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Hydrology and Earth System Sciences Discussions

Interactive comment on "Multi-model comparison of a major flood in the groundwater-fed basin of the Somme River (France)" *by* F. Habets et al.

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This paper provides an excellent multi-model comparison on the hydrologic simulations at the macroscale Somme River basin in France, by using four distributed or semi-distributed models (incluing two LSMs). The simulations span over a 18-year (1985-2003), which is long enough to lead to convincing conclusions. The 4 models were tested by comparing their simulations to observed hydrographs at 5 streamflow gauges as well as groundwater level averaged from several tens of monitoring wells. Moreover, the simulation of flooded areas was also nicely investigated by comparing it to the remte sensing images.

Overall, this is a fairly good paper. In my evaluation, it deserves to be published after

minor revisions. There are not too many published multi-model comparison studies, perhaps due to the difficulty in performing such type of work by single research group alone. Therefore, this paper has great value to be published on HESS, particularly given that many interesting physical insights and modeling issues were revealed and reported in this paper.

However, the authors are suggested to clarify some ambiguities in this paper before publication according to my following review comments. Also, the length of this paper can be shorten by more concised way of presenation.

On p.6137, line 18, modify the word "react".

On p.6139, line 10, modify "The both other ones". How about "Another two"? line 20, modify "developped" into "developed". (also p.6144, line 10) line 21, modify "..model, that solves.." into "model that solves".

On p.6142, line 7, "on an annual basiS"

On. p.6146, line 18, unit is not correct. line 19, change "interception" into "interception loss".

On ps. 6147 and 6148, Is the unit "m/day" correct? I doubt.

On p. 6149, line 9, How about " the coefficient of efficiency"?\

On p.6151, line 4, correct "diffrents" into "different".

On page 6149 and Fig 4: What are the reasons responsible for the biases of CLSM< in the summer of 1990-1991, and for the SIM for the summer of 1985-1989? This needs to be discussed. The same commets also apply to Fig. 5: Why these four models generate quite different behavioris in five sub-basins, as authours summzrized in te last pagragrah of page 6149? I am particularly interested in: why the model MARTHE show lagged behavioirs in aVRE sub-basin whiel other models do not? The

authors emntioned that "This is certainly die to the aptial differences in the simulatied functioning of the aquifer". more discussion to elaborate on this poitn would be very useful.

Are the two sub-plots (1998and 2001) in Figure 7 absolutely necessary? The difference between these two years is not easy to observed from the plots. I personally do not see the point to present them...

When the groudnwater contributions to riverflow during floods are discussed (for example, bottom of p. 6153 and middle of p.6156, and elsewhere throughout the manuscript), the following study is very relevant in additon to several ones that Authours have already cited:

Eltahir, E.A.B, and , P. J. -F. Yeh, 1999. “On the asymmetric response of aquifer water level to droughts and floods in Illinois”, Water Resources Research, 35 (4), 1199-1217.

In this paper, the interactions between topography (hillsope), water table position, and base flow were discussed based on the intepretation of long-term measured data set in Illinois. It demonstrated that at the regional (macro-) scale, the dependence between shallow groundwater level and baseflow is in general non-linear due to the seasonal intersection of regional water table with local topography..

On P. 6157, "Discussion" instead of "Discussions".

There are way too many redundant "the" used thoughout the paper. Instead of pointing out all of them (which is not practical!!), pardon me just use the abstract as the example:

On p.6136, remove one "the" on lines 7 (the flooded), 8 (..the surface...), 11 (the observed...), 13 (the deep...), 20 (the overflow...), 21 (the overflow...).

Finally, and perhaphs the most important and challenge issue to answer, how the calibration will change the simulation result and the findings? It is well-know that the problem of equi-finality in the parameters of all hydrologic models would make validation of model simulations rather difficult, if not impossible. Could the Authors please

comment on this point (The "robustness" of the findings) more or less in the end of paper?

On the last paragraph of p. 6157, the Authors stated that "as the temporal evolution of the water fluxes is deeply modified by the transfer in the unsaturated and saturated zone, the impact of the surface schemes is mostly hidden by the calibration of the UZ and groundwater parameters. This is an interesting finding, but this statement may be limted to the basins where groudnwater outflows dominates such as the Somme basin here. In this regards, the following paper might be relevant to cite:

Gulden, L. E., E. Rosero, Z. L. Yang, M. Rodell, C. S. Jackson, G. Y. Niu, P. J. -F. Yeh, and J. Famiglietti, 2007, “Improving land-surface model hydrology: Is an explicit aquifer model better than a deeper soil profile?” Geophys. Res. Lett. 34, L09402, doi:10.1029/2007GL029804.

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Additonal Referece: Yeh, P. J.-F., and E. A. B. Eltahir, 2005, “Representation of water table dynamics in a land surface scheme: 2. Subgrid heterogeneity”, Journal of Climate, Vol. 18, No. 12, pages 1881-1901

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