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Interactive comment on "A flood episode in Northern Italy: multi-model and single-model mesoscale meteorological ensembles for hydrological predictions" *by* S. Davolio et al.

Anonymous Referee #1

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General comments:

The paper entitled "A flood episode in Northern Italy: multi-model and single-model mesoscale meteorological ensembles for hydrological predictions" by S. Davolio et al. presents an investigation of uncertainties in the meteo-hydrological forecast chain and sources of these uncertainties. Specifically, the focus is on the differences of precipitation forecast of LAM ensembles with different ensemble generation strategies and the effect on discharge forecasts by a rainfall-runoff model (also in comparison to the results based on a global model NWP EPS as input to the discharge model). This

investigation is carried out on the basis of a flood episode in Northern Italy with two events affecting the Reno River (Apennines).

The topic of the paper is up-to-date and addresses relevant questions. Most of the uncertainty in such approaches of river discharge predictions has its origin in the meteorological input. Therefore, investigating different aspects of this NWP uncertainty and their effect on the discharge forecast is essential for dealing with probabilistic approaches in hydrological modelling. This approaches and results are important contributors to decision making processes.

The paper contains a good description of the state of the art. The applied methods are well described, the structure is clear and the road-map of this investigation is easy to follow both from a scientific point of view as well as concerning the quality of the presentation. The study is based on sufficient forecast data of state-of-the-art model and discharge observations. The caveat related to general conclusions based on case studies is included in the paper. Even though the paper is based on one case study, the results are sufficient to highlight important aspects of the problem and to trigger further research.

The language is good (as far as I can judge this as a non-native speaker).

However, needs some minor revisions concerning motivation for and scope of the paper. Furthermore, one aspect of the results deserves more attention. Those aspects are briefly described below together with a list of additional minor comments.

I can recommend the paper for publication after some work has been done on those minor aspects.

A) Scope of the paper / motivation

The state of the art an related literature is actually very well described in the introduction. However, the statements relating this to the specific research target and the scope of this paper have to be more precise and significant. For example, the sentence containing the statement "..two different...approaches...are compared" is too weak, but currently, there are no stronger statements of motivation for the work which has been done. Statements like: "Considering the entire meteo-hydrological chain, the lack of theoretical development supporting strategies for flood forecasting leaves room for testing ad hoc methodologies on a case by case basis (Cloke and Pappenberger, 2009)." sound a bit like "We are doing it, because according to a review paper, we can try anything". It would be good to have a few more statements on why the authors did exactly what they did (why single-model versus multi-model). This can be easily done by distinguishing their work from i.e. those by Adams & Ostrowsky (forecast range) and Addor et al. (single model EPS) so that the reader knows what is really new and can easily get an idea of the overall research target of the presented work. Furthermore, the authors could motivate there work with better arguments, e.g. referring to the "meteorology-only" literature or the other aspects of review papers like Cloke & Pappenberger and Cuo et al. All this is already hidden in the current text but needs some more depth in the argumentation to highlight those aspects of the work which bring an additional contribution to meteo-hydrological forecasting.

B) Extra from hydrological model

Beside mentioning the spread (e.g. represented by the 10th and 90th percent quantiles), section 5 could put more emphasis on the interpretation of the best discharge model forecasts (in this case study) as scenarios which provide decisive information for the comparison of the EPS approaches and their effect on river discharge forecasts as well as for any potential decision making based on such forecasts. The focus right now is on the spread which is of course influenced by the "extreme members" but at the time of the observed peaks, those "extreme members" can make the difference between the multi-model EPS and the COSMO-LEPS. E.g. for the first peak and the investigation of the shorter forecast range (bottom row in Fig. 6) the distance between the 10th and 90th percent quantiles is not significantly different for the two EPS approaches, however the most extreme member of the multi-model EPS provides the

decisive information for the warning level which is not revealed even by the 10/90th percent quantiles. Such effects cannot be seen in the meteo-only forecasts if looking at exceedance probabilities as it is done in Fig. 4 and 5. This is an important aspect of the investigation of uncertainty in such a meteo-hydrological forecast chain.

C) Other comments âĂć Introduction, page 13417, line 27/28: why parentheses for "(and boundary)" ?

âĂć Intro, p.13420, l. 1: "multi-analysis ensemble" It is true that he members use different ICs, but are those really based on different analyses? From the current description of the systems I would assume that the analysis in a sense of assimilation of observations for all LAM members has been in the ECMWF EPS and the LAM EPS do not incorporate different analysis (apart from the effect of mixing two ECMWF EPS start times in the clustering, but thus is more "same analysis approach at different start times"). I have doubts whether "multi-analysis" is appropriate here.

âĂć Section 2.2: Is there any reference for "Jacobsen and Heise" available?

âĂć Section 2.3: Is there any reference for "Noah land-surface model" available?

âĂć Is there any reference for "Jacobsen and Heise" available?gure 6: The graph needs a higher resolution. The annotations explaining the different lines is hardly readable, even when zooming in (it's better in Fig. 7).

âĂć Section 4, p. 13428, l. 17+19 and next page, l. 2+ 12: Should it be "LAMs" instead of "LEPSs"?

âĂć Section 5 and Fig. 7: It is interesting to note that most members do not have the observed two-peak structure in the discharge prediction. However, if a member shows the two peaks (e.g. P35 members), the effect depends on the LAM, e.g. WRF has the lowest peaks. The authors should include this in the discussion of the multi-model results.

âĂć Section 5, p. 13430, l. 4: "...it provides a more reliable estimation...". This could be interpreted as the EPS being reliable which is a technical term in EPS forecasting. This would need a proof in term of a calculated reliability or another word should be used instead.

âĂć Section 5, p. 13430, l. 15/16. "...precipitation patterns remain similar among the forecast driven by the same global representative member.." I think, this statement is too generalizing. E.g: COSMO (m36) member is more similar to the WRF(m3) than to WRF or BOLAM (m36) member, the same with COSMO (m35) and WRF(m23). The stratification along driving members is not that obvious which by the way further supports the use of multi-model approaches. Authors should comment on this in the text

âĂć Section 5, p. 13431, l. 11-13: I would prefer a less generalizing conclusion about the dominating effect of boundary conditions based on such a case study. The tendency is obvious, but maybe a weaker formulation would be better

âĂć Section 5, p. 13431, l. 27+28: "...have not fully diverged yet...,...initial perturbations have not grown enough...". This is also linked to the general properties of COSMO-LEPS as being based on clustering of IFS-EPS. The latter has its focus on spread in the medium range.

âĂć Section 5, p. 13431, l. 29: "close to each other" instead of "close each other"

âĂć Section 6, p. 13432, l.12: "multi-analysis"-> see above

âĂć Section 6, p. 13422, l. 9 and l 23: "..Reno River basin as an area likely to be affected by...". Possible redundancy or repetition of statement?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 13415, 2012.