Hydrology and Earth System Sciences

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Discussions

Interactive comment on "Development and verification of a real-time stochastic precipitation nowcasting system for urban hydrology in Belgium" by L. Foresti et al.

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This is a comment on the manuscript entitled "Development and verification of a real-time stochastic precipitation nowcasting system for urban hydrology in Belgium". The paper discusses the probabilistic STEPS system implemented in a real-time environment in Belgium. The paper is generally very well written, and it follows a logical pattern. The technical description of the system demonstrates well the operational feasibility of the system. Also, the implementation, validation and verification of the proposed technique are well described.

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An extensive set of verification tools is applied to demonstrate both deterministic and probabilistic nowcasting capabilities of the system, and the results of the verification are discussed extensively. The scientific rigor, soundness of the methods, and well-written text made the paper very interesting and pleasure to read.

I also have a few additional minor comments.

1.Introduction: I think it is quite widely accepted that the term "nowcasting" refers to very short range forecasting in the time range 0-6 hours (e.g. https://www.wmo.int/pages/prog/amp/pwsp/Nowcasting.htm). To avoid confusion, it would be better to first define the term nowcasting with this definition, and then specify that this paper considers only the first two nowcasting hours.

2.Introduction: The paper underlines advantages of radar-based nowcasting over NWP during the first nowcasting hours. To be fair, authors should better acknowledge that NWP typically outperform radar-based nowcasting after a few forecast hours, which is still in the nowcasting time range (assuming that the definition of 0-6 hours is adopted). I also think the paper should acknowledge that NWP community working very hard to improve the nowcasting of rainfall (see e.g. Sun, J., and Coauthors, 2014: Use of NWP for Nowcasting Convective Precipitation: Recent Progress and Challenges. Bulletin of the American Meteorological Society, 95, 409-426.).

3.The verification was performed with four case studies. This not very extensive verification, given the availability of radar data and low computational costs nowadays. I do not feel strongly enough about this to make it a major issue, but perhaps Authors could underline that more extensive evaluation would be needed to capture full performance of the system.

4.I was a bit surprised that Authors did not do any comparison against a reference system (e.g. basic deterministic extrapolation). It would have been interesting to see differences between a legacy system and STEPS-BE (e.g. in terms of RMSE, GSS).

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5.P. 6850: "Another explanation for this underestimation is due to not using a model for the radar measurement errors, in particular due to the space– time variability of the Z–R relationship". It is not clear to me how errors due to initial conditions can be observed in this verification, because the reference data applied in the verification data is obtained from the same erroneous radar data.

6.Authors might want to revise the use of the term skill. Isn't it by definition a measure of forecast accuracy with respect to the accuracy of a reference forecast? The term is quite widely used throughout the text.

7.P. 6587 and p. 6849 (Brier score and Brier skill score), also related to my previous comment. Brier score (BS) is a measure of accuracy, and BSS compares BS of two systems. Thus, I believe it would be better to say that "The Brier skill score characterizes the relative accuracy of the probabilistic forecast compared to a reference system". Although climatology or sample climatology is often used as a reference, BSS can also be computed against other reference forecasts, e.g. another probability forecasting method or even a deterministic forecasting method treated as a probabilistic binary forecast.

8.P. 6858: Foresti et al. (2013). I couldn't find it in the reference list. Foresti et al. (2014)?

9.eq. (8). It seems that index m is not defined. Shouldn't the index i under the square root be replaced with m?

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