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Discussions

Interactive comment on "An attempt to deal with flash floods using a probabilistic hydrological nowcasting chain: a case study" *by* F. Silvestro et al.

Anonymous Referee #1

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In their manuscript "An attempt to deal with flash floods using a probabilistic hydrological nowcasting chain: a case study", F. Silvestro and colleagues present – as the title suggests – a case study on the potential benefit from probabilistic flash flood forecasting in the Liguria Region, Italy.

The authors first describe the general hazard from flash floods, the challenges in their prediction, and the specific settings of the study basin in the Ligura region. Then, they briefly outline their "Probabilistic hydrological nowcasting framework" (PHNF). This framework includes the estimation of rainfall fields from merging radar-based rainfall estimates with rain gauge observations (Silvestro and Rebora (2012)), the stochastic

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nowcasting of the precipitation with a lead time of two hours according to Metta et al. (2009), and the simulation of the rainfall-runoff transformation. The entire framework has already been presented in detail in Silvestro et al. (2011) and Silvestro and Rebora (2012). Finally, the authors present results of the PHNF for a flash flood event that occurred in the night from June 8-9, 2011, in the Entella basin. Forecasts are presented for different forecast issuing times, and the PHNF system shows a significant increase of streamflow with a lead time of about five hours.

The topic of flash flood forecasting, including the aspects of uncertainty and predictability, and the operational applicability of probabilistic forecasting frameworks, is surely within the scope of NHESS.

The documentation of the events and activities on June 8-9, 2011, and the post-event analysis of the PHNF system output is surely interesting to read, and is, as such, worth being shared with other practitioners in this field. However, I have to say that the paper does not provide any new insights or scientific innovation that would merit a publication in NHESS. I hope that this does not sound too harsh because it is not meant to be. The entire forecasting approach is valid and interesting, but it has been entirely published in the aforementioned references. The event itself is significant, but not unusual in a way that would merit a publication in terms of a full-blown post-event analysis (which the manuscript is not!). The main motivation of the authors to write this paper seems to be the discrepancy between the operational numerical weather prediction models (which failed to forecast the event) and their PHNF system (that relies on precipitation nowcasts from radar-based rainfall fields). An interesting experience as this might be for the local Civil Protection agency, such a discrepancy between NWP and radar-based nowcasts is not unusual at all. The two hours lead time of the radar nowcast (once the radar detected significant convective activity) and the catchment's three hours reaction time add up to an effective lead time of the system of about five hours as expected. Surely though, the analysis of a single event does not tell us *anything* about the added value of a (probabilistic) forecasting system. Hence, the

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paper does not reach any substantial conclusions based on the presented results.

Therefore, I do not recommend this manuscript for publication in NHESS.

Apart from the aforementioned reasons which mainly include the lack of new scientific insights and substantial conclusions, I have a couple of other objections (which are not exhaustive!):

1. The PHNF in its presented form does not qualify as a probabilistic forecasting system, but rather as an ensemble forecasting system. According to Silvestro and Rebora (2012), the forecasted discharge is computed from the mean of the forecast ensemble members. A probabilistic forecast would come up with a statistical distribution that allows computing the probability that a certain discharge is exceeded. There are various approaches to derive probabilistic forecasts from ensembles. A simple example is to derive the distribution from plotting positions (see e.g. Wilks 2006 for a nice overview with a focus on meteorological forecasting systems). More advanced approaches would use Bayesian approaches (see e.g. Krzysztofowicz and Kelley, 2000).

2. Based on the previous remark, the authors should prove the added value of a probabilistic forecast if they claim to operate a probabilistic forecasting system. In Silvestro and Rebora (2012), the skill was only evaluated based on the ensemble mean. There are scores which are explicitly useful to evaluate the skill of probabilistic forecasts (see e.g. http://www.cawcr.gov.au/projects/verification). It would be interesting to prove the superiority of the probabilistic over a conventional deterministic approach.

3. It would be interesting to separate the skill of the precipitation forecast from the skill of the forecast that is due to observed rainfall that is routed through the model (thus the skill that the forecast has simply due to the lag or reaction time of the catchment).

4. The authors often make rather absolute statements such as

- “Numerical Weather Prediction Systems (NWPS) and Ensemble Prediction Systems (EPS) do not allow for the prediction of precipitation with sufficient accuracy” (p. 7498,

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l. 20 ff.),

- ”the unpredictable nature of the aforementioned storms” (p. 7499, l.24),

- “these events cannot be forecasted with the hydrometeorological forecast chains” (p. 7499, ll. 27 ff.),

- “it is impossible to base the streamflow forecast on rainfall observations and rainfall-runoff models” (p. 7500, ll. 3 ff.),

- “This could be a very useful tool when faced with unpredicted/unpredictable intense rainfall events” (p. 7507, ll. 14 ff.).

In my opinion, such statements are neither adequate nor helpful. The skill of a forecast system, let it be meteorological or hydrological, could be evaluated depending on the spatiotemporal application scale, the methodological approach, the lead time, the informative value, and many other criteria, but classifying events as “unpredictable” or “impossible to forecast” is not helpful in addressing issues of predictability and uncertainty.

5. The language of the manuscript needs substantial review by a native speaker.

6. Particularly in the introduction, the referencing is weak, with a lot of statements that are not well founded.

7. I think the use of SMS to signal unusual observations or simulation results is useful from an operational perspective, but it is not a scientific innovation that should be reported in NHESS.

Other minor remarks (not exhaustive!):

p. 7502, l. 11: Silvestro and Rebora (2011) is not contained in the references section (it seems to be Silvestro and Rebora (2012) instead)

p. 7509, ll. 25-26: Reference has the wrong title

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Section 2 should be accompanied by a map that shows the basin, the radar range circles and the rain gauges in one spatial context.

p. 7502, ll. 10 ff.: The term “scenario” is inappropriate in the context of ensemble forecasts (also on other locations of the manuscript). Instead, the authors should use the term “ensemble member”.

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