We would like to thank Referee Prof. Hubert H.G. Savenije for his interest in this topic and for the valuable comments to improve our manuscript. Based on the comments additional calculations have been performed in the revised manuscript. Our point-by-point response to the comments is given in the following (**Comments in black**, Answers in blue and the corresponding changes in the revised manuscript are marked in orange.):

First of all, I would like to mention that I find this an important paper. In my view, the authors have convincingly shown that their model is a very valuable addition to the set of existing global hydrological models. Its innovation is that it uses the root zone storage capacity (RZSC) determined independently by remote-sensing-based global products for precipitation and evaporation and does not calibrate obtain it by calibration. As far as I know all other global models either calibrate the RZSC or determine it on the basis of incomplete soil maps and inaccurate maps for rooting depth. The authors using climate-derived RZSC has the intuitive advantage that ecosystems apparently adjust their RZSC to climate variability by creating a buffer against dry periods. In hydrological models the RZSC is the key variable determining the partitioning of precipitation into transpiration, recharge and surface runoff, making it the most important hydrological parameter for land-atmosphere interaction and runoff generation. The fact that the authors demonstrated that a remote-sensing-based estimate of the RZSC can be efficiently used in a global hydrological model is nothing less than a breakthrough.

We thank the referee for this comment.

On top of this, the authors demonstrated in their validation that the NDII, a simple remote sensing based proxy for root zone moisture stress, is a powerful tool to validate (and possibly calibrate) global hydrological models. Highly sophisticated satellites claim to monitor soil moisture, with limited success (e.g. SMOS, ERS, and AMSR-E). But NDII, a readily available remote sensing product observing the moisture content of vegetation, apparently can do this better, because it connects to the root zone moisture tension and not the moisture content of the surface. (Sriwongsitanong et al., 2016).

We thank the referee for this comment.

Of course this paper is a modelling paper, and should be treated as such. In that respect I think that the authors should make the code of the model freely available and not merely on request. The model builds on earlier work by Gao et al. (2014a and 2014b), and by Wang-Erlandsson et al. (2014 and 2016), and I think it is fair that the software is freely made available so that other people can advance this approach further. In fact, I think that a more sophisticated evaporation module as in Wang-Erlandsson et al. (2014) could improve the model even further.

Thank you for the suggestion. We will make the code of WAYS model freely available after the manuscript is accepted. Moreover, we completely agree that the evaporation module in the STEAM model (Wang-Erlandsson et al., 2014) could further improve the WAYS model, because the former (Wang-Erlandsson et al., 2014) separates the evaporation fluxes in a more detailed way. We are also very interested in coupling the STEAM model with WAYS in our future work by cooperating with the authors of the STEAM model.

Authors' change in the manuscript.

Page 26, Line 9: (the changes is marked as blue)
The precise simulation of variables in the root zone could benefit the simulation of other elements in the model, thus advancing the model simulation toward an advanced philosophy, i.e., obtaining the right answers for the right reasons rather than simply obtaining the right answers (Kirchner, 2006). In addition, the WAYS model can be further improved by integrating a more sophisticated evaporation module, e.g., the STEAM model developed by Wang-Erlandsson et al. (2014), which separates the evaporation fluxes in a more detailed way. Finally, a runoff generation module recently developed by Gao et al. (2019), HSC-MCT, could provide another possibility to improve the WAYS model, as it offers another venue for determining one of the key parameters in WAYS (β) independently without calibration. This calibration-free module could actually benefit any conceptual hydrological model.

Having said this, the paper requires some (major) revision. I shall highlight the major points.

We thank the referee for the constructive comments. Below, we give a point-to-point reply to the comments posted by the referee.

1. The comparison in Figures 4 and 5 is not entirely fair. The models of the ISIMIP2a data set are not calibrated, whereas WAYS is. This is mentioned in the paper, but the comparison in these figures suggests otherwise. The caption should mention this.

Thank you. Actually, the simulated runoff of the WAYS model is first compared with the reference data ERA-Interim/Land runoff. The performance of the WAYS model in runoff simulation is evaluated mainly based on the comparison between ERA-Interim/Land data and WAYS simulation. Since WAYS uses the same driving data as ISIMIP2a models and the ISIMIP2a simulations are widely studied and discussed in many studies, we believe the additional comparison between WAYS and ISIMIP2a models can provide added-value for examining our model. Therefore, ISIMIP2a simulations are also shown in the results section together with the ERA-Interim/Land data.

We agree with the referee and have revised the captions of Figures 4 and 5 accordingly. We would also like to note that because Referee #2 suggested the use of ERA-Interim/Land data instead of reference data in the captions of Figures 4 and 5, these changes are also shown in the captions. In addition, in short comment #1 (comment 10), the reviewer suggested us to move the Figures 2 and 3 from the main text of the manuscript. Thus, the figures in the revised manuscript are re-sorted.

Authors' change in the manuscript.

Page 14: Figure 2 caption is updated

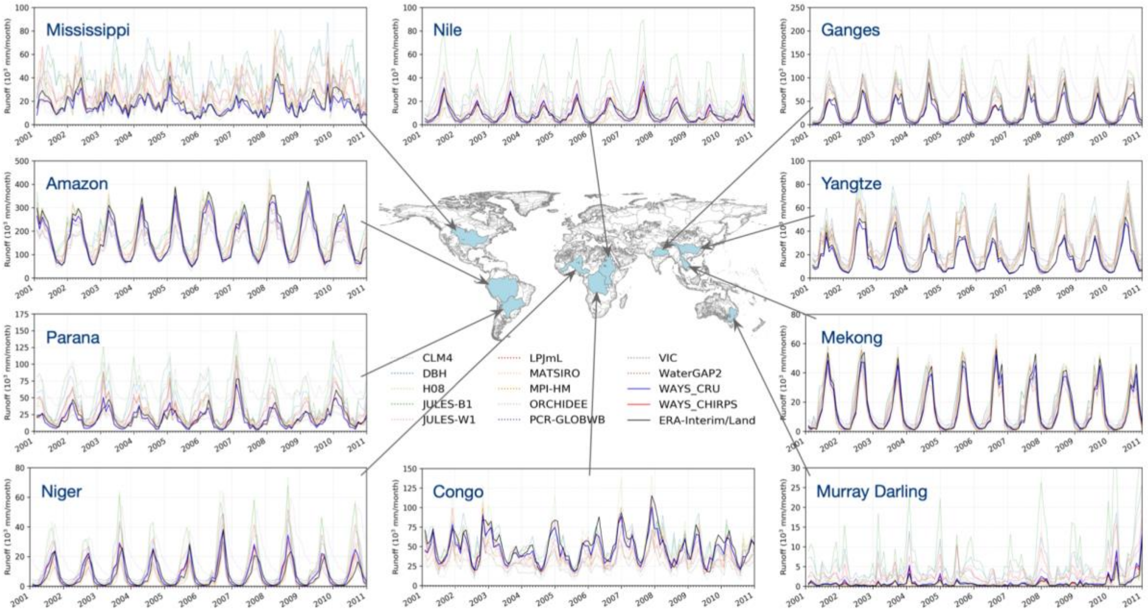


Figure 2. Time series of monthly runoff simulated by WAYS and the ISIMIP2a models, as well as the reference data. The basins highlighted in the world map indicate the selected catchments for model evaluation. The solid lines in blue and red indicate the WAYS simulations with two different RZSC products. The solid line in black indicates the ERA-Interim/Land data, and dashed lines represent the ISIMIP2a model simulations. In some plots, the red line is not visible and is covered by the blue line due to the small differences between the two WAYS runs. WAYS is calibrated using Composite Monthly Runoff data, while the ISIMIP2a models are not calibrated for the simulation.

Page 15: Figure 3 caption is updated

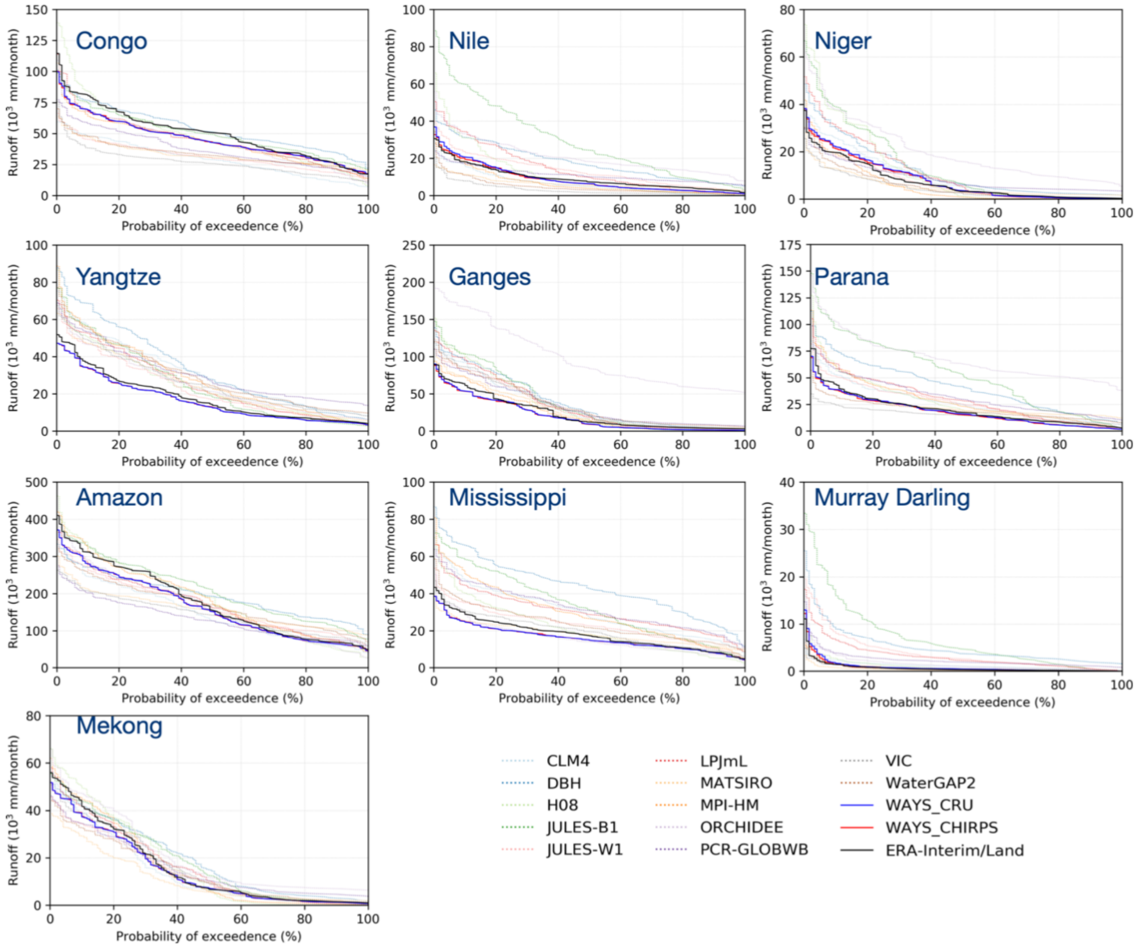


Figure 3. The probability of exceedance for monthly runoff simulated by different models as well as the reference data in ten selected basins. The solid lines in blue and red indicate WAYS simulations with two different RZSC products. The solid line in black indicates the ERA-Interim/Land data, and dashed lines represent the ISIMIP2a model simulations. In some plots, the red line is not visible and is covered by the blue line due to the small differences between the two WAYS runs. WAYS is calibrated using Composite Monthly Runoff data, while the ISIMIP2a models are not calibrated for the simulation.

2. It is important that the authors indicate which parameters are input independently (from what I can see: S_(rz,max), K_s, f_s, R_(s,max), S_(I,max)) and which are calibrated (I guess: Beta, K_ff, . . .). The fact that a number of these have been input as independently obtained parameters is crucial information, but we should also know which have been obtained by calibration. It is well known that there is equifinality between Beta and the RZSC, so this is not trivial. I would also want to see a Table with the calibrated values. There should be an openly shared data set with all parameters used, whether obtained by calibration or independently.

We agree with the referee’s comments and have inserted a table to describe the parameters that are used in the WAYS model as well as their ranges. WAYS has 13 parameters in total, seven of which are obtained from the literature and the rest (six parameters) from the calibration (see page 11, table 2 in the revised manuscript). We will share all the model parameters after the manuscript is accepted. Since the calibrated parameters are spatially varied, it is not appropriate to show them in tables. Here we provide the spatial patterns of two key parameters (β, C_e) that are calibrated, as these two parameters mostly affect the partitioning of precipitation (see Figure S21 and Figure S22). The rest of the calibrated parameters are uploaded to the response thread in terms of netCDF files (parameters.cn4) as Supplements.

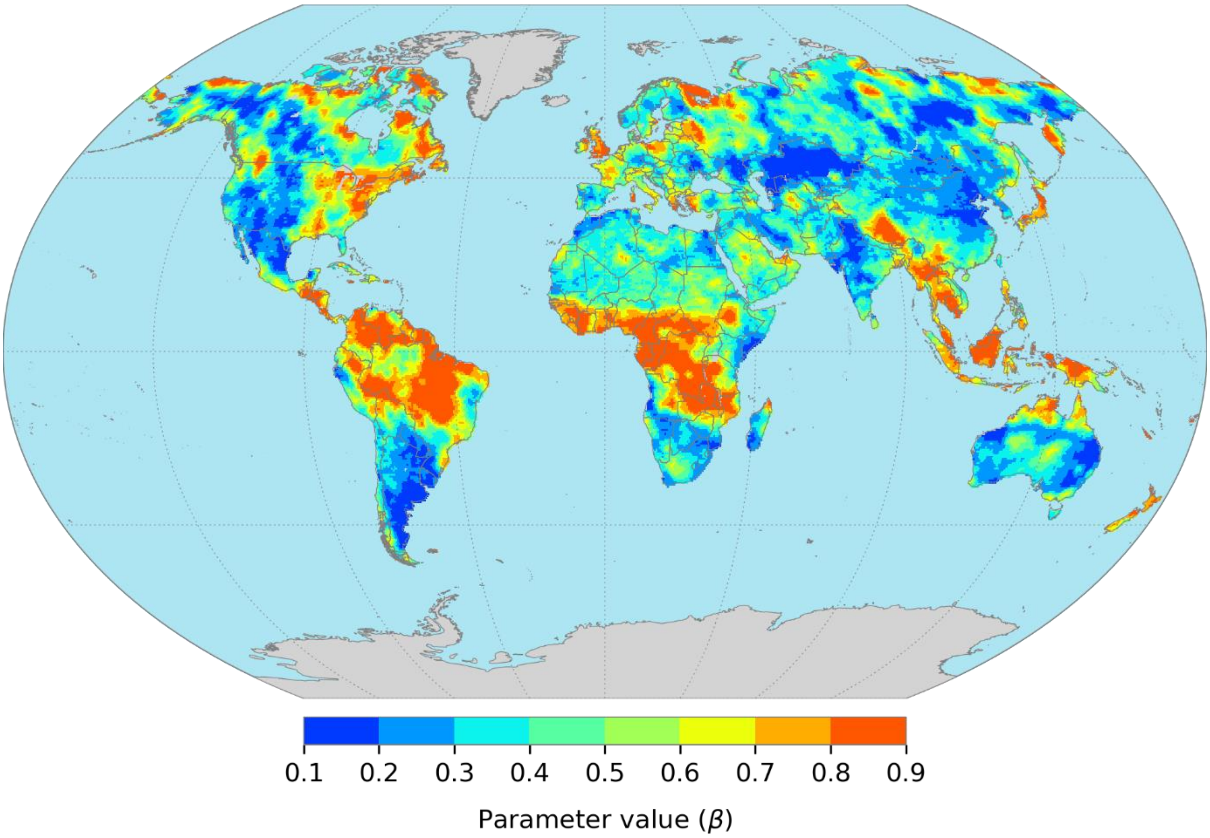


Figure S21. The spatial distribution of the model parameter β

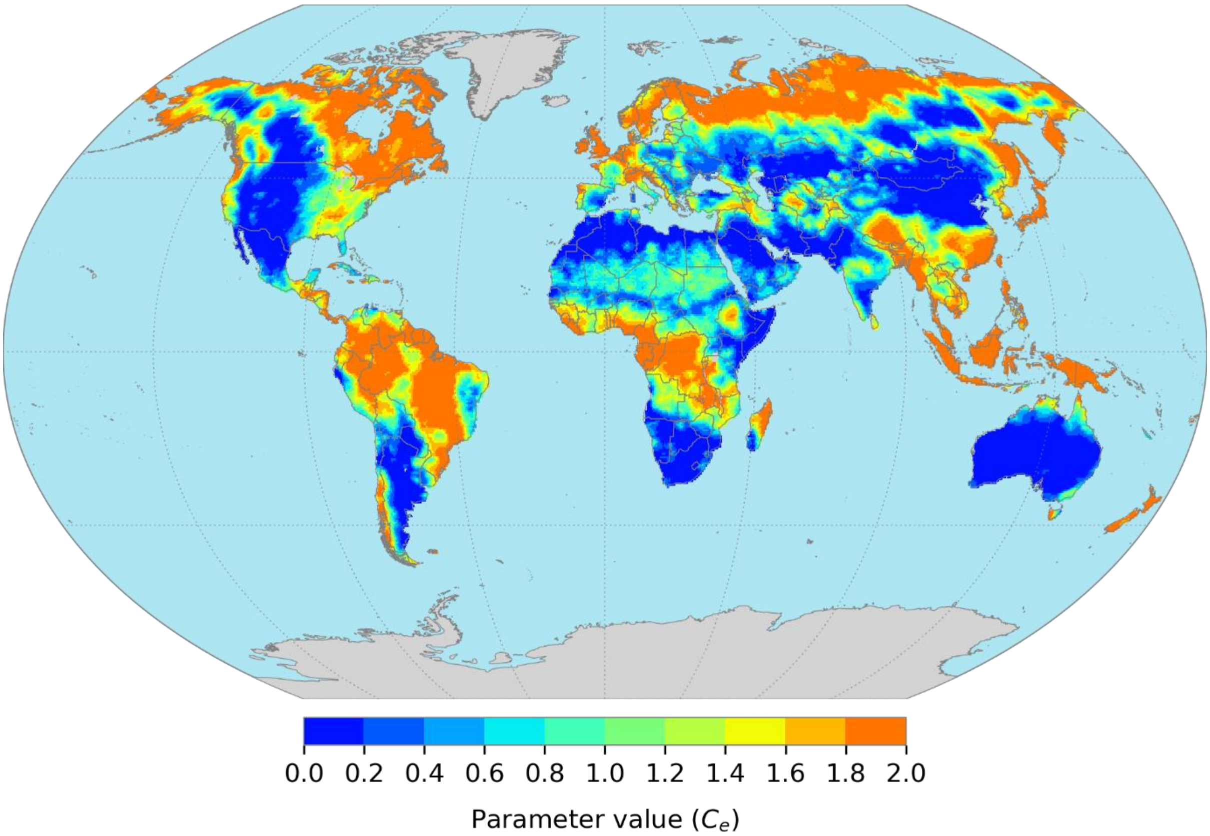
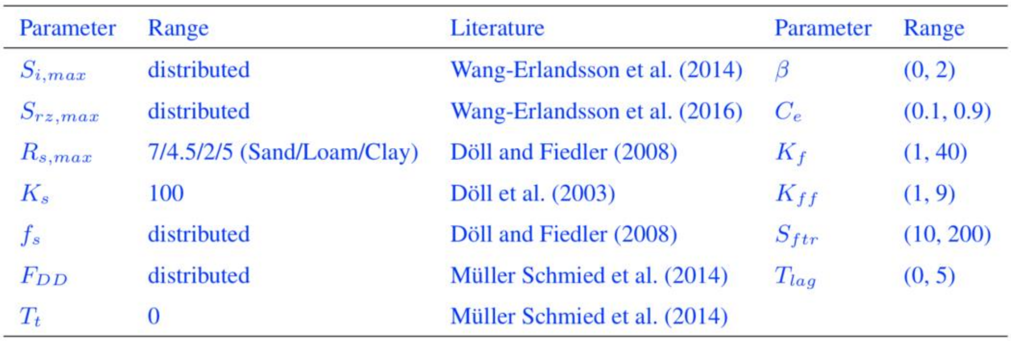


Figure S22. The spatial distribution of the model parameter C_e

Authors' change in the manuscript.

Page 12: A table is added

Table 2. Parameter ranges of the WAYS model



Parameter	Range	Literature	Parameter	Range
$S_{i,max}$	distributed	Wang-Erlandsson et al. (2014)	β	(0, 2)
$S_{rz,max}$	distributed	Wang-Erlandsson et al. (2016)	C_e	(0.1, 0.9)
$R_{s,max}$	7/4.5/2/5 (Sand/Loam/Clay)	Döll and Fiedler (2008)	K_f	(1, 40)
K_s	100	Döll et al. (2003)	K_{ff}	(1, 9)
f_s	distributed	Döll and Fiedler (2008)	S_{ftr}	(10, 200)
F_{DD}	distributed	Müller Schmied et al. (2014)	T_{lag}	(0, 5)
T_t	0	Müller Schmied et al. (2014)		

3. In my view, the Beta parameter is crucial. It affects the partitioning of precipitation into transpiration and runoff. The time scales K_s, K_ff and K_f merely affect hydrograph shape, but not the water balance. In this regard, it is interesting to know that Gao et al. (2019) developed a HAND-based method to determine Beta from independent topographical information. This method assumes that the dominant mechanism is Saturation Excess Overland Flow and therefore is not applicable on hillslopes. So it should be used with good judgement, but it offers another venue of estimating Beta independently without calibration.

We completely agree that parameter β is crucial as it controls the precipitation partitioning, thus mostly affecting the water balance. In addition, parameter C_e plays an important role in water balance control as it affects the evaporation and consequently influences the root zone water storage, which determines the precipitation partitioning. Indeed, the rest of the parameters (e.g., K_s, K_{ff} and K_f) are more important for hydrograph shape adjustment rather than the water balance.

We would like to thank the referee for sharing the recently published paper by Gao et al. (2019). The authors developed a calibration-free module (HSC-MCT) for runoff generation based on the Height Above Nearest Drainage (HAND) data. They found that the runoff coefficient can be ingeniously linked with the HAND-based area fraction, and thus β can be determined accordingly. This finding offers another venue for determining β independently without calibration. The HSC-MCT module can provide added-value for any conceptual hydrological model. We agree that it would be very interesting to integrate HSC-MCT into WAYS and that it can further improve the WAYS model. However, at the current stage, the main purpose of our work is to demonstrate the value of integrating a remote-sensing-based RZSC into a global hydrological model, especially for RZWS simulation. The integration of HSC-MCT could potentially introduce uncertainties into the model as HSC-MCT is currently only applicable to regions dominated by Saturation Excess Overland Flow. Moreover, HSC-MCT is heavily dependent on HAND data, and there are currently no available HAND data at 0.5 degrees. Self-derived HAND data could further introduce uncertainties as HAND is very sensitive to the drainage threshold and the open water elevation (Nobre et al., 2016). A thorough validation of HAND on a global scale is necessary before it can be applied in subsequent analyses. In these regards, we would like to skip the integration of the HSC-MCT module into WAYS at the current stage, but we will examine its inclusion in our future works.

Authors' change in the manuscript.

Page 26, Line 9: (the changes is marked as blue)

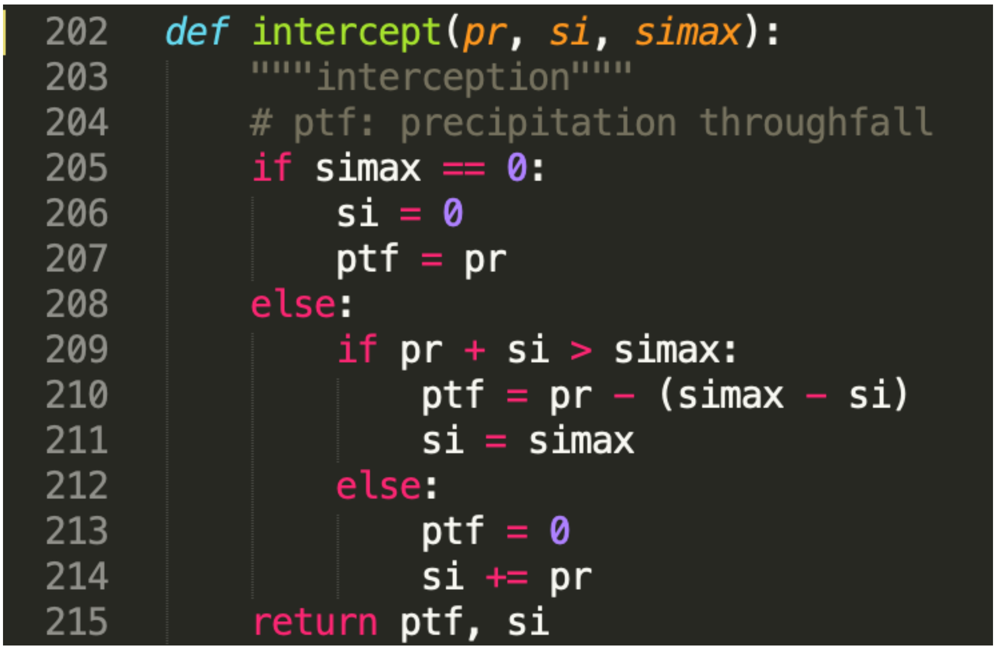
The precise simulation of variables in the root zone could benefit the simulation of other elements in the model, thus advancing the model simulation toward an advanced philosophy, i.e., obtaining the right answers for the right reasons rather than simply obtaining the right answers (Kirchner, 2006). In addition, the WAYS model can be further improved by integrating a more sophisticated evaporation module, e.g., the STEAM model developed by Wang-Erlandsson et al. (2014), which separates the evaporation fluxes in a more detailed way. Finally, a runoff generation module recently developed by Gao et al. (2019), HSC-MCT, could provide another possibility to improve the WAYS model, as it offers another venue for determining one of the key parameters in WAYS (β) independently without calibration. This calibration-free module could actually benefit any conceptual hydrological model.

4. I found a mistake in Equation (2). The correct equation should read: P_tf = MAX{0, P_r – (S_imax – S_i)/Delta t }. The Delta t is required to make the equation dimensionally correct and to prevent that if the model is used at another time step, no error is made. The MAX{0,x} operator is essential since P_tf is an overflow. Forgetting the MAX{0,x}

operator can lead to negative P_tf values for small amounts of rainfall. This may trigger relatively small errors, but particularly in wet environments (the Amazon?) this can create errors. I fear that the authors have to rerun the models to correct this mistake.

We thank the referee for pointing out the mistake in Equation (2), which has been corrected, and the codes have been changed accordingly (see Figure 1). As a result, the model has been rerun and the results updated. We would like to state that before rerunning of the model, parameter RZSC ($S_{rz,max}$) was also updated in the model based on the comment of Referee #3 (comment 6). Referee #3 suggested that RZSC should be updated by applying the Gumbel normalization, as Wang-Erlandsson et al., (2016) found that normalizing the RZSC using the Gumbel distribution by land cover type further improves performance. The model results are updated in the revised manuscript. In comparison to the previously simulated results, both simulations of runoff and RZWS are only slightly altered due to the correction of the precipitation throughfall equation as well as the update of the RZSC data. In the Amazon basin, the rank correlation between simulated RZWS and NDII are improved from 0.533 (WAYS_CRU) and 0.506 (WAYS_CHIRPS) to 0.593 (WAYS_CRU) and 0.552 (WAYS_CHIRPS).

WAYS is currently run on a daily scale, and the **Delta t** suggested by the referee is necessary for running the model at other time scales. **Delta t** is required not only for the precipitation throughfall equation but also for all the time scale-related parameters. Therefore, we have updated the table of model equations and stated the following at the end of the table: "Note: all time scale-dependent parameters need to be divided by Δt to make the equation dimensionally correct and suitable for any other time scales".



```python
def intercept(pr, si, simax):
    """interception"""
    # ptf: precipitation throughfall
    if simax == 0:
        si = 0
        ptf = pr
    else:
        if pr + si > simax:
            ptf = pr - (simax - si)
            si = simax
        else:
            ptf = 0
            si += pr
    return ptf, si
```

Figure 1. the codes for the precipitation throughfall (P_{tf}) calculation

Authors' change in the manuscript.

Page 8: Table 1 is updated. (the changes are marked as blue)

Table 1. Water balance and constitutive equations used in WAYS

Reservoirs	Water balance equations		Constitutive equations		Reference
Interception reservoir	$\frac{dS_i}{dt} = P_r - E_i - P_{tf}$	(1)	$P_{tf} = max(0, P_r - (S_{i,max} - S_i))$	(2)	-
			$E_i = E_p\left(\frac{S_i}{S_{i,max}}\right)^{2/3}$	(3)	Deardorff (1978)
			$S_{i,max} = m_c L$	(4)	Wang-Erlandsson et al. (2014)
Snow reservoir	$\frac{dS_w}{dt} = \begin{cases} -M & \text{if } T > T_t \\ P_s & \text{if } T \le T_t \end{cases}$	(5)	$M = \begin{cases} min(S_w, F_{DD}(T - T_t)) & \text{if } T > T_t \\ 0 & \text{if } T \le T_t \end{cases}$	(6)	Rango and Martinec (1995)
Root zone reservoir	$\frac{dS_{rz}}{dt} = P_e - R - E_a$	(7)	$P_e = P_{tf} + M$	(8)	-
			$\frac{R}{P_e} = 1 - \left(1 - \frac{S_{rz}}{(1+\beta)S_{rz,max}}\right)^{\beta}$	(9)	Sriwongsitanon et al. (2016)
			$E_a = (E_0 - E_i) \cdot min\left(1, \frac{S_{rz}}{C_e S_{rz,max}(1+\beta)}\right)$	(10)	Sriwongsitanon et al. (2016)
Slow response reservoir	$\frac{dS_s}{dt} = R_s - Q_s$	(11)	$R_s = min(f_s R, R_{s,max})$	(12)	Döll and Fiedler (2008)
			$Q_s = S_s / K_s$	(13)	Döll et al. (2003)
Fast response reservoir	$\frac{dS_f}{dt} = R_f - Q_{ff} - Q_f$	(14)	$R_f = R - R_s$	(15)	-
			$Q_{ff} = max(0, S_f - S_{ftr})/K_{ff}$	(16)	-
			$Q_f = S_f / K_f$	(17)	-

Note: all the time scale-dependent parameters need to be divided by Δt to make the equations dimensionally correct and suitable for any other time scales.

- in the reference column indicates that the formula is taken from the FLEX model.

Page 14: Figure 2 is updated

To avoid repetition, please to see the changes in response to comment 1

Page 15: Figure 3 is updated

To avoid repetition, please to see the changes in response to comment 1

Page 17: Figure 4 is updated

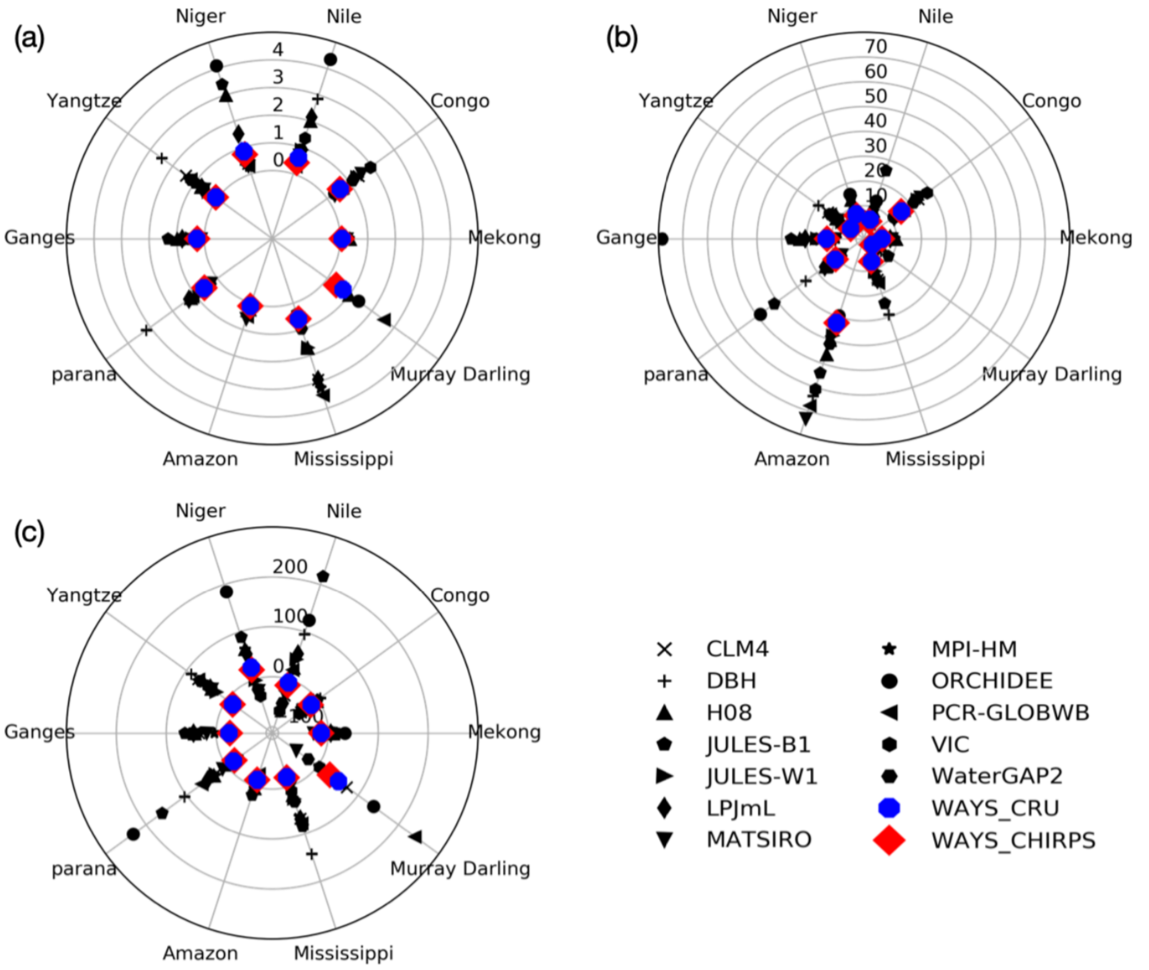


Figure 4. The catchment clockwise pole plot according to different metrics, (a) 1-NSE, (b) RMSE, and (c) PBIAS. Colored markers indicate the score for the WAYS model with two different simulations, and black markers represent the score for ISIMIP2a models. For all the metrics, the value of 0 is the benchmark.

Page 18: Figure 5 is updated

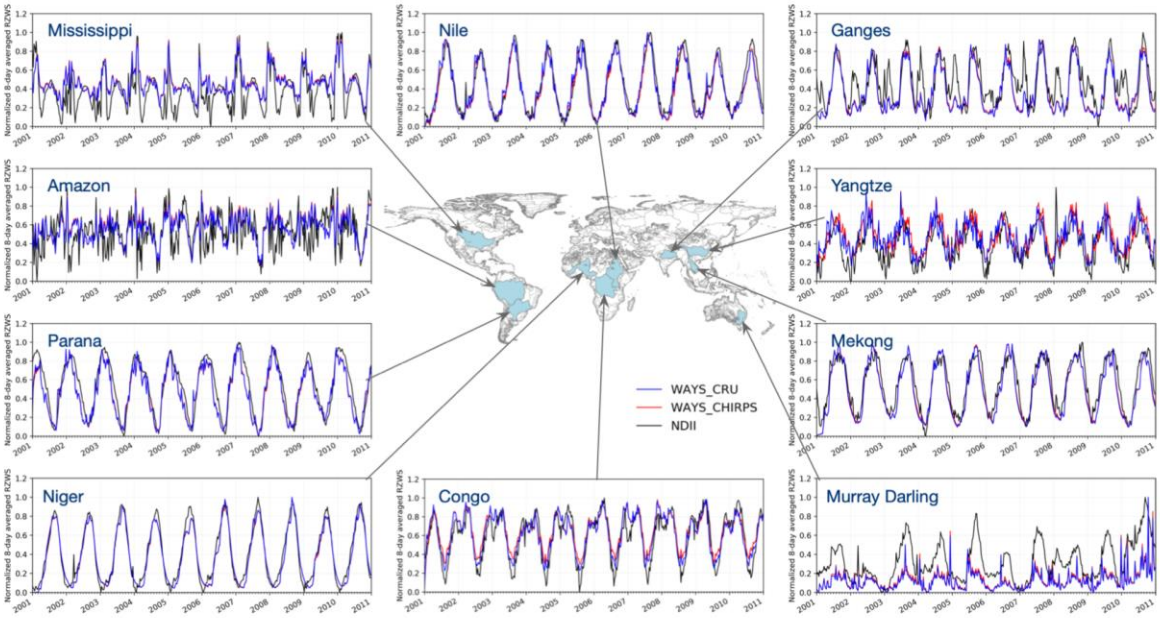


Figure 5. Time series of 8-day normalized RZWS simulated by the WAYS model and NDII value.

Page 19: Table 3 is updated

Table 3. The rank correlation of NDII and WAYS-simulated RZWS in ten selected basins

Selected River Basins	Models	
	WAYS_CRU	WAYS_CHIRPS
Congo	0.872	0.871
Nile	0.951	0.967
Niger	0.975	0.975
Yangtze	0.713	0.764
Ganges	0.803	0.817
Parana	0.931	0.934
Amazon	0.593	0.552
Mississippi	0.689	0.677
Murray Darling	0.614	0.636
Mekong	0.936	0.938

5. In the validation against NDII, one should realise that some ecosystems (particularly Australian) tap into groundwater, so that in those ecosystems the NDII may not be the correct proxy for moisture stress in the root zone during dry periods. This may be another reason why the Murray Darling performs less well in the comparison with NDII.

Thanks for the comment. We agree with the referee and have mention this in the manuscript.

Authors' change in the manuscript.

Page 20, Line 8: (the changes is marked as blue)

In contrast, WAYS shows a trend of underestimation in the Murray Darling. A possible reason could be that deep rooted plants are widespread across the Murray Darling basin and can tap into groundwater (Runyan and D'Odorico, 2010; Lamontagne et al., 2014); thus, the NDII may not be the correct proxy for moisture stress in this region. A vast amount of groundwater drawing from the saturated zone to the root zone could explain such underestimation of RZWS (Leblanc et al., 2011). Other reasons behind these findings could be the underestimated RZSC in this region as well as the intensive human activities, including dam construction, a water diversion system and river management, which will impact both the RZSC estimation and RZWS simulation (Reid et al., 2002; Kingsford, 2000).

6. This brings me to another point, that the FLEX model used apparently does not include capillary rise. In wetlands, this is a dominant mechanism, and also some dryland vegetation is known to tap water from deeper layers. A landscape-based model as developed by Gao et al. (2014a) could cater for this and could also distinguish between an independently derived Beta function for the wetland-terrace-plateau continuum and a calibrated Beta for hillslopes

Thank you for the comment. We agree that the capillary rise is important in regions in which surface water and groundwater exchanges are intense, e.g., in wetlands and regions with deep root plants. The upward capillary fluxes can impact the root zone water storage and thus the water budget between the surface and the lower atmosphere (Vergnes et al., 2014). In fact, WAYS does include the capillary module from Gao et al. (2014a), a key publication of the FLEX model. At the current stage, it is, however, disabled due to the lack of global information on the groundwater table. Of course, this will affect the simulated results in this work, e.g., the evaporation and RZWS. However, we disabled the capillary module based on our analysis.

We set up two experimental runs for the WAYS model, for which one run with the capillary module was active and the other with the capillary module inactive. For both runs, the RZSC data ($S_{R,CRU-SM}$) that are derived based on CRU, SSEBop and MOD16 are used. The model is calibrated before running for each simulation run, and the parameter range of C_{Rmax} is set to (0.01, 2), where C_{Rmax} is the key parameter for the capillary module that controls the maximum capillary rise. The simulation period is set to 1989-2005 for both runs, as we compared the simulated evaporation with the LandFluxEVAL data set, which is only available in this period. The LandFluxEVAL data are a merged benchmark synthesis product of evaporation on the global scale and comprise a combination of land-surface model simulations, remote sensing products, reanalysis data and ground observation data (Mueller et al., 2013). The LandFluxEVAL data are used in many studies as refence data for evaporation evaluations (Lorenz et al., 2014; Martens et al., 2017; Wartenburger et al., 2018). The results show that the simulated evaporation is significantly overestimated for the run in which the capillary module is switched on. The global averaged annual evaporation is estimated as 513 mm/year by WAYS with the capillary module switched off, while the global averaged annual evaporation is simulated as 697 mm/year by switching on the capillary module. In the LandFluxEVAL data, the global averaged annual evaporation is 491 mm/year. The significant overestimation of evaporation by WAYS with capillary module switched on.

This is mainly because there is no observed groundwater table information to constrain the capillary rise amount. Therefore, we decided to disable the capillary module in the current version of WAYS; it can be active once the global groundwater table information is available. To clarify this information in the manuscript, we have revised the text accordingly by stating our analysis-based decision regarding capillary module deactivation as well as discussing the impacts of ignoring the capillary rise. The revisions can be found in "Authors' change in the manuscript" below.

Regarding the issue of "using an independently derived Beta function for the wetland-terrace-plateau continuum and a calibrated Beta for hillslopes in the model.", we agree with the referee that this would be an interesting experiment and the derivation of parameter β from HSC-MCT without calibration can benefit any conceptual hydrological models. However, the HSC-MCT module is heavily dependent on the HAND data, and a verified global data set on HAND is not currently available. The landscape classification for wetland, terrace, plateau and hillslopes is also based on the HAND data. Indeed, HAND can be simply derived from DEM data. However, HAND data that are not well verified could potentially introduce large uncertainties because HAND is very sensitive to the drainage threshold and the open water elevation (Nobre et al., 2016).

Given that the manuscript is already quite extensive, including these results would not necessarily contribute to improving the manuscript clarity. In addition, it is not the main objective of the paper to focus on deriving the beta function independently from the HAND data. Therefore, we prefer not to include these results in the paper.

Authors' change in the manuscript.

Page 25, Line 4: The following paragraph is inserted in the discussion part
Moreover, the current study does not consider the groundwater access and irrigation mainly due to the lack of global information. The groundwater table information is crucial for capillary rise simulation (Vergnes et al., 2014). Capillary rise simulation without proper water table information could significantly overestimate the evaporation. Thus, the capillary rise flux is ignored in this study. A similar strategy has also been applied by other works due to the absence of the information on the global water table (Döll et al., 2003; De Graaf et al., 2015; Hanasaki et al., 2018). Observations of irrigation on the global scale are also not available (Leng et al., 2015). Although there are simulated irrigation data available on the global scale, the inherent uncertainties could be propagated in our model simulation. Therefore, irrigation is also not considered at this time. However, this neglect could potentially introduce biases into the model simulation in irrigated areas and deep rooted plant-distributed regions, as both irrigation and capillary rise are an additional supply of soil water recharge. The biases may cause an underestimation of evaporation, especially in the dry summertime (Vergnes et al., 2014). This underestimation could consequently affect the simulation of RZWS and runoff because of the interlinkage of these three elements (Rockström et al., 1999). It is found that ignoring the capillary rise could reduce soil water content in the root zone (RZWS), while the runoff will also be reduced (Vergnes et al., 2014). However, these shortcomings can be simply overcome once the global data are available.

7. I don’t understand the last sentence in the abstract. Indeed CHIRPS-CSM is limited to lower latitudes, but CRU-SM covers the entire globe. I think the sentence "Therefore, the performance etc." can be deleted.

Thanks for the comment. We have deleted this sentence in the manuscript.

Authors’ change in the manuscript.

Page 1, Line 14:

The following sentence is deleted in page 1, line 12.
~~“Therefore, the performance of the model in such regions is not justified.”~~

8. There are many typos. I think the paper requires copyediting, which probably Copernicus can take care of.

Thank you for the comment. The revised manuscript has now been edited by a professional academic language and manuscript service company.

So in summary, I think this is an important paper, but additional work needs to be done before the paper can be published.

The references used in this comment also occur in the discussion paper, except the following:
Gao, H., Birkel, C., Hrachowitz, M., Tetzlaff, D., Soulsby, C., and Savenije, H. H. G., 2019. A simple topography-driven and calibration-free runoff generation module, Hydrol. Earth Syst. Sci., 23, 787-809, https://doi.org/10.5194/hess-23-787-2019.

We would like to express our sincere thanks again to Referee Prof. Hubert H.G. Savenije for his time reviewing our manuscript and for the valuable comments to improve our manuscript.

Reference:

Nobre, A. D., Cuartas, L. A., Momo, M. R., Severo, D. L., Pinheiro, A. and Nobre, C. A.: HAND contour: A new proxy predictor of inundation extent, Hydrol. Process., 30(2), 320–333, doi:10.1002/hyp.10581, 2016.