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Hydrology and Earth System Sciences Discussions

Interactive comment on "Inclusion of potential vorticity uncertainties into a hydrometeorological forecasting chain: application to a medium size basin of Mediterranean Spain" *by* A. Amengual et al.

Anonymous Referee #2

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General comments

The authors present case studies examining the efficacy of a method for flood forecasting where ensemble precipitation outputs from a mesoscale numerical weather prediction (NWP) model are used to drive a semi-distributed rainfall runoff model. In their approach, an ensemble of NWP rainfall forecasts is generated by applying potential vorticity (PV) perturbations on the synoptic scale. The mixed results illustrated in the paper add to the growing mound of evidence that deficiencies in available hydrometerological modelling techniques and inherent difficulties in validation need to be

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overcome, in order to improve flash flood forecasting.

The various skill scores presented give an overview from an operational viewpoint of the performance of the system. However, the authors could also consider giving more emphasis to examination, at the process level, of the poorly performing forecast cases. This would give added value to the paper and give an indication of areas to concentrate future research in improving such models and their initialization.

The paper is mostly clear, although the section describing the generation of the PV perturbations was difficult for me to follow, and there are a few missing details. My specific comments are given below.

Specific comments

1. What are the problems/advantages of using MM5 for predicting rainfall? Has any work been done evaluating its performance in this area? How does it compare to other more modern high resolution NWP models such as WRF, the UK Met Office's UM etc?

Lean HW, Clark PA, Dixon M, Roberts NM, Fitch A, Forbes R, Halliwell C. 2008. Characteristics of high-resolution versions of the Met Office Unified Model for forecasting convection over the United Kingdom. Mon. Wea. Rev. 136: 3408-3424.

Weisman ML, Davis C, Wang W, Manning KW, Klemp JB. 2008. Experiences with 0-36-h explicit convective forecasts with the WRF-ARW model. Wea. Forecasting 23: 407-437.

2. The authors should also consider other possible methods for NWP rainfall ensemble generation in their literature review (section 1) and compare how their PV perturbation approach influences the physical processes and hence results in the forecasts, in comparison to the other methods.

3. p 539 line 6-7. What do the authors mean by "predict the probability of future weather events as completely as possible"? This is a vague statement and open to misinterpretation. Please explain/define the probability distribution or distributions you

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are hoping to characterize. Exactly what you are aiming at should influence the design of your ensemble.

4. sec 2.2/4.1 The authors use a set of raingauge data. What is the observation error variance for this data? What quality control procedures were carried out? Are there any independent data for the region to compare with (e.g. rainfall radar or satellite derived estimates). The authors should provide a reference for the kriging method used (there are several variations of kriging) and indicate the value of any parameters that need to be set. How sensitive are the results to the grid length chosen? Are there any issues in representativity of the NWP data compared with the gauge data in applying this technique?

5. Section 5. The description of the method for calculating the PV displacement errors and intensity errors is unclear - I am not sure I would be able to repeat your work and get the same climatology or produce the ensemble perturbations in exactly the same way. Perhaps if you were to pin down the definition of these errors in terms of some equations this would become clearer.

6. p550 How long is the spin-up period for MM5 in each case-study? Are the results sensitive to the length of the spin up period?

7. Have you considered the use of any scale dependent measures to evaluate your precipitation forecasts relative to the gauge data - clearly MM5 will not provide useful information at all scales. For example (and references therein)

Roberts NM, Lean HW. 2008. Scale-selective verification of rainfall accumulations from high-resolution forecasts of convective events. Mon. Wea. Rev. 136: 78-97.

8. For three of the cases, the MM5 forecasts are clearly not close enough to reality to allow particularly skillful rainfall-runoff forecasts. In the text you attribute these problems to "biases", when the addition of PV perturbations makes little difference to the development of the forecast and when orographic effects are thought to be dominant.

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This would seem to imply a need to carry out the experiments at a higher resolution where orographic effects are better resolved. Have the authors tried this for any of the poorly performing cases?

Minor comments

1. There are a few instances of awkward phrasing through the paper. I have given some of them below.

p 537 line 15 "contributing to implement"
p539 line 5 "has arisen a"
p549 line 27 "allows introducing"
p552 line 27 "are benefited from"

2. Typos:
p 560 line 14 "diferent"
p560 line 27 "cathment"
p583 caption of Fig 10 "desviation"
p584 caption of Fig 11 "exceedence"

3. Fig 1 Highlighting in shaded green is difficult to see because green is also one of the colours used for the topography.

4. Fig 3 "Watershd.shp" ?

5. Fig 7 needs a colour-bar to explain the shading (presumeably this is model orography?)

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 535, 2009.