Reply to Referee comment 3

Dear Editors and Reviewers:

We would like to thank the editor and all reviewers for their valuable suggestions and comments on the manuscript. These comments have not only improved the quality of the current manuscript, but also are beneficial to our future research in general. All point-by-point responses are presented as follows and we will carefully revise the manuscript based on these comments. For clarity, all comments are given in the original version, while responses are marked in blue.

Anonymous Referee #3

The authors of the manuscript "Impacts of land use/cover change and reforestation on summer rainfall for the Yangtze River Basin" present work that show the effects of land use and land cover change on regional climate processes including summer rainfall. The manuscript shows the importance of better understanding these effects and has some interesting discussion points. These types of studies are difficult to do and this is a great start. However, in my opinion, the points outlined in this review need to be addressed for this work to have scientific merit.

Thanks for the positive evaluations and comments, all the comments and suggestions have been addressed and will be incorporated into the revised manuscript.

General comments:

1. The methods used to change land cover need to be discussed further as other reviewers have mentioned. The land surface model (Noah-MP) is complex and offers many options to better represent land surface processes. The land surface model is only mentioned once in the text. Noah-MP contains too many options that need to be carefully chosen for this to be glossed over. Additionally, Noah-MP uses only the dominant land use category when calculating surface fluxes, so at 15km an increase in forest will not matter if it doesn't become the dominant category. This may help explain the inconsistent results between the 20% and 50% reforestation but without more information, it's hard to say.

Thanks for the comment. The land use changes were included in the WRF model by modifying the geographical static data used in the model which further changed the simulation of subprocesses such as the vegetation phenology, canopy stomatal resistance, runoff and groundwater in the land surface model Noah-MP (Li et al. 2018). Many parameters were used in Noah-MP to describe the characteristics of different land use types, such as albedo, HVT (Top of canopy), LAI (Monthly leaf area index), and VCMX25 (Maximum rate of carboxylation at 25 °C). When the land use changed, these parameters changed accordingly which finally led to the changes in substance and energy exchanges between atmosphere and land surface. The geographical static data we used in the WPS is *landuse_30s_with_lakes* which was download from the WRF website. The land use data of 1990 and 2010 scenarios were derived from the Landsat thematic mapper (TM) digital images. And then, in the YRB, we replaced the land use data from the *landuse_30s_with_lakes* with the land use data from the Landsat thematic mapper (TM) digital images. Finally, we randomly changed 20% and 50% of the croplands to be forests using the 2010 scenario as a baseline to produce 20% and 50% reforestation scenarios. Both the land cover data (downloaded from the WRF website and derived from the digital images) have a resolution of 1km. As the resolution of inner domain of WRF model was set as 15km, the post-processed land cover data was resampled from 1km to 15km by the WPS (the WRF Preprocessing System). Then, the dominant land cover categories in model grids were used for the Noah-MP model to correctly reflect the intended land cover changes. For 20% and 50% scenarios, there were 408 and 1060 cropland grids experience land cover changes, while the total grids of cropland in 2010 scenarios was 2231.

Reference:

Li, J., Chen, F., Zhang, G., Barlage, M., Gan, Y., Xin, Y., and Wang, C.: Impacts of Land Cover and Soil Texture Uncertainty on Land Model Simulations Over the Central Tibetan Plateau, Journal of Advances in Modeling Earth Systems, 10, 2121-2146, https://doi.org/10.1029/2018ms001377, 2018.

2. The limitations of using a convective parameterization when investigating rainfall extremes needs to be discussed. In a region with large vertical relief, the choice to use a course resolution for this study should be justified. Convection permitting scales (<4km) not only allow for better representation of precipitation processes, but also better land surface representation (including topography).

Thanks for the comment. We realize that convective parameterizations differ greatly in their treatment of the cloud up draughts and down draughts, mass-flux closure and triggering, often assuming that one is averaging over both cloud up draughts and the subsiding environment. As a result, all these schemes are better at predicting the area-average rainfall (Clark et al., 2016). Additionally, the cumulus parameterizations also introduce uncertainties to the model results (Liu et al., 2017). We also agree that higher model resolution can better represent precipitation processes and land surface. However, this study focused on the Yangtze River basin, which had a total area of ~1.8×10^6 km^2. Considering the huge area, the spatial resolution of 15 km may be enough to reproduce reliable rainfall patterns. In some other studies which evaluated the impacts of land use/cover changes on climate over such a big region, the resolution of model was usually even coarser (e.g., Hu et al., 2014; Zhang et al., 2017). We will add above clarification and the relevant information and discussion in the Discussion section of the revised manuscript.

References:

Clark, P., Roberts, N., Lean, H., Ballard, S. P., and Charlton-Perez, C.: Convection-permitting models: a step-change in rainfall forecasting, Meteorological Applications, 23, 165-181, https://doi.org/10.1002/met.1538, 2016.

Liu, C., Ikeda, K., Rasmussen, R., Barlage, M., Newman, A. J., Prein, A. F., Chen, F., Chen, L., Clark, M., Dai, A., Dudhia, J., Eidhammer, T., Gochis, D., Gutmann, E., Kurkute, S., Li, Y., Thompson, G., and Yates, D.: Continental-scale convection-permitting modeling of the current and future climate of North America, Climate Dynamics, 49, 71-95, https://doi.org/10.1007/s00382-016-3327-9, 2016.

Hu, Y., Zhang, X.-Z., Mao, R., Gong, D.-Y., Liu, H.-b., and Yang, J.: Modeled responses of summer climate to realistic land use/cover changes from the 1980s to the 2000s over eastern China, Journal of Geophysical Research: Atmospheres, 120, 167-179, https://doi.org/10.1002/2014jd022288, 2015.

Zhang, X., Xiong, Z., Zhang, X., Shi, Y., Liu, J., Shao, Q., and Yan, X.: Simulation of the climatic effects of land use/land cover changes in eastern China using multi-model ensembles, Global and Planetary Change, 154, 1-9, https://doi.org/10.1016/j.gloplacha.2017.05.003, 2017.

3. The model validation is insufficient. Look to Liu et al., 2017, for an example of full model validation. To be specific, I would like to see the figures reworked to show the spatial patterns of rainfall on a seasonal and annual basis in the observations and in the control simulations. Furthermore, the figures should include a representation of percent change in rainfall. A bias of 600mm of rainfall during the summer months is a lot if the average summer rainfall is only 1000mm. This information isn't shown so it's hard to know if the bias is significant. Statistical testing should also be included where appropriate. Additionally, validation of other climatic components that contribute to rainfall (such as the vertical structure of the atmosphere, PBLH, CAPE, CIN) would aid this study. Validation of surface fluxes would also help build a better picture of how well the model can represent this region. There are several eddy-covariance towers in the eastern part of the domain and a comparison of sensible and latent heat flux to those towers would be interesting. Any change that is presented should have an accompanying discussion of validation for that component. Showing Figure 10 but compared to observations would be necessary to see if WRF can capture extreme rainfall.

Thanks for the comments. We will add the figures to show the spatial patterns of rainfall on a seasonal and annual basis for the observations and in the control simulations. The figures will include a representation of percent change and the results of statistical testing in the revised manuscript. In addition, we are aware of that the validation of other climate components and surface fluxes can be helpful. Actually, we have already looked for the data from the several eddy-covariance towers in the eastern part of the domain that reviewer mentioned here. However, we can only get flux data of one of these towers from the China Nation Science and Technology Infrastructure (http://www.cnern.org.cn/index.jsp) and the data are from 2003 to 2010 which is mismatch with our simulation period. So unfortunately, we don't have such observation data in the study. In this case, we will add ERA5 dataset in the revised manuscript for a further model evaluation, including the 850hpa humidity and surface fluxes variables, such as sensible and latent heat fluxes. Besides, we will also add the temperature evaluation based on the observed temperature data, which is the only available observation data we have besides observed precipitation in the study. Furthermore, we will also show the probability distribution functions of rainfall in 2010 scenarios compared to observations to see if WRF can capture extreme rainfall.

4. The taylor diagrams are honestly pretty confusing, I would remove them and provide a table of biases instead. The correlation coefficients are rather low for temperature (the easiest for the model to accurately capture) and lower for rainfall when compared to observations. This leads me to believe that the model isn't configured properly for this region. If the above issues were tackled, then this opinion might change. One way to show that the model is well validated is to show that the temperature and rainfall falls within the spread of observations. Comparison to not only the station data but to an independent gridded dataset (such as ERA5, CRU, etc.) would strengthen this point.

Thanks for the comment. We have removed the taylor diagrams. The correlation coefficient was rather low for temperature might be because that the taylor diagrams were calculated at station basis by interpolating the simulation results to stations. When calculating the correlation coefficient of temperature at grid scale, the result was acceptable. We have added qq-plots of temperature and rainfall between simulated and observed data (Fig. 4 and Fig. 5), and find that the distribution of temperature and rainfall simulated by model are linear correlated with those of observation. We will also compare the simulated data with both station data and ERA5 dataset to see whether the simulated temperature and rainfall fall within the spread of observations.

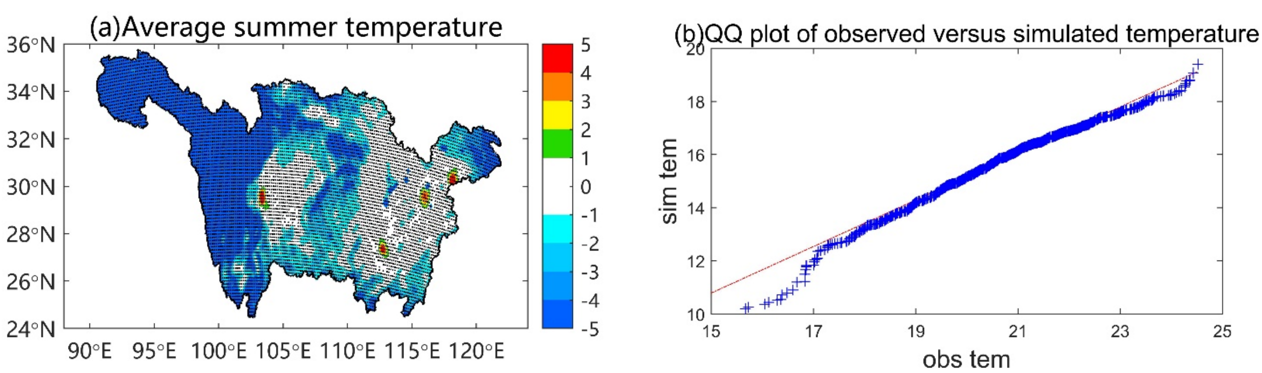

Figure 4. The bias of (a) average summer temperature (℃), and (b) the qq-plot of observed temperature versus simulated temperature. The stippling regions show statistically significance of bias identified by t-test at a 5% significance level.

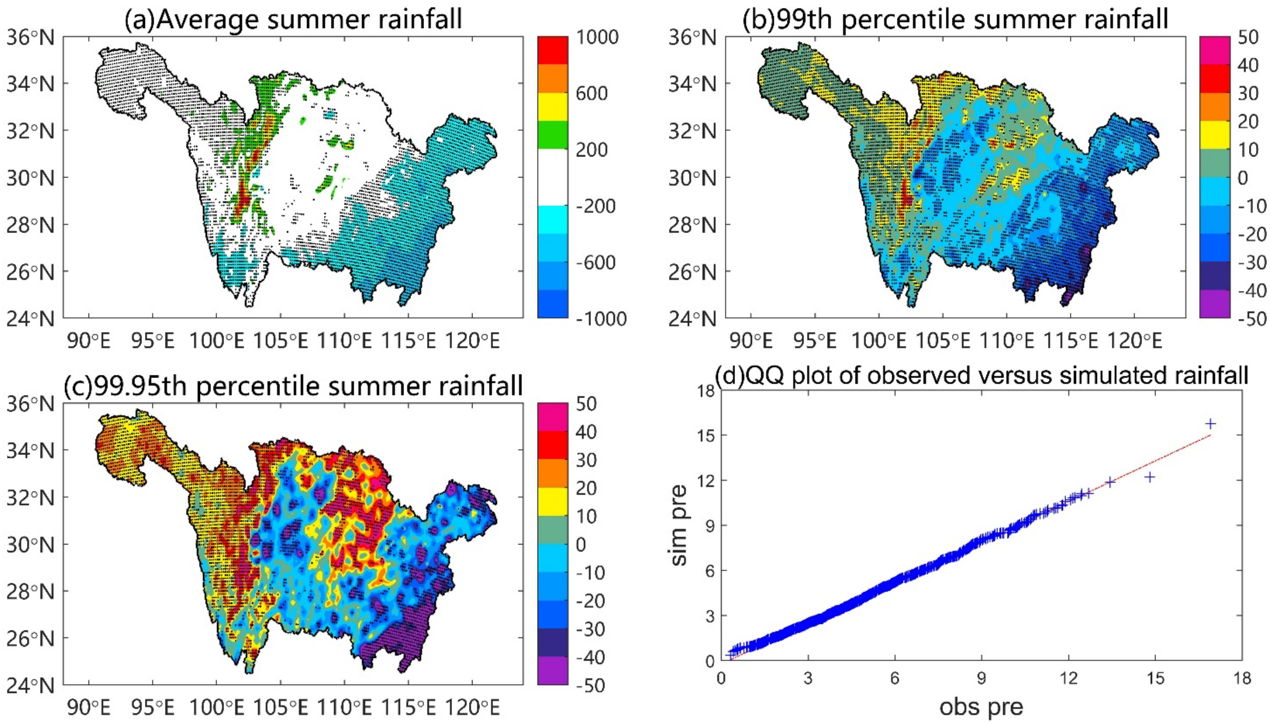

Figure 5. The bias of (a) average summer rainfall (mm), (b) 99th percentile summer rainfall (mm/day) and (c) 50th percentile summer rainfall (mm/day) between the observed data and 2010 scenario, and (d) the qq-plot of observed rainfall versus simulated rainfall. The stippling regions show statistically significance of bias identified by t-test at a 5% significance level.

5. The percentiles of rainfall need to be defined better. What does 99th percentile mean in this case? Is it the 99th percentile of rainfall events over the 11 years? Without sub-daily rainfall, I'm not sure that this qualifies as extreme per se. A common extreme rainfall metric is the 99th percentile of daily maximum rainfall (requires sub daily rainfall to properly calculate). In my country, the storms that produce flash flooding often last only a few hours, vs a monsoon type rain that produces flooding from many, many hours of low intensity rainfall. More discussion of rainfall in this region would put this information into context. I would remove the figures that show changes to median rainfall and instead discuss some other metric of interest.

Thanks for the comment. The 99th percentile is the multiyear average value from the 99th percentile rainfall in each year. We did not use sub-daily rainfall because the flash flooding in Yangtze River basin was often caused by continuous rainfall last for a few days, as it is a big basin. A few hours of high intensity rainfall would not cause severe flooding due to the construction of cascade reservoirs along the river. Moreover, we have replaced the results of median rainfall with 99.95th rainfall in order to give a more comprehensive assess on the different levels of extreme rainfall. The analysis and relevant results will be added in the revised manuscript.

6. All the figures showing change between simulations need to have statistical testing. The figures all look very noisy and some of the changes to precipitation could be because the storms moved, not because more rain fell.

We agree with this comment. We have conducted statistical tests and modified all the figures showing change between simulations to present the results of statistical tests. We display the revised Fig. 7 as follows; results of other significance test will be incorporated into the revised manuscript.

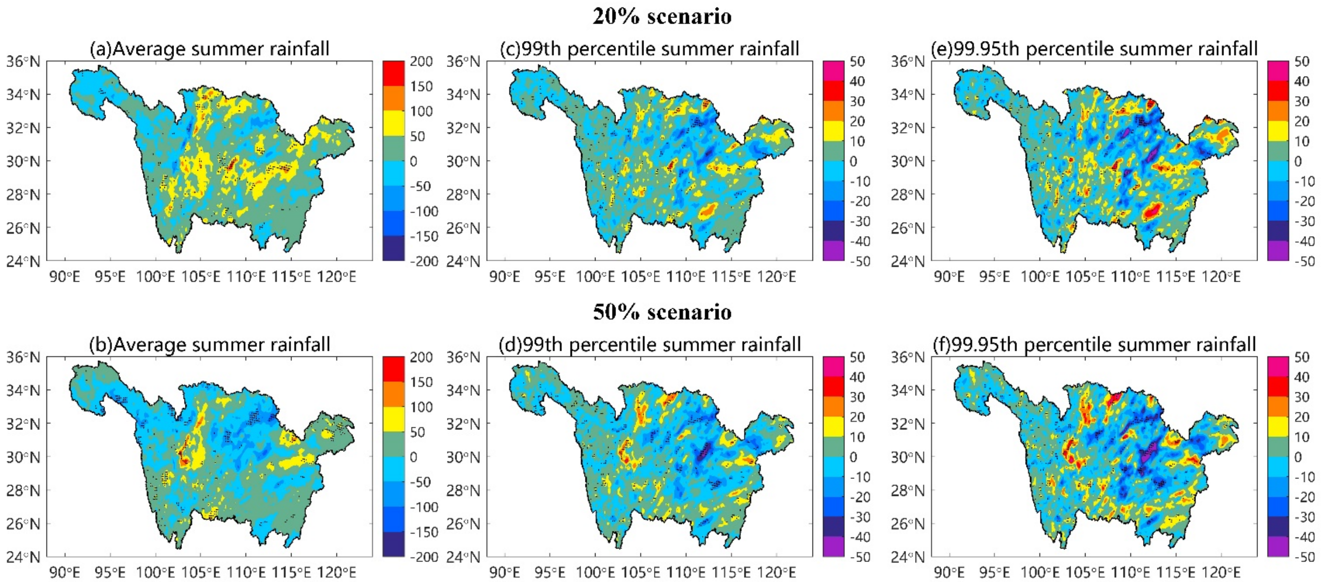

Figure 7. The changes in (a-b) average summer rainfall (mm), (c-d) 99th percentile summer rainfall (mm/day) and (e-f) 50th percentile summer rainfall (mm/day) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of bias identified by t-test at a 5% significance level.

7. Instead of bar graphs, boxplots or violin plots should be shown. This will capture the distribution of the change.

Thanks for the comment. We will add boxplots to show the distribution of the changes in the revised manuscript.

Minor specific comments:

The convention I have seen for abbreviating land use and land cover change is LULCC not LUCC.

Thanks for the comment. We will replace all the “LUCC” with “LULCC” in the revised manuscript.

165 There are some English language errors in the text, but these don't bother me that much and have been covered by other reviewers.

We will carefully check the whole paper to improve the quality.