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Hydrology and Earth System Sciences Discussions

Interactive comment on "Recent developments in predictive uncertainty assessment based on the model conditional processor approach" *by* G. Coccia and E. Todini

Anonymous Referee #2

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General

The paper addresses several aspects: First it analyzes the issue of predictive uncertainty (PU) in the context of operational forecasting and presents an overview of current approaches on PU assessment, such as the Hydrological Uncertainty Processor, Bayesian model averaging, quantile regression .

The authors again present the model-conditional processor, which was introduced in Todini (2008). In this paper they use the output of multiple hydrological models to obtain a distribution on the discharges, conditional on model output . Subsequently

they derive the PU in terms of a Bayesian formulation by i) applying a Normal Quantile Transformation to transform a distribution into the normal space, and ii) by formulating a multivariate normal conditional distribution between observed stages/discharges and modeled stages/discharges.

Using a the normal distribution as a basis enables the authors to formulate Bayes' theorem and to obtain a mathematically tractable expressions for the conditional densities of the PU. The authors correctly acknowledge that a single expression of the normal joint distribution is not adequate to represent extreme discharges over the entire range of possible values (from low flows to high flows) and resort therefore to truncating the distribution and using different joint normal expressions fitted to restricted data intervals separatedly (Truncated Normal Distributions). The truncation of the distribution and the verification are the actual innovative aspects of the paper with respect to Todini (2008)

The authors apply the model first to a single-model situation, and subsequently extend it to a number N of models, which causes the dimensionality of the problem to increase accordingly. The example reported in the paper refers to a three model case, the Topkapi model, the Thetis model and an ANN model.

Comments

The paper constitutes an advance in addressing the assessment of model uncertainty and using hydrological models as prior information. The problem is correctly posed in terms of evaluating the predictive uncertainty (Krzysztofowicz 1999, Raiffa and Schlaiffer). The authors emphasize the necessity to use hydrological models only as a virtual reality (pseudo-observations) to condition the probability of occurrence of the predictand (e.g. the water level) . Once the probability of occurrence of an event, conditional on model forecast is known, a cost function, expressing the potential damage, can be evaluated and effectively be used by the decision-maker.

In general I find the paper poorly written. Some sections cause confusion for the reader and are very difficult to follow. Others repeat information already presented much more clearly in Todini (2008).

Section 1.2 “Probabilistic threshold paradigm” is too wordy and an effort should be made to formulate the concept more concisely and clearer, using shorter sentences and/or expressions. A reference to a conference paper does not seem to be appropriate (Todini and Coccia 2010). The statement “dramatically changed the deterministic threshold paradigm” is overstated, as I find it overstated to talk about a “paradigm” in this context.

One of my major points of critique is the use of discharge as a forecasting variable in place of the water level, especially as they show “water levels” as critical variable in the explanatory figure 2. Discharge is a derived quantity calculated from the observed water level as primary variable. In my opinion working with observed water levels and transforming the output of hydrological models (Q) into levels reduces the risk of introducing spurious uncertainty with respect to doing it the other way around.

I do not find it appropriate to present the quantile regression (2.4.1) after introducing the model-conditional processor. I should instead present it before and refer to it afterwards only, ie. Section 2.1.4 should follow Section 2.2. The way it is done (in the middle of the MCP description) is confusing.

I absolutely disagree with the authors view that the HUP concept cannot be extended to multiple models as a justification for using MCP.

The authors should explain why the approach is to be considered “Bayesian”, also if it is called that way in similar contexts such as Bayesian Model averaging. A pure Bayesian approach expresses a posterior conditional probability of a hypothesis A given B in terms of a prior probability of A, the prior probability of B an a conditional probability of B given A. It is not clear to me how Eqn (7) represents an application of Bayes theorem in this sense, as no prior information is being revised, except perhaps the model output being used as prior information.

I suggest to the authors to use a more concise and clearer mathematical notation in 2.3 and 2.4.3 and the Appendix. There is significant room of improvement, while saving space in the presentation and enhancing clarity (see. e.g. Krzystofowicz 1999).

Minor remarks

On page 9229 line 22 “Weibull plotting position” should be explained better.

Page 9237: line 20: “compared each other” should read “compared to each other”

References

Krzysztofowicz, R. (1999). Bayesian theory of probabilistic forecasting via deterministic hydrologicmodel, Water Resour. Res., 35(9), 2739-2750.

Todini E (2008) A model conditional processor to assess predictive uncertainty in flood forecasting Intl. J. River Basin Management Vol. 6, No. 2 (2008), pp. 123–137

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