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Discussions

Interactive comment on "The importance of topography controlled sub-grid process heterogeneity in distributed hydrological models" *by* R. C. Nijzink et al.

Anonymous Referee #1

General comment

A methodology to incorporate sub-grid process heterogeneity within the grid based distributed hydrological model mHM is presented in the manuscript. The authors define three different landscape units derived from topography and parameterize processes within each unit separately under the assumption that each unit has distinct hydrological functioning. A multi-scale parameter regionalization scheme is implemented where parameters at spatial resolution of the data were regionalized as functions of catchment descriptors separately for each landscape unit and upscaled to the model resolution, where upscaling was done to each landscape unit within the model grid. The authors

also employed additional process constraints by imposing realistic ranges to seasonal runoff coefficients as well as relational inequalities in soil moisture between different landscape units. They evaluated their approach in terms of its ability to reproduce a range of catchment flow signatures. They also tested the transferability of their parameterization to ungauged catchments by using the parameterization derived from a set of catchments to other independent catchments.

Their results show that inclusion of sub-grid variability alone did not show obvious improvement in all tested catchments. Improvements were noted in catchments where landscape units show contrast and little improvements were noted where there is little contrast. Constraints on the other hand lead to marked improvement. Low flow signatures were particularly noted to be reproduced better when sub-grid variability was considered.

I find the experiment and the results interesting and the paper is well written. I feel it is worthy of publication. I only have a few points to make, which I believe the authors can address fairly easily.

Detailed comments

Sometimes, I find discussion of the findings contradictory and confusing. For instance, it is mentioned that the introduction of sub-grid heterogeneity leads to improvement in capturing the flow signatures related to peak flows in the low flow period. However, I find the explanation given on pages 13322 – 23 misleading. This explanation applies to only wetlands, but the authors also show a considerable improvement in performance for the urbanized catchment, Orge as well.

I miss a proper interpretation for the lack of improvement to the models ability to capture the autocorrelations of the flows when the proposed structural changes were introduced. Why does a simpler model respond faster and why should a model that responds faster lead to a better representation of the autocorrelations (page 13323, lines 7-10)?

I find the discussion on page 13325, lines 17-25 interesting. Why was it necessary to impose the constraints in Equations 4 and 5 in the first place?

Page 13328, last paragraph: Does the considerable improvement in model transferability due to introduction of constraints apply to all parts of the flow regime or only to low flow signatures?

Page 13329, lines 14-16, Why is it difficult transferring parameters to this particular catchment from other catchments?