good and reliable choice, but again, would using data from other centres like CMA and/or ECMWRF change your final conclusion? Further, please make it clear whether the NCEP data has also involved assimilating GTS data in its operational cycle – i.e., whether it an analysis or a forecast initialised at 00hUTC on the day?

3. The hydrological context. Data assimilation is routinely done at various levels in numerical weather prediction. The big problem to produce a hydrologically compatible rainfall forecast is that many of those forecasts fail to capture the two essential aspects: amount and distribution. With reference to the paper, Fig 5 shows a consistent time shift of all the runs in all modes, i.e., the predicted storms started and stopped around 6-h earlier than the actual one. This might be linked to the setting of assimilation, and I suspect that more likely than not it is due to the constraint imposed by the back-

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ground field from the lateral boundary conditions. This however, has not been properly explored.

4. The choice of using cumulative (only) rainfall may be OK to compare the overall amount in general. Again, for hydrological use, we'd like to see how the prediction agrees with the distributions (both temporal and spatial) of the actual rainfall. So, I think it would be interesting to have a normal hyetograph and a spatial distribution would be more helpful. Some derivative indices like RMSE would make the discussion more convincing.

5. A few terminology and grammar issues: 1) we don't quite often use 'curve' in general, hyetograph is a better and more accurate choice when being used to describe the temporal distribution of rainfall. 2) P5 L26-28 'If more than . . . average value'. This sentence is confusing. 3) P11 L13-15 'The assimilation of radar velocity . . .' I think you meant 'radar radial velocity'. Also the sentence itself is self-contradicting: moisture transport does affect the rainfall 'physical' process. Please elaborate more. 4) P11 L18 '. . . are quite variably' should be 'are quite variable'

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