We would like to thank prof. Huub Savenije for his comments on our manuscript. We reply to the raised points below in italic.

The weakness of the paper is that there is no insight whatsoever in the hydrological model used. The references to Van Dijk 2010a,b are insufficient. This is grey literature about a model developed for Australia, which I cannot find on the web and which gives the reader no insight into the working of this model. There is also no indication whether this model (calibrated for Australian circumstances, I presume) would work for the Mississippi or the Rhine as well. Probably, it is a semi-distributed conceptual model that distinguishes between deep and shallow-rooting vegetation to represent the ecosystems. This may be a good and appropriate model for Australia, but without a description of the main characteristics of the model, the effect of scale in the hydrological response cannot be evaluated.

We will add more peer reviewed references of the W3RA model to the manuscript (Van Dijk, 2010b; Van Dijk and Renzullo, 2011; Van Dijk et al., 2012a). The main evaluation of the model on global scale is documented in Van Dijk (2013) in a Water Resources paper. The main description of the model code and parameters is given here (Van Dijk, 2010):
http://www.clw.csiro.au/publications/waterforahealthycountry/2010/wfhc-aus-water-resources-assessment-system.pdf
The model code is also open source and online available on Github:
https://github.com/openstreams/wflow/blob/master/wflow-py/wflow/wflow_w3ra.py

W3RA is a global model and therefore not specifically calibrated/tested for the Rhine and Mississippi regions. The model is indeed a semi-distributed conceptual model and it distinguishes between deep and shallow-rooting vegetation to represent the ecosystems (as mentioned in line 5 on page 5 in the manuscript). For an extensive description we refer to the documentation of the model, which presents all equations and parameters (see document above; Van Dijk, 2010). Furthermore, we would like to indicate more studies which use W3RA for analysing discharge at catchment scale (van Dijk, 2014; Beck et al., 2017; Schellekens et al 2017; van Dijk 2013), from which van Dijk (2014) and Beck et al. (2017) are published in HESS. We will edit the manuscript to ensure that the full description of the model and its source code is retrievable from the cited references.

The most crucial parameter in any hydrological model is the storage capacity that the vegetation has created in the root zone. Surprisingly, this root zone storage capacity is independent on the soil parameters, because the vegetation adjusts the rooting depth to the soil, so as to create sufficient buffer capacity to overcome drought. This root zone storage capacity is scale-independent and directly connected to climate (Gao et al., 2014b), can be applied globally (Wang-Erlandsson et al., 2016) and locally (Boer-Euser et al., 2016), outperforming traditional soil-based approaches. So instead of using soil information and rooting depth as the main (and highly unreliable) input to hydrological models, it would be much better –and much easier – to use the scale- independent climate-based root zone storage capacity as the key input. I am curious to hear the opinion of the authors on this issue.

We thank the reviewer for his comment. We agree that the rooting depth and root-zone storage (and crop factors) play an important role in hydrological models. We agree that this novel approach deserves full attention in the global hydrological modelling and therefore should be studied further. Application and testing of approaches developed by Wang-Erlandsson et al (2016) and others, or going to even more dynamical root density approaches (e.g. van Wijk and Bouten, 2001), is very interesting, in particular because of the scale independence . When reading the comments we realize we have not given full attention to these innovating topics on hydrological modelling and we will elaborate more on them in the discussion and suggest further work on it.

My assessment:
I do consider this a well-written and well-prepared paper, but I find the research question not very innovative: testing with an ill-described model (probably developed for local circumstances) whether a reduction of resolution results in better performance. Runoff processes are largely determined by climate, ecosystem and topography. A model that requires calibration of scale-dependent parameters is not suitable for such an exercise. Although I support the conclusion about the reduction of scale in the meteorology and the difference between the dominant rainfall bringing mechanisms in the Mississippi and the Rhine, I doubt the adequacy of the study on reducing the scale of the hydrological model. The authors apparently missed a considerable part of the debate on hydrological modelling, as

for instance presented in Hrachowitz et al. (2013), where these issues were summarized after an intensive debate among the entire hydrological community.

We thank prof. Savenije for the assessment. We are aware of the ongoing debate about scales and hydrological modelling, and we have sharpened our knowledge on root-zone storage capacity. We would like to emphasize that we do not claim that the approach taken here is the best. We have tested from a 'global modelling perspective' if scaling up the resolution will lead to better performance (going from 0.5 to 0.05 degrees). We have investigated if rescaled parameters, and only using high-resolution information we are certain about (topography and vegetation), at higher resolution would give better simulation results. This also allows comparing the outcome of the models in a fair and transparent way, which would not be possible or be very difficult otherwise, because the model itself would change. As concluded in the manuscript, we find that the improvements are limited and that likely other process representation (e.g. subsurface lateral flow) is needed, especially if we move to even finer resolution than 0.05 degrees.

The comments of Huub Savenije stress the challenges in the field of hydrological modelling, especially at different scales. We hope this manuscript can contribute to this discussion by showing the results of one approach to deal with different spatial scales, which we conclude is not the best method. We will include an extra paragraph in the discussion to put our approach in perspective with the challenges in hydrological modelling across scales, and the potential benefits of a different viewpoint, illustrated along the suggested references by the reviewer.

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