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Hydrology and Earth System Sciences Discussions

Interactive comment on "HESS Opinions "Classification of hydrological models for flood management"" *by* E. J. Plate

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I appreciate the interest in my paper by Prof. Blöschl, and I am sorry that I disappointed the sophisticated reader. Actually, I initially also had some misgivings about submitting the paper, which had been my Darcy lecture in 2005, but the editor felt that the material was interesting enough. And I agreed because my intention was not to present essentially new material, but to classify existing model types according to structure and application, as stated on p.4679. I had in mind some first approach to giving guidance for selecting or developing, in a given locality, a hydrological model for floods. This purpose will be stressed more in the revised paper. It bothered me that so many numerical "universal" models are in the market. Nature is much too complex to be pressed into a

uniform model, as is often done by practicing engineers.

Your comment on ensemble forecasting is justified, and I shall expand a little bit on that topic in the final paper. Actually, I had just published a paper on models for flood forecasting (E.J.Plate (2007): Early warning and flood forecasting for large rivers with the lower Mekong as example. (Elsevier Journal of Hydro-Environment Research. Vol.1 pp.80-94) in which ensemble forecasting was discussed to some extent, so I did not want to expand on this issue in this paper.

On flood frequency: this comment I do not understand. Flood frequency analysis in the classical sense is a very old and much used technology, and this is why I referred to it only in passing (on p.4690). But I made the very strong point that flood frequency analysis, due to its inherent sources of uncertainties, is never exact. But I also stressed that extreme accuracy is not required at the design stage. This point I thought I had made clear. Extreme value analysis yields acceptable discharge values, because even comparatively large deviations from the actual probability cause only small change in discharge. How will anybody ever be certain that a flood is the 100 year flood, or even the 1000 year flood? Yet I think that where statistical discharge data, especially long time series, are available, they should be used, because numerical rainfall – runoff models not only rely on statistical models for the rainfall also, but in addition they are subject to a number of additional uncertainties. Our statistical tools do not help much!

Operationally, the freeboard used in most hydraulic applications of flood hydrology more than sufficiently accounts for the uncertainty of the design. Of course I do not advocate to work without models, but it is a fact that in most areas of Germany (and I assume also of other parts of Europe and elsewhere) good protection works have been constructed over the ages without the use of models. More critical is that by requiring safety against the 100 year flood (or a similar criterion) water managers want to have legal security, and this actually should imply that the method by means of which the 100 year flood is determined, should also be made part of the legal requirement. The US IACWD recommendations are consistence in this sense, as they require the

log-Pearson III distribution with regionalized fixed skew coefficients. This point I shall stress in the revised paper.

The statement that " .. any forecast model may be used that yields acceptable results" is seen from the operational view. By making this statement I did not intend to say anything about the model to be used for the forecast, so I included a qualification in the text in order to make sure that models not mentioned are not excluded. If a physical model including rainfall distribution or snow melt yields better results than a conceptual model, so be it. Important is that forecast models need frequent recalibration, and this is true for any model, and no model that I know of can be adapted reliably to a changing situation without calibration. In fact, our experience with a Mekong Flood Forecasting Model used by the Mekong River Commission up to this year was a conceptual model, which had lost its calibration long ago and yielded entirely unsatisfactory quantitative results.

I agree that decision criteria for the success of a forecast should be based on its role within the early flood warning system of a locality. I touch upon this point in the (very short) discussion of the Flood Management Cycle Fig.1. On this I have published extensively, i.e. my cited paper in the Journal of Hydrology. I put the criterion Eq.6 into the paper because I usually find that in papers on hydrological forecasting (see for a recent example Bravo et al., 2009: Incorporating forecasts of rainfall in two hydrological models used for medium range streamflow forecasting, J.Hydrological Engineering, ASCE, Vol.14, pp.435-445) the criteria used are Nash Sutcliffe, variance ratio with and without forecast and other statistical parameters (minimum least squares for model fitting etc.). The point which I wanted to stress is that all these statistical criteria do not use the fact that forecasting models have the advantage of additional information not available for operational models: they start with a known initial value. This fact should be used in assessing the forecast quality – we find, for example, that a good value of the Nash Sutcliffe criterion value may perfectly hide a poor forecast performance. I fully agree that it is desirable to include other criteria into a decision model for the use of

forcasts, and to have this reflected in criteria for the quality of the forecast. However, I find little support for this in the literature.

Concerning differentiating between what is done and what should be done: By referring to the Flood directive at appropriate places in the text I have also made clear that while risk maps for an existing situation are state of the art and required by the FD, calculations of residual risk is a suggestion, not a commonly accepted practice. The reference to the Water Framework Directive and the Flood Directive will be clarified. I loosely translated from the German, and I quoted from memory. I meant the Flood Directive, but at the time of original writing, only the Water Framework Directive was available. I shall correct this and clarify the reference.

Finally, I have no problem with replacing flood event with flood peak, although some hydrologists would take exception to this, as we clearly mean the whole event (peak and volume and duration) when we speak of a flood.

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