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Hydrology and Earth System Sciences Discussions

Interactive comment on "Flash-flood early warning using weather radar data: from nowcasting to forecasting" *by* K. Liechti et al.

Anonymous Referee #1

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Recommended Disposition: Subject to re-review

Summary

This paper is effectively a hydrologic-focused evaluation for a modeling chain conditioned on different forcings. The forcings vary from deterministic to probabilistic, radar vs. gauge, and NWP vs. extrapolated radar fields. Overall, the objectives of the study are interesting and worthy of pursuit. Moreover, the radar and atmospheric NWP components of the study are quite well developed. However, there are some methodological issues in the use of the hydrologic model which will require more careful consideration by the authors. I summarize them below, and encourage the authors to pursue this study despite the additional work that will be needed to justify their present conclusions.

Major Comments:

1. Dependence of results on hydrologic model parameters – The authors devote no more than 2 sentences regarding the details of hydrologic model calibration process toward the end of section 2. Later in the paper, we're told that the interpolated rain gauge fields were the forcing dataset to calibrate the model. Several details are lacking here. This is critical information because the model parameters can dictate the apparent skill in the forcing datasets, which are being compared. How many parameters does the model have? Are they spatially distributed? Are the estimated separately for each basin? Is the parameter estimation method automatic or manual? If the model parameters are estimated with rain gauge forcing, then the hydrologic model forecasts will be most skillful when the same inputs are used, or at least inputs that resemble the rain gauge dataset used during calibration. If something different is used as forcing, then the forecasts will inevitably become biased even if the forcing is closer to the truth. In other words, the model parameterization process can obscure or rather account for errors (especially bias) in the rainfall forcing. Another issue here is the statistical objective used during model calibration. The authors state on pg. 1295 that the best parameter set is chosen which simulates the average flows best and has the smallest volume error. The authors need to report on the specific modeling objective and the statistical scores during calibration and validation. Also, if the model parameters are tuned to simulate average flows, then they're not necessarily guaranteed to produce forecasts that are skillful for flooding flows (i.e., q95). There is a wealth of studies reported in the literature that have come up with novel methods to objectively evaluate rainfall inputs using a hydrologic model. I suggest the authors review those studies and reconsider their present strategy of using a fixed model conditioned on rain gauge forcing.

2. Hydrologic model uncertainty – The authors go to great lengths to develop ensembles based on the radar data combined with an NWP model. However, the resulting ensembles of hydrographs assume there is a perfect hydrologic model with perfect initial

soil conditions and parameters. Hydrologic model uncertainty is significant, especially for extreme events in complex terrain, yet it isn't considered nor even mentioned in this study. Justification for this simplification is needed.

3. Interpretation of results – First, interpreting the results have issues in regards to my first major comment. But, when I view the overall results in Fig. 6 it appears as though REAL-C2 is the best followed by PLU-C2. PLU-C2 has an advantage of being the forcing data set during calibration, so it should be relatively immune from any deleterious effects due to bias. However, it has the disadvantage of being fairly simple. The authors state on pg. 1298 that an inverse distance weighting scheme is used for the spatial interpolation. Why not consider orographic enhancement in the spatial interpolation scheme like you did with the radar data? Why not use kriging? Also, since this product is used as forcing during calibration, the reader really needs to see some of the gridded gauge fields. Perhaps the density is sufficient enough to resolve orographic effects already? Regardless, the skill of this product being second only to REAL-C2, which gets the advantage of providing an ensemble, isn't mentioned in the conclusions. I hypothesize that PLU-C2 would be the overall winner in the present experimental design if the authors created an ensemble of PLU-based inputs, which is readily possible if they used kriging. Would it be possible that the best flash flood forecast chain is accomplished using the ensemble gauge estimates for nowcasting combined with the NWP for forecasting? This result would be rather problematic for the radar community I would think.

4. Specificity of study – The title in its present form is far too broad and does not adequately describe the study. It is quite specific to the radar ensemble method, orographic considerations, COSMO-2 forecasts, and the hydrologic model specifically calibrated to data on a few Alpine basins in Switzerland. The regionality and Alpine specificity needs to be included in the title. Moreover, I would argue that this paper is a hydrologic evaluation of different rainfall inputs that are radar-estimated, forecast, deterministic, and probabilistic. It is doubtful the hydrologic model and its parameterization will apply else-

where. Even the rainfall forcing products will have limitations in other complex terrain flooding situations where there is severe beam blockage, overshooting, and convective initiation in the mountains, rather than more straightforward quasi-linear propagation and orographic enhancement. This latter situation is more typical in mid-latitude synoptic systems that encounter very large mountain chains, such as the Alps in Europe or Sierras in California.

Minor Comments:

1. Pg. 1290, Line 18 - Please just report the skill rather than a qualitative description.

2. Pg. 1294, Line 19 – An hourly time step is incommensurate with the model grid cell resolution (and flash flooding). Please comment on why they didn't run the hydrologic model at a shorter time step.

3. Figure order (throughout) – The figures should be called in sequential order, so that Fig. 1 is cited first, then Fig. 2, etc.

4. Pg. 1296, line 10 – The 3rd criterion relates to my major comment #4. That is, these types of flash-flood producing systems are rather specific to this region.

5. Section 3.2.1 (and throughout) - This is not coupling unless there are 2-way feedbacks between REAL and COSMO-2. Instead, the two forcings are merely provided to the PREVAH hydrological model. This needs to be corrected in several instances in the text.

6. Pg. 1298, line 15 - This is an important detail that needs to be expanded. Were the bias factors computed for each input, or just the PLUVIO input? Why would a bias factor be needed during calibration? Typically, a systematic bias in the precipitation is readily dealt with by adjusting a model parameter (e.g., making the soil profiles either deeper or shallower).

7. Pg. 1300, line 16 – Subject-verb disagreement.

8. Section 6.2 - Results are presented out of order in relation to Figs. 5-6. Also, the order of the basins in the figures does not match the sub-section ordering.

9. Pg. 1308, line 1 – How to explain this different bias behavior for the basins? 10. Pg. 1312, line 1 - Can this statement be true if there is no orographic enhancement

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 1289, 2013.