

Interactive comment on “Real time drought forecasting system for irrigation management” by A. Ceppi et al.

Anonymous Referee #2

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General comments

This manuscript proposes the feasibility of implementation of a real-time forecasting system to be used in irrigation management (PRE.G.I.). The forecast system relies on a meteorological (WRF) and a hydrological component (FEST-WB). The authors state that the models were calibrated and validated using in-situ measurement as latent heat flux and soil moisture. I found the paper potentially interesting but there is a need for improve. One of my main concerns is how the model was validated. In figure 7 the authors states that the rainfall forecasts shows better skills for more extreme precipitation thresholds (100 mm), however this is not completely true if not misleading. As the Brier score is defined, the rarer an event it is easier to get a better BS without having any real improvement in the forecast skill. In this respect I would recommend

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the authors to benchmark the model with different metrics that take into account a reference forecast as the climatology or the persistence. Just to name one, this is the case for the Brier Skill Score (BSS, see Mason 2004). In this way, some of the authors' statements need additional justification. However I think the manuscript can be improved with additional analysis of results to support that the proposed system will be useful for such a forecasting system. Therefore I think that the article is of relevance for readers of HESS, I do recommend some revisions –listed below- which could make the paper more complete and better understandable.

Particular comments

Page 15812, line 14: The reference of a forecast of up to 30 days as a long-range forecast is not totally accurate. Usually long range forecasts are associated a periods from 30 days to 2 years. As the authors are discussing about forecasts mainly between periods beyond 7 days and up to 30 days I would recommend to refer to that as Medium-range or Monthly forecasting.

Page 15815, lines 16-18: Here is not clear the source of the temperature and precipitation data. Are an output from the WRF or is observed data? Why at every 2 days? And not 1 or 10 days? Then in the next sentence the authors state that the hydro model is initialized with observed data. Are referring to the same data from the previous statement? I do think that the entire paragraph need to be rephrased, please try to be more specific here.

Page 15815, line 19: The acronym ARPA should be defined here instead of in page 15820.

Page 15815, lines 22-24: “In addition to observed and forecasted data, the knowledge of scheduled irrigation dates are fundamental to calculate the irrigation water input over the experimental field of Livraga.” This is a general statement or the authors want to refer to the information used in the analysis? This sentence seems to be disconnected, please rephrase.

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Page 15819 eq (2): For me it is not clear how eq(1) becomes eq(2) and how the stress threshold is defined. A clearer link between the two equations is necessary. Please explain in more detail the meaning of RAW and TAW and their link with the stress and water surplus threshold.

Page 15819 lines 12-13: the values of 0.23 and 0.33 are intended to be incorporated in eq(2)? I can't follow the construction of this thresholds.

Page 1589 line 20: "some observed data are missing. . ." Please be more specific.

Page 15820 line 4: It's hard to see the contribution of the precipitation and irrigation separately. I suggest to use a stacked bar with two colors (one for each contribution) in the figures 2, 3 and 4 and enlarge the axis fonts -specially the horizontal axis- as it is difficult to read them in the printed version.

Page 15820 paragraph between lines 9-13: Is this paragraph referring to Figure 3? If yes, I would recommend to swap this paragraph with the next in order to present the Figure first.

Section 4.2. I feel that this section could be reorganized and addressed in a better way. For instance, some results of the performance metrics are presented first than the metric is defined. This is the case of MAE and MRE that are already depicted in the previous section 4.1. Also MAE values are presented but this metric is not defined at all in the text. The Nash-Sutcliffe index is used (lines 16-20 page 15822) before the equation is defined (eq-4). Also the acronym related with this index should be homogenized (NS or ENS, or are different things?). I suggest to present first the performance metrics used. Sections 2 and 3 can be merged as a section named as data and methodology where all the metrics can be defined. Or preferably this metrics can be defined separately in an appendix.

Page 15821 line 27: To be consistent with the reference list, Jolliffe, 2003 should be Jolliffe and Stephenson, 2003. Please check the citation or reference.

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Page 15824 line 2: It's not clear to me why the thresholds of 50 and 100 mm are equivalent? Please explain or rephrase.

Figure 7. In my opinion this is one of the weakest points of the paper. The authors states that the greater skill is observed for the forecasts of the extreme events. These results obtained in such a short period are only an artifact of the methodology used to assess the skill. I would recommend the authors to assess the skill of these forecasts by using other metrics that take into account reference forecasts like the climatology as a benchmark.

Page 15824 lines 19-21: This sentence is a little bit cryptic. Please consider rephrasing it.

Page 15824 lines 22-26. After reading this sentence, I'm not 100% sure if I understand how the probabilities were computed. The number of ensemble members exceeding the threshold is a daily value or it is accumulated over 30 days? Please explain.

Figure 8: What are the meaning of the yellow circle and the 60% value? Please add a clarification in the caption.

Page 15825 lines 11-14: The authors state that "The two irrigations scheduled on 29 June and 14 July 2012 raised significantly soil moisture values in the following days above the water surplus threshold as shown in Fig. 10." However Figure 10 is showing the observed accumulated rainfall and forecasts. Are the authors referring here to Figure 9?

Figure 9: Some of the text of this figure is in Italian and some in English. Please homogenize the language.

Figure 10: As far I understood the extra irrigation water is not affecting the rainfall forecasts but is a deterministic value that is systematically added to both observed and forecasted information. I think that adding here the water added for irrigation is not necessary and can hide the real magnitude of the differences between the forecast

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and observations. Also it can be helpful to see in the plots the 25 and 75th percentile as in figure 9.

Page 15825 lines 15-18: The authors state that “The comparison between the REPS-WRF model forecast and the observed value at Livraga rain gauge (leaving out the two scheduled contributions coming from irrigation which are known a priori) shows a good agreement during the central phase of the maize growing season.” How the authors drawn this conclusion? Is hard to see it from figure 10 as the magnitude of the irrigation is too high. Please consider to redraw Fig 10 with only the accumulated rainfall. Moreover, how the authors determine the good agreement? Is this measured somehow or is only a graphical estimation? Please provide further elements that sustain this conclusion, as this is one of the key questions.

Page 15826 lines 10-16: This paragraph is a quite general statement that is not supported in the paper. Moreover, I can't agree that the system presented in this paper “has a higher reliability in comparison with flood forecasting systems”, at least I can't found any evidence of that in the paper. Please consider deleting or rephrasing this paragraph as in the present form is not completely accurate.

The reference to the Wilks, 2006 citation (Page 15821 line 26-27 and Page 15823 line 15) is missing.

References: Mason, 2004: On using “climatology” as a reference strategy in the Brier and ranked probability skill scores. *Mon. Wea. Rev.* 132(7), 1891-1895.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 15811, 2013.

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