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

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We would like to thank the Referee for the comments, which are highly appreciated. We will try to improve on the raised issues.

"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."

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To clarify on this issue, the urbanized Orge catchment, which showed a relatively poor performance in terms of objective functions, did indeed also show improvements for the low flow signatures. Even though partly urbanized, another large part of the catchment is still classified as wetland (see also Figure 1), where the upward seepage of water may be important. Thus, as we also state in lines 5-9 of page 13322, the more general objective functions over the full validation period showed a decrease in performance, as the relatively high fast flows are not represented sufficiently, but these flows dominate these metrics. The low flow signatures improved at the same time, due to the incorporation of sub-grid variability. In our view, this confirms how misleading objective functions calculated for the full validation period can be, as the peaks in the low flow period are better captured by the topography driven model. Nevertheless, this can go unnoticed when calibrating on more general objective functions.

"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)?"

We acknowledge that our discussion here may need further elaboration and, thus, this will be adjusted in the revised manuscript. Basically, the original model showed a quicker response, whereas the adjusted model delayed the signal more. Our explanation for this observation is that the adjusted model has more options, in terms of reservoirs, to store the water. Effectively, this could delay the signal. In case of fast reacting catchments like the Orge or Loisach, this means that the 1-day autocorrelation, which tells us something about the timing of the peaks, is poorly represented. Besides this model based explanation, the used data may have an influence on the representation of the autocorrelation. The E-OBS data is of rather low resolution (24 km by 24 km), what could lead to a low estimate of the precipitation in locations with a steep topography. When rainfall peaks have averaged out, discharge peaks may not

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be well represented either, again leading to a low performance for the autocorrelation. Nevertheless, this applies to each of the models.

"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?"

In our first hypothesis, more constraints should define the 'plausible' parameter space more, leading to more pronounced differences. The chosen constraints were relatively simple and easy to implement, and, at the same time, there was enough support from literature to confidently apply these constraints. Nevertheless, this appeared not to be true in all cases.

"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?"

The introduction of constraints led to a more general improvement in transferability. For example, in Figure 14d it can be seen that the transferability of mHMtopo with constraints compared to mHMtopo without constraints improves over the full range of signatures. Also Figure 14c shows this for mHM, even though the differences are smaller. We will clarify this in a revised version.

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

Apparently, the derived global relations, like the example in Figure 4, do not hold for this catchment. This could mean that this catchment is significantly different from all other four catchments used in calibration. This may well be, as this catchment is merely gently sloped with agriculture. The Loisach and Broye are more mountainous, whereas the Treene is very flat and wetland dominated. In nature, the Orge catchment should be relatively similar, but this catchment is strongly affected by urbanization. The use of more similar catchments as the Briance in calibration, could maybe improve the

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transferability to this catchment.

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