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Discussions

Interactive comment on "Flash-flood forecasting in two Spanish Mediterranean catchments: a comparison of distinct hydrometeorological ensemble prediction strategies" *by* Béatrice Vincendon and Arnau Amengual

Anonymous Referee #2

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I totally support the opinion of referee #1.

The paper now submitted to NHESS is substantially identical to the previous version submitted to HESS, just some minor (and of less importance) reviewer comments have been taken into account. No additional tests, new results and conclusions characterise this last version of the manuscript. The contents of figures is the same (except for the values of two measures in Fig. 15, panel b, that have unexpectedly been modified with respect to the HESS version, given that no changes in the text of the manuscript justify a change of results with respect to the version submitted to HESS). Even, in

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Discussion paper

this last version, authors have avoided some critical issues by simply omitting technical details throughout the manuscript. As I have already reviewed the paper submitted to HESS, for this review process I just briefly recall (with slight adjustments for the present submission) the major comments that I provided for the previous review's process. For the contents of minor comments, please refer to the review report provided to HESS, given that most of those minor comments are still unsolved or not fully fulfilled.

Overview

In my opinion, the contents of this manuscript fit better to a technical report (for instance, a "deliverable" for the HYMEX experiment) rather than to a research article (to be published on a international scientific peer-review journal). Actually, even though the subject of the present manuscript is within the journal's scope, I deem that it fails to meet the requirements to be considered as a research article for the journal NHESS, given that it does not report substantial and original scientific results with respect to: - the modelling of natural phenomena, and the integration of measurements and models for the understanding and forecasting of the behaviour and the spatial and temporal evolution of hazardous natural events as well as their consequences; - the design, development, experimentation, and validation of new techniques, methods, and tools for the forecast of heavy precipitation events and the resulting river floods. This opinion is based on the following issues: (1) the present manuscript does not introduce an innovative use of ensembles in hydrological forecasts and provide original recommendations about the interpretation of outcomes from meteo-hydrological model coupling; (2) the considered meteorological and hydrological models were already tested in several previous studies, some of them also from the same authors; (3) one of the two case studies was already investigated in a past work by one of authors (i.e., Amengual et al., 2015), testing a near identical meteo-hydrological forecasting chain; (4) the study suffers from some potential fatal flaws about the calibration/validation of the two hydrological models and the application of the method to generate one of the ensemble forecasts (i.e., the "perturbation method"; see general comments for more details), that may affect the interpretation of results; (5) the large majority of conclusions are quite expected and/or duplicates previous studies of both authors.

General comments

(1) The SREPS approach (page 3, lines 14-16) should be deeper introduced and described, acknowledging specific references on the subject over the last years. For instance, it should be highlighted which are the advantages of a SREPS approach with respect to other meteorological modelling approaches, which is the state-of-the-art of SREPS systems.

(2) One of the two case studies (i.e., the 2012 event) was already investigated in Amengual et al. (2015), testing a near identical meteo-hydrological forecasting chain. Actually, the version of WRF used in this study differs slightly from the version of WRF used in the previous study (in particular, for the higher horizontal resolution and number of vertical levels). However, a in-depth discussion about this issue is not provided in the present manuscript. An original development should be the evaluation of the impact of these upgrades (for instance, by showing and discussing whether the rainfall forecast accuracy for hydrological applications improves with the latest version of WRF, preferably testing a longer period or more case studies), in order to determine the model sensitivity to the atmospheric processes leading to the high precipitation amounts.

(3) The two meteorological models have the same nominal horizontal resolution. However, the ensemble based on AROME-WMED may be characterized by an actual horizontal resolution that is higher than the ensemble based on WRF, due to the different approaches used to generate the ensemble (that is, the AROME-WMED approach inherently contains a sort of downscaling due to the error climatology correction). This issue may be an additional subject to be deeply investigated in order to improve the contents of the manuscript.

(4) The description of the application of the perturbation method to generate an ensemble forecasts starting from the deterministic AROME-WMED forecast lacks of some

details. In this version of the manuscript, authors state that “For the present study, this climatology has been extended using a more extended domain (including Northern Spain) and including more rainy events. The study sample gathered rainy events from 2008 to 2010.”. Evidences (results and/or references) should be provided to prove this statement. Authors should highlight the differences in the computation of statistics of the perturbation method for the present study with respect to the same statistics computed for the study Vincendon et al. (2011). Are the study catchments included in the mentioned “extended domain”? At least one study catchment seems to be located outside Northern Spain. The feeling is that this approach was not properly adjusted to the investigated study areas (i.e., statistics of the perturbation method estimated over not fully appropriate area and data period).

(5) The calibration/validation task used for the two hydrological models may hamper a proper comparison of performance. On the one hand, HEC-HMS is calibrated by considering (at least) one of the investigated case studies (if inferred correctly from Amengual et al., 2015). On the other hand, ISBA-TOP did not require a specific calibration for the selected study catchments (as stated in the manuscript). The potential overfitting of the HEC-HMS parameters could influence the comparison on the impact of different model structures (which is one of aims of the manuscript). Furthermore, the calibration performed considering the investigated case studies may not conciliate with a simulation of real-time application of the forecasting chain driven by HEC-HMS (which is one of aims of the manuscript).

(6) The term “multi-model” is not properly used, given that a real multi-model approach is not proposed. Just a separate evaluation (and comparison) of outcomes from the coupling of each pair of meteo-hydrological model is discussed For instance, a statistical post-processing approach that merges the input from both meteorological ensembles (to be then used as a single input to hydrological simulations) or the outcomes from both hydrological models (driven by the same input) could be considered as an original development and added value for the manuscript with respect to previous re-

search articles published by the same authors. The results of such a merging could then be compared to outcomes of the single meteo-hydrological forecasting chains.

(7) The conclusions are quite expected or similar to conclusions in Amengual et al. (2015) and Vincendon et al. (2011). For instance,

similar conclusions:

- Amengual et al. (2015): page 21, "it is clear the added value conveyed by an ensemble strategy when compared against a deterministic forecasting approach"

nhess-2017-353, Page 16, lines 5-7, "The use of an ensemble rather than a deterministic approach clearly improves the forecasts"

- Amengual et al. (2015): page 22, "We expect that further work will allow to account for uncertainties associated with different physical parametrizations of the WRF model (i.e., cloud microphysics, moist convection and boundary-layer schemes) in order to determine the model sensitivity to the atmospheric processes leading to the high precipitation amounts. Further application and evaluation of these methods to a broader climatology might also improve the forecasting and warning schemes presented herein, and will better establish their confidence levels from an operational perspective"

nhess-2017-353, Page 16, lines 9-10, "accounting for the physical scheme uncertainty could enhance the ensemble spread."

- Vincendon et al. (2011): page 1541, "Considering their cost, the size of such convection-permitting NWP ensembles should still be limited (around 10–20 members) in the foreseeable future."

nhess-2017-353, Page 16, lines 17-19, "Regarding the ensemble size, the ensembles with 50 members obtain the best objective scores, but considering more computationally-affordable ensemble sizes (around 20 or 25 members) does not deteriorate the ensemble performance significantly."

expected conclusions:

- Page 16, lines 3-4: “a spatially-distributed (even uncalibrated) model better reproduces the flood dynamics.”. This outcome is quite expected, given the features of tested models (fully distributed and physically-based model versus a semi-distributed and conceptual model).

- Page 16, lines 12-13, “the conclusions made on the catchment-averaged rainfall forecasts can be different from those made on the discharge forecasts owing to the strong non linearities in the rainfall-runoff transformation.”. This outcome has widely been described in past specific literature.

- Page 16, lines 13-14, “This fact clearly shows the added value of assessing ensembles through an hydrological point of view.” This outcome (i.e., the use of hydrological model simulation to evaluate the accuracy of QPFs) has widely been described in past specific literature (also by Amengual et al. (2015): page 21, “Since small-scale and intense cores of precipitation limit the accuracy of evaluating QPFs from sparse rain-gauge networks, the streamflows estimate better the amount of precipitated water over the basin.”).

- Page 16, lines 15-17: “As far as lead time is concerned, a better skill is obtained for ensembles with shorter forecasting lead times”. This outcome has widely been described in past specific literature.

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