Hydrol. Earth Syst. Sci. Discuss., 2, S600–S602, 2005
www.copernicus.org/EGU/hess/hessd/2/S600/
European Geosciences Union

Hydrology and Earth System Sciences Discussions

Interactive comment on "Spatial and temporal patterns of land surface fluxes from remotely sensed surface temperatures within an uncertainty modelling framework" *by* M. F. McCabe et al.

A. Montanari (Editor)

alberto.montanari@unibo.it

Received and published: 28 August 2005

I am glad that the comments by the Referees and myself seem to have provided useful suggestions to the authors. Their reply to the Referees' remarks is to the point and based on convincing arguments. I still do not have available the revised manuscript and therefore I am looking forward to reading in detail how the referees' concerns have been addressed.

I would to like to comment some points of the authors' reply. The observations I am providing here below should not affect the revision of the manuscript, which I consider an interesting contribution, and its evaluation. I just would like to provide some addi-

tional observations about this discussion that in my opinion is potentially interesting for readers of HESSD interested in uncertainty estimation.

I completely agree with the authors in recognising that GLUE has already a long history. I personally agree in considering GLUE a extremely significant approach. However, we should not forget that in the last years uncertainty estimation in hydrology has received a lot of attention by researchers. Other uncertainty assessment techniques were proposed, which have different behaviours with respect to GLUE, and GLUE itself has been partially revisited. Therefore, I believe that is becoming today extremely important to put any contribution about uncertainty estimation within the context, by specifying why a given method was preferred and how and why some choices within GLUE were made. It is certainly true that GLUE is well known, but I think we should take care that any study is self explaining, therefore providing full details.

For instance, in the present paper the authors made a choice that is new to me (see authors' comment: "The specification of a priori likelihood weights can also be incorporated for particular parameterisations or model predictions, allowing the rejection of a parameter set if it falls below a certain likelihood threshold, or if it is classed as non-behavioural. No such implementation of this has been employed here, with all model simulations and parameterisations given equal weighting").

In fact, it is usual practice within GLUE to reject some models as non-behavioural. This is not related to assuming a prior information (a priori likelihood weights or a priori probability distribution for model parameters). Even when a prior information is not available, one usually computes the likelihood of a modelling solution and rejects this latter if such likelihood falls below a subjective threshold (see Cameron et al., 2000). This has not been done here, where all modelling solutions were retained and used to compute the likelihood weighted prediction bands. I think it would be interesting to know why the authors decided to make such choice, that it is not common in GLUE applications.

HESSD
2, S600–S602, 2005

Also, I agree in recognising that it is indeed difficult to identify a proper objective function when calibrating hydrological models or when applying GLUE. However, Beven and Freer (2001) proposed a set of alternative likelihood measures that can be used. Some of them are statistically based and therefore account for correlation in the model residuals. I think it would have been interesting to know why a least squares approach has been selected, in view of its limitations and in view of the alternatives that are available today. I believe that the reason that led to selecting least squares is related to the inherent behaviours of the application. In fact, some likelihood measures cannot be used in view of the data availability and hydrological model that is used in the specific case study.

I understand that my comments above are strictly pertinent to uncertainty estimation (and therefore may be considered not completely relevant to the main subject of this paper), in view of my personal interest on this topic.

I would like to congratulate again with the authors and I am looking forward to the revised manuscript.

References

Beven, K. J., and Freer, J.: Equifinality, data assimilation, and uncertainty estimation in mechanistic modelling of complex environmental systems using the GLUE methodology, J. of Hydrol., 249, 11-29, 2001.

Cameron, D. S, Beven, K.J., and Naden, P.: Flood frequency estimation by continuous simulation under climate change (with uncertainty), Hydrol. and Earth System Sci., 4, 393-405, 2000.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 569, 2005.