

Interactive comment on “The effect of flow and orography on the spatial distribution of the very short-term predictability of rainfall” by L. Foresti and A. Seed

E. Ruzanski

evan.ruzanski@vaisala.com

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This is a well-written and well-developed paper that provides a valuable contribution to the body of work on the topic of predictability of precipitation. The paper provides an especially novel study of the effect of flow regime and complex orography on predictability.

I have separated my comments into two parts. The first part includes review-type comments that I feel should be suitably addressed before the paper goes into final archived form. The second part includes specific questions related to studying

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the predictability of precipitation to hopefully stimulate thoughts and discussion on the topic aimed towards directing and improving future related work.

Review comments

1. I believe the term "composite radar observations" should be added to the title to specify the proper frame of the work. There exist significant sources of uncertainty (described in more detail later in this commentary) when using radar observations that create separation from these results and those relating to the true underlying nature of precipitation.

2. Why were rainfall estimates chosen and not a primitive radar product such as reflectivity to do this analysis (e.g., Germann and Zawadzki 2002, Grecu and Krajewski 2000, Ruzanski and Chandrasekar 2012a)? While using rainfall estimates allows verification with rain gauges, uncertainties in the radar–rainfall estimation and radar–rain gauge comparison processes increase distance of the analyses from the true underlying physical characteristic of the predictability of precipitation. If using rainfall estimates is insisted and suitably justified, I contend such estimates should be as accurate and thus as close to physical reality for a study of this nature, where the use of dual-polarization rainfall estimation (e.g., Wang and Chandrasekar 2010) would be more appropriate and should be used.

3. How were edge effects on the measurement domain accounted for in this study? I believe the data coverage area (about 250 x 250 km) is insufficient to estimate lifetimes of precipitation scales sufficiently large as these features will advect through the coverage area within the estimated lifetime. For example, the likelihood that a precipitation pattern will move more than 256 km in the lifetimes of meso-beta and meso-alpha precipitation features is significant. And this is an ideal case where the pattern moves exactly through the center of the coverage area. What if the pattern

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skirts a corner of the coverage area? In this case, observations of the precipitation pattern may be far shorter than the actual lifetime of that precipitation pattern. How did you confirm the events in the dataset were observed to either advect into and out of opposite sides of the coverage domain, advect into and dissipate within the coverage domain, or completely grow and decay within the coverage area?

4. It was written on p. 7753, lines 8–9, that, "To our knowledge there are no comprehensive studies on the scale-dependence of the predictability of rainfall by Lagrangian persistence employing X-band radar data." The work by Ruzanski and Chandrasekar (2012a) (from here forth in this commentary referred to as "RC12") is then cited and thus inferred to not be "comprehensive". Please explain why you believe this was not a comprehensive study; please detail and defend or otherwise delete this statement.

5. It was written on p. 7752, lines 16–17, that X-band radars give higher spatial and temporal measurement resolutions and on lines 25–27 that the optical flow technique used in this study cannot capture motion at scales smaller than those measured by the C- and S-band radars used to collect the data for this study. Yet on lines 27–29 it is written that a simple extrapolation of the results presented in this paper question the utility of X-band radars for very short-term forecasting. These seem to be contradictory statements. Many others have shown research and operational value in using (especially networks of) X-band radars for QPE (e.g., Maki et al. 2005, Wang and Chandrasekar 2010, Lim et al. 2014) and QPF (e.g., Ruzanski and Chandrasekar 2011), specifically using dual-polarization products (e.g., Ruzanski and Chandrasekar 2012b). The difference between the results shown in RC12 and the simple extrapolation of the results in this paper differ by an order of magnitude in terms of predictability. The results in RC12 using microscale data also fit well linearly with those of previous studies using different data sets and models. Perhaps the longer lifetimes of microscale precipitation represented by networked X-band radar

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observations presented by RC12 suggest the enhanced value in using X-band radars to observe these small scales, as stated in this paper? Please explain and defend your statement to the contrary made on p. 7752, lines 27–29, or otherwise omit from the paper this statement and also the results of extrapolating larger-scale results to smaller scales below those represented in the data. It seems unfair to make general statements about the utility of X-band radar data when X-band data were not used in the analyses, especially when such statements contend with results presented in studies that support the contrary conclusion that did use X-band radar data.

Discussion comments

The comments and questions that follow seek to motivate accurately answering the question, "What is best way (in terms of accuracy and generality) to quantify the underlying physical property of predictability of precipitation?"

1. Many papers (e.g., the series beginning with Germann and Zawadzki 2002) use a nowcasting model as the tool to study/quantify predictability. Thus, isn't model and/or analysis technique dependence implied in this and other related studies? This question comes as an extension of the statement written in this paper on p. 7753, lines 11–13, "The scale-dependence was analyzed by upscaling the forecasts and the values are not directly comparable to the ones obtained by scale separation within STEPS."

2. This paper and the others cited within stated many sources of uncertainty in their models and approaches (e.g., the "Conclusions" section). Similar studies have also stated an extensive list of uncertainties, including but not limited to radar observational issue such as ground clutter contamination, beam blockage, and bright band contamination, data processing issues such as the gridding and mosaicking processes, and radar–rain gauge verification uncertainties. It may be the case where

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all these sources of error [e.g., observational and data processing, model, verification errors; see equation in Ruzanski and Chandrasekar (2012a)] preclude any type of quantification of predictability using a practical model-based performance approach using only radar data. Can we accurately quantify this uncertainty? If we cannot, it is difficult to truly say which studies (if any) reliably or how reliably quantify the predictability of precipitation.

3. I believe better observations and sophisticated data analyses methods (e.g., LAPS) should be used to create the datasets used for future related precipitation predictability studies vs. strictly using radar observations, especially moving to the analysis of smaller scales. What are the specific effects of the nature of radar observations and related data processing on limiting the study of predictability? For example, it is well-known there is a fundamental space-time scale relationship; yet, radar observations fundamentally don't have a constant spatial Nyquist frequency (Trapp and Doswell 2000) but do have a constant temporal Nyquist frequency (facilitated by a fixed scanning strategy). Analysis methods such as LAPS can necessarily mitigate short-comings in radar observational characteristics (e.g., beam spreading and heightening with increasing distance from the radar with corresponding ground clutter contamination near the radar) and simplifying assumptions and associated degradation in data processing (e.g., the gridding and mosaicking processes) by suitably including other observations. Analysis techniques such as LAPS are continually improving (Toth et al. 2012), have improved significantly since some earlier work in quantifying the predictability of precipitation using radar (e.g., Zawadzki et al. 1994) and composited radar observations (e.g., Germann and Zawadzki 2002), and should be considered for future work on this topic.

4. Should an accurate quantification of predictability of precipitation include meta-analysis of the body of work on the topic done so far? Following comment (2) in this section, can only qualitative statements be made and conclusions be drawn

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to characterize the true, underlying predictability of precipitation given the many (and sometimes/often times large) sources of error contributing to quantitative results, especially when characterization of such error structure is itself not well-modeled/uncertain?

5. Should studies of "predictability" be restated/redefined/reframed as "the limit of value in forecasts made by a particular model using particular data for a particular end-user application or range of applications"? The study of predictability relates to the estimation of a general upper limit on the extent of short-term forecasting. I believe it is possible that forecasts that have decorrelated, perhaps due to dominant phase errors but are still accurate in terms of shape and intensity, can still provide value to the end-user in some applications. Thus, it may be misleading to say precipitation is not predictable when such predictions can still add practical value in certain meteorological scenarios and/or applications.

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