Hydrology and
Earth System
Sciences

Open Access

Discussions

Interactive comment on "The Wageningen Lowland Runoff Simulator (WALRUS): application to the Hupsel Brook catchment and Cabauw polder" *by* C. C. Brauer et al.

Anonymous Referee #3

Received and published: 24 May 2014

General Comments: Overall, the paper entitled "The Wageningen Lowland Runoff Simulator (WALRUS): application to the Hupsel Brook catchment and Cabauw polder" offers a nice comparison between two very different catchments in a similar climatic regime. The Cabauw polder experiences heavy influence of tile draining to ensure efficient runoff of water when the groundwater table is shallow. The WALRUS model shows flexibility to account for an influx of water into the catchment area from sources other than precipitation and the interaction and feedback that accounts, in part, for artificial drainage networks. Overall the work appears to be well done, although I found the structure of the paper to be an inefficient way to portray the good work that was

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

completed. In general, I would suggest 1) to separate out the methods applied in this study to its own section for an easy to see overview of your methodologies and 2) include additional discussion and references comparing the results of this work to others. Is the surface water-groundwater interaction enough to model artificial drainage networks? Had this been something traditionally absent in studies trying to model catchments with artificial drainage, which found poor results with overly simplistic models? What did we learn in this study that was not known in past experience running this model? The quality of the research is good, however the structure of the paper should be refined. It is for this reason that I recommend major revision. Specific Comments: Optimized parameter sets seemed not to be very behavioral. Meaning, the performance of the model was not sensitive to most model parameter values. This suggests interaction between parameters in the model, which hints at the problem of equifinality. This is briefly discussed, but I think it would be constructive to include a bit more discussion. It might be to answer a question such as "Could you constrain the model in the future to help alleviate this problem?" Why do we see the differences in parameter sensitivity depending on the objective function? This is a result that I would expect to see, however it would be good to offer a bit more discussion as to why you think this might be the case. Page 2097 Line 16-17 "Before 1988 the method of Thom and Oliver (1977) has been used and since 1989 the method of Makkink (1957)." This sentence is a bit awkward. Please revise to make your point clearer. Page 2103 Line 14 – Many dates are shown as time periods that have necessary time series data for calibration. However, it is not clear to me what time periods were actually used for calibration. Was there a warmup period to initialize states within the model? Was the time period used for calibration similar to that used in validation? If not, what might be the consequences of this (would a longer validation period cause a degradation in performance of the model over time, or would the variations between the time series used for validation be averaged out over time?)? Page 2115 Line 23 – 'are not physical' should read something like 'are not physically feasible.' Table 1 – ET is listed on the table, however it is not very clear if this is ETpotential or ETactual. Please make

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

notation regarding ET consistent with the rest of the paper.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 2091, 2014.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper