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Hydrology and Earth System Sciences
Discussions

Interactive comment on "HESS Opinions: Advocating process modeling and de-emphasizing parameter estimation" *by* A. Bahremand

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I fully agree with the ideas expressed in this opinion paper. I also like the discussion "On the modeling and evaluation of hydrological processes" which provides a well reasoned call for "consistency and realism". Therefore, I am supportive of the publication of this paper. However, with my review I would like to stimulate the Author to provide a more optimistic view on the current state and practice of hydrology.

In fact, while reading the text I found myself in full agreement with the Author, by I felt that the paper is presenting a perspective that is overly pessimistic. It is certainly true that hydrologists are now much relying on optimization, like scientists in several other scientific fields do, but I think that optimization is in most cases used by keep-

ing the physical knowledge into account. For instance, reasonable bounds are usually set in the optimization algorithm for parameter values (as the Author mentions at page 12381), basing on the catchment behaviours and/or previous model applications, and in some cases parameters are assigned fixed values. Even sensitivity analysis is often applied by taking the physical behaviours of the catchment into account. If one browses the scientific literature, then several papers can be found presenting attempts to incorporate the physical knowledge into hydrological practice. An example that comes to my mind is given by the concept of flood frequency hydrology presented by Bloeschl and co-workers (Merz and Bloeschl (2008a, 2008b), Viglione et al. (2013)), who remarked how field surveys and preliminary knowledge provide a valuable support to peak flow estimation. I believe that it is very rare the case where models are optimized with a "blind" approach, namely, without performing any a-priori and/or a-posteriori check of the physical representativeness of parameter values. I think that a paper presenting unrealistic parameter values is unlikely to pass the review phase. In my experience as editor, I sometimes found examples of studies that presented unrealistic estimates, for instance for the hydraulic conductivity. If the inconsistency is evident, these papers are likely to be rejected without review, as physical realism is considered to be a necessary prerequisite to ensure that a model is working correctly.

However, I do see the danger of being misguided by the increasing efficiency and the easy application of optimization algorithms, and I do see the necessity of educating young scientists to always be realistic. For this reason I am supportive of the publication of this paper, but I would like to offer here below some suggestions to make the approach more optimistic.

First, it seems to me that the Author is only considering the two extremes of no-calibration and "blind" optimization. Actually, most of the technical and scientific approaches are taking a mid-way between these two extremes. For instance, manual calibration is frequently used, where the modeller applies a manual trial and error procedure to maximize the match between observed and simulated variables, by taking

a-priori knowledge on the modelled processes into account. Is the criticism of the Author refereed to automatic calibration only or to any kind of calibration? Is the author critical with respect to manual calibration? I suggest to clarify this issue in the text. Actually, the manual calibration may coincide with the workflow presented by the author at page 12380, but the manual trial and error procedure is not mentioned in steps (a) and (b). In my view, a third step may be introduced in the workflow, which is extremely important for the sake of refining the “reasonable” initial parameter values. This step is in my view a particular case of calibration. It is not automatic (and therefore one may say that it is not an optimization) but it is still a trial and error procedure.

Second, I do not agree with the sentence at lines 4-6 of page 12381: “(.... in my view, automatic calibration takes the hydrologist nowhere and does not contribute in a significant manner to the enhancement of hydrologic knowledge)". In this case as well, I think that the Author is overly pessimistic. We should not forget that automatic calibration has several advantages, including the possibility of checking several combinations of plausible parameter values that would be impossible to manually try. Moreover, automatic calibration is a very useful support to model diagnostic. Indeed, when the best parameter estimate of a model is physically unrealistic, then one may conclude that the model is not adequate. Such a conclusion can be reached only if an efficient (and possibly automatic) search for the best parameter estimates has been carried out.

Third, it seems to me that the Author is (not explicitly) implying that calibration may eventually be not necessary in hydrology. If this is not the case, I suggest to the Author to revise the text to make his opinion clearer. Conversely, if my interpretation is correct I would like to suggest to the Author to consider that a hydrological model is unavoidably affected by uncertainty (see, for instance, Beven (2002) and the discussion in Montanari and Koutsoyiannis (2012)). The presence of uncertainty means that a perfect model in hydrology may not be a realistic target (and therefore I do not think that the third option discussed by Bergstrom (2006) and reported by the Author at page 12379 is a feasible way forward). Uncertainty also implies that parameter values are affected

by uncertainty as well, and therefore calibration will always be necessary to find the best parameter values and their probability distribution. Calibration may eventually be performed with a manual trial and error procedure, or using alternative information with respect to observed data, but in my opinion is unavoidable.

In conclusion, my suggestions to the authors can be summarized with the three following items:

1) Clarify whether the Author is critical with respect to any kind of calibration and clarify if the Author is convinced that calibration (including manual trial and error procedures) may eventually be not necessary. If this is the case, I would like to invite the Author to consider how uncertainty can be treated, and how the probability distribution for the parameters can be estimated.

2) Discuss the advantages of automatic optimization, otherwise the reader may understand that it is useless.

3) In general, take a more optimistic approach with respect to the current state and practice of hydrological sciences.

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