Hydrol. Earth Syst. Sci. Discuss., 7, C3287–C3292, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C3287/2010/

Hydrology and Earth System Sciences Discussions

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

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Received and published: 1 November 2010

Response to Referee 3

We thank the referee for their detailed comments and we respond to the points raised below.

"...My first concern with the paper is that it does not really give a sufficient depth of overview.... The authors seem to focus on NWP, but there is a lot of non-NWP work directly relevant to the topic of the paper."

Ensemble methods are being applied to predictions on a range of timescales from climate timescales to a few hours, in a range of different types of model in meteorology,

oceanography and hydrology. It is our belief that a great deal of progress could be made if the scientists in these individual disciplines were aware of developments in each other's fields, were able to abstract the ideas from the applications and apply these ideas in new areas. In writing this paper we have attempted to identify a set of common questions and approaches that could be employed to understand better the propagation of uncertainty through chains of coupled models in meteorology, oceanography and hydrology. To ensure that the paper remains accessible to readers from each discipline, we have tried to give an overview of the problem using informative examples from each area rather than in–depth coverage. We believe that this is appropriate to an *opinion* article –it was not our original intention to write a full review paper.

We hope we have provided a balance of examples from different disciplines in the paper, although since NWP is the area that has pioneered ensemble predictions, it is perhaps inevitable that there are a larger number of examples given from this field. We agree that the comment by Pappenberger et al has given a very nice overview of developments in hydrology, and we would of course incorporate the suggested references, as appropriate, in the revised paper. We will also include additional review material and references from coastal science and oceanography.

"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.... 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."

We disagree with the referee, although we acknowledge that perhaps in our desire to be succinct, the detailed implications of the challenges and solutions were perhaps underemphasized.

Regarding the solutions we proposed, we have explained some of these in more detail in our response to referee 1. We hope this more detailed response makes the dis-

tinctiveness of our solutions more apparent. Regarding the novelty of our solutions, we agree that the essence of these may not be entirely new in one of the fields but it is new in another field. What we are promoting here is an interdisciplinary exchange, where solutions employed previously in one application may be adapted to another in a different field.

- We believe that many of the challenges we have posed are novel, although perhaps in different ways for each application due to different levels of maturity in each field. For example in section 3:no operational weather prediction centre currently takes account of observation error correlations in data assimilation

- We only know of two research papers that use data assimilation techniques with real data in coastal morphodynamic modelling (van Dongeren et al., 2008; Scott and Mason, 2007)

- Ensemble modelling of complicated coastal bathymetric changes, such as the entrance of estuaries or sandbanks is the subject of current research (Reeve et al 2008).

- Several new satellite instruments have recently been launched providing important data for hydrology e.g., SMOS (soil moisture), high resolution SARs (urban flood extent, Mason et al 2009). In our opinion, the optimal use of such remote sensing data for model and prediction improvement requires data assimilation. Thus there is a pressing need for new data assimilation schemes appropriate to the models where we wish to use this data. Even when existing assimilation schemes are available, the optimal use of a new observation type requires significant research in terms of building observation operators, characterizing observation errors and observation processing.

"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, their 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."

In the discussion paper we clearly define the general meaning of observability and identifiability (lines 20-25 of p3597) and refer to other papers that provide a more detailed discussion. We mentioned an insensitive parameter as an example, rather than a generalization.

We agree that we should have provided a reference for our suggestion of reparametrization of the model (on line 26 of p2597). In a revised manuscript we would cite Janssen and Heuberger, 1995. We agree that reparametrization is not necessarily straightforward, however experience gained when trying to calibrate the model using the first parametrization may provide some hints as to where to begin. Another approach could be to use a mathematical "model reduction" technique to simplify a more complex physically based model so that it becomes suitable to use as a parametrization. We will add the above two sentences to the revised paper.

We mention Bayesian approaches on lines 6-8 of p 3597, in order to provide some additional context for the section. However, since the focus of the paper is on ensemble approaches, we do not think it is appropriate to give a detailed discussion of general Bayesian calibration techniques, but in the revised paper with refer the reader to the review article by Sorooshian and Gupta, 1995.

"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."

Since the focus of the paper is on ensemble predictions, we have limited ourselves to techniques for estimating the structural errors, and the ways that model structural errors can be accounted for using an ensemble. We agree that we have neglected references from the hydrological literature on multi-model ensembles, and we would

rectify this by inclusion of references to He et al., 2009, 2010; Pappenberger et al., 2008 in a revised manuscript.

We are unclear what the referee means specifically by "various methods for statistical representation of these errors." We agree that we have not mentioned state-dependent parametrization, so we will include a reference on this (Young, 2002) in the section 4 on parameter errors in a revised manuscript. We would regard most bias correction techniques as post-processing steps. We have excluded a discussion of post-processing from our manuscript, since this was not discussed in detail at the workshop, and thus we considered it beyond the scope of our paper. Nevertheless, it is clear that raw ensemble validation and verification is a required first step before such model output statistics can be computed and this is discussed in section 7 of our paper.

"in sections 4.1 and 4.2, the authors ask "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.

We have given a more detailed description of what we meant by these statements in our response to Ref 1. We hope that by providing more detail, the recommendation we have made will provide a "straw man" proposal for debate.

"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..."

We note that Pappenberger et al are not official referees for the paper, but are 10 international authors who submitted a lengthy comment on our paper. Ref 3 may not regard our paper as controversial, however it must at least be regarded as provoking a significant response from a number of well respected authors.

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