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Hydrology and Earth System Sciences Discussions

Interactive comment on "On the benefit of high-resolution climate simulations in impact studies of hydrological extremes" *by* R. Dankers et al.

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Response to the interactive comment by Anonymous Referee 1

(Hydrol. Earth Syst. Sci. Discuss., 6, C702–C703, 2009)

R. Dankers, L. Feyen and O.B. Christensen, 17 June 2009

Reviewer 1 maintains that neglecting lake storage and river regulation is a serious problem.

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Once again, we would like to emphasize that we do not disagree with the reviewer in principle, in the sense that we would prefer to include the effects of lake storage and river regulation in our simulations, if sufficient data were available at European scale. However, we would like to point out two things. Firstly, the only difference between the model experiments described in the paper is in the climatology coming from the regional climate model. The hydrological model setup was the same in all LISFLOOD runs. This means that all experiments are equally affected by uncertainties in the model structure and parameterization of the LISFLOOD model. The problem of the effects of lake storage and river regulation, as noted by the reviewer, therefore does not invalidate the main thesis of our paper, which is a comparison of the simulations using a different climatological input from HIRHAM.

Secondly, we mentioned that calibrating the model implicitly accounts to some extent for the effects of storage and regulation on river discharge. The reviewer argues this implies that some processes are not simulated correctly in the model. To be clear, in river basins that were strongly affected by river regulation the calibration procedure focused on reproducing the water balance rather than the actual streamflow dynamics. In these basins, the parameters that describe these streamflow dynamics were obtained by regionalization from nearby unregulated catchments. This means that even in heavily regulated rivers the model can be expected to reproduce the natural dynamics of the system reasonably well.

Furthermore, all hydrological models, and especially large-scale models such as LISFLOOD, have to make concessions to input data availability and resolution and represent many processes in an implicit, or conceptual way rather than in an explicit, or physically-based manner. In fact, if the required input data are not available (as is the case for lake and reservoir dimensions and operation rules at European scale), extending the model to include more processes does not guarantee an improvement in the simulation of river discharge but rather increases parameter uncertainty and model predictive uncertainty. By unnecessarily increasing the degrees of freedom in the cal-

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ibration process the problem of non-uniqueness of parameter sets is aggravated and potentially more bias is introduced in the parameter estimates (see e.g. Beven and Freer (2001) for a thorough discussion on non-uniqueness of parameters and uncertainty estimation in mechanistic modeling).

Finally, the reviewer notes that comparing the different RCM runs assumes that the internal stochastic component in the simulations is small compared to the boundary conditions. This is true, and we believe that the near-perfect agreement in river discharge we obtain in some basins (Nash-Sutcliffe coefficient of 0.8–0.9 or higher – see also Figure 3 and 5) proves this is a reasonable assumption. This is also consistent with previous studies; see e.g. Christensen et al. (2001). We propose to emphasize this more in the text if it is not sufficiently clear from the manuscript.

References

Beven, K. and J. Freer: Equifinality, data assimilation, and uncertainty estimation in mechanistic modelling of complex environmental systems using the GLUE methodology, Journal of Hydrology, 249: 11-29, DOI: 10.1016/S0022-1694(01)00421-8, 2001.

Christensen, O. B., M. A. Gaertner, J. A. Prego and J. Polcher: Internal variability of regional climate models, Clim. Dyn. 17, 875-887, 2001.

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