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Hydrology and Earth System Sciences Discussions

Interactive comment on "*HESS Opinions* "Ensembles, uncertainty and flood prediction"" *by* S. L. Dance and Q. P. Zou

Anonymous Referee #3

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The paper overviews some recent work on the issue of uncertainty analysis in flood modeling. As pointed out by the authors, this is a topic that received a lot of attention in the published literature and from many different angles.

My first concern with the paper is that it does not really give a sufficient depth of overview. Many important papers, and indeed entire research directions, are not mentioned at all. The authors seem to focus on NWP, but there is a lot of non-NWP work directly relevant to the topic of the paper. I have to admit I found the Supplement by Pappenberger to provide a far better review of international initiatives and literature than the Opinion Paper itself.

My second concern is that the authors are not really proposing any solutions, at least,

no new solutions. Nor are they stating any distinct or new opinion with regard to what existing solutions could be applied. As a result, this is hardly an opinion paper. The authors outline a list of challenges, but in my opinion these are well known, well recognized, and have been treated or are being treated in significant depth in various previous publications.

The manuscript, in attempting to combine an overview and some kind of discussion, becomes quite cursory. This is so for even the relatively technical content. For example, the section on parameter errors is rather superficial, even trivial. It is of course impossible to estimate a parameter that the model is insensitive to, but it also follows that not estimating this parameter will have little effect on the predictions. There is more to this type of equifinality than an insensitive parameter, and the practical problem is much more than not being able to refine a parameter through assimilation – but this is not discussed at all. Next, they recommendation to consider a reparameterization of the model is of course reasonable, but, firstly, hardly new and, secondly, far from easy to carry out in practice for complex models. In the discussion of Bayesian approaches, the key problem/task seems not mentioned – better likelihood function (and the related debates on multi-objective calibration), difficulties in obtaining/eliciting priors, etc.

The section on model structural errors: many recent developments in hydrology not mentioned, including various methods for statistical representation of these errors, work on state-dependent parameters, biases corrections etc.

Section on sampling: no discussion of MCMC techniques?

The paper would perhaps read better if, instead of limiting itself to an (incomplete) overview, it gave itself a better defined aim (and a narrower scope) and made stronger more detailed statements (even if just representing the opinion of the authors – that is the very point of an Opinion Paper!) on how to overcome the problems they survey. Otherwise we are left with a rehash of quite well known challenges and a unfortunately vague "wish-list" type responses. For example, in sections 4.1 and 4.2, the authors asks “How can we achieve observability and identifiability for parameter estimation with current and future models?” and respond “Designers of model parametrization schemes should take into account issues of identifiability and observability”. This is certainly hard to argue with, but in my opinion these kind of “truisms” are not really helpful and, do not really advance or “encourage debate about the most important future directions for research” as aimed by the authors.

In responding to one of the reviewers comments that the paper is too uncontroversial, the authors refer to a lengthy 15-page comment by other reviewers. Yet the comment by Pappenberger et al was not raising any controversies with the opinion paper, just listing lots of recent work that was already addressing the many issues that this paper deals with (but did not mention). These warrant discussion in any possible revision.

In my opinion a major revision, including a better structure of arguments, a better literature review, and the presentation of a distinct new opinion or (perhaps speculative) insights, is needed here. Otherwise the article will not really contribute to a debate on the topics it tries to debate.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3591, 2010.