

Interactive comment on “Estimating the benefits of single value and probability forecasting for flood warning” by J. S. Verkade and M. G. F. Werner

Anonymous Referee #2

Received and published: 1 September 2011

The paper presents a method for the economic evaluation of the benefit of flood forecasting and warning systems. The approach is applied to a case study in the White Cart river basin. The paper is well-written and organized. It deals with an interesting topic for the hydrological forecasting community and I recommend it to be accepted for publication. Given the comments already posted by the first reviewer (with which I fully agree) and the answers of the authors to them, I have only two additional main comments I would like to be addressed by the authors in the preparation of their final version (besides some minor comments suggested at the end).

C3762

Main comments:

- The methodological steps the authors adopted for the combination of the EAD and the REV methods are interesting and the authors show scientific rigor when developing their approach and considering the hypotheses underlying such a complex issue. It is also interesting to note that attention is paid to the "practical" aspect of their developments, when the links to potential operational application of their methods are discussed. However, I have a concern about the limits of the conclusions drawn from the case studied. For acceptable reasons, the authors apply the approach on re-forecasts under "perfect scenarios", i.e. based on observed precipitation used as "forecasts" to force the hydrological model. This is clearly mentioned by the authors (including in their response to the first referee). Although I am positive that it is a reasonable assumption to illustrate the method, I am not so sure that conclusions based on the behaviour of such type of forecasts can be extended to those one would get if weather forecasts actually provided by a numerical weather model were used (whatever model it is). This is particular the case when the impact of increased lead times is considered, since at lead times longer than the response time of the catchment, uncertainty in precipitation forecasts will play a significant role, and, besides, a variable role according to the potential flood event that is being predicted by the forecasting system. Usually when we move from the "perfect scenario" (situation that in practice never occurs) to a "forecast scenario", things start to go wrong (or to behave strangely. . .). I would thus like to know how the authors expect the impact of using real "forecasts" would be. In their response to the first referee, the authors say that they expect that "this has no effect on the (. . .) difference between the four cases". It is probably so, but, within a case, when conclusions on the economic performance of the system according to lead times are drawn, wouldn't it have an impact? I think that this point should be emphasized in the text and discussion. Also related to this aspect, I would like the authors to discuss the fact that the "probabilistic forecasts" used are the result of the "dressing" of the deterministic "perfect scenario". How does this interdependency affect the results (if it does!)? This same point makes me wonder if the title of the paper is accurately reflecting the main

C3763

message of the authors' study. The use of "single" and "probability" in the title shifts the attention to the comparison between deterministic and probabilistic forecasts, which, in my opinion, is less the key point of the study (its main contribution is, in my opinion, the "economic evaluation of the benefit" of flood forecasting and warning systems).

- My second point concerns the length of the paper. It is clear that the authors made efforts to be clear and the result is a very didactic manuscript. However, for a scientific paper, I think that it is too long and some parts could be reduced to make the reading more agreeable. A reduction in the discussion and conclusions sections, together with the use of bullets for the main points addressed, would also help to fix the main ideas the authors want to convey as the result of their study. Some suggestions of where cuts could be made are: line 4, page 6645; lines 4-10, page 6646; lines 3-6, page 6647; lines 7-8, page 6648; lines 11-12 and 14-15, page 6650, lines 10-14, page 6650; lines 16 and 27 on page 6651 are repetition; line 21, page 6652; lines 5-8, page 6654; lines 17-23, page 6655; lines 14-20, page 6659; line 26, page 6660 to line 11, page 6661 (cut or summarize and include just the essential information in the section describing the study area); lines 4-14, page 6662.

Minor comments: (L = line, P = page) - L8, P6643: check typing of 'conclusions' - L13, P6645: consider changing to 'The starting point of the analysis. . .' - L20, P6645: 'can be easily established' - L13, P6648: 'The Relative Economic Value. . .' - L19, P6648: '(5) in (6) yields' - L12, P6649: 'may be estimated' - L5, P6650: '...of a FFWR is different. . .' - L15, P6650: consider simplifying the title to '2.5 Case study: White Carf river basin' - L4, P6652: consider simplifying the title to '3 Results' - L2-5, P6656: consider reviewing the sentence - L10, P6658: consider changing to 'results in probability thresholds' - Discussion: consider reducing it and presenting the essential points with bullets. - L20, P6657: 'As such, it also adds. . .' - L19, P.6659: take out 'as' - review using 'White Carf river basin' instead of 'Water Carf Water' in the whole text.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6639, 2011.

C3764