

Dear Anonymous Referee #2,

Thanks very much for your constructive comments concerning our manuscript entitled “Responses of runoff to historical and future climate variability over China” (Manuscript No.: hess-2017-98). Those comments are all valuable and very helpful for revising and improving our paper, as well as the important guiding significance to our researches. We have studied comments carefully and here replied each comment below.

Comments from Anonymous Referee #2:

This paper applies Budyko’s concept of ‘climate elasticity’ in the response of runoff to changes in precipitation, potential evapotranspiration and catchment properties to projections of climate change from an ensemble of general circulation model projections. The authors use this to assess the robustness of projections of changes in future due to climate change in different regions of China.

Climate elasticity concept seems quite neat for the question of responses to climate change (separating P and PET drivers, and also with the potential for accounting for other drivers via the catchment properties) and in my opinion the authors have applied this appropriately to the specific question of responses to an ensemble of climate change projections. I would however advise more care in the interpretation, as these should not be taken as actual predictions of the future (which the language used some- times suggests that there are). There are 3 reasons for this :

1. (1) While the use of the multi-model ensemble probably is a good, well-established way to explore a number of possible outcomes, the ensemble is not designed to be probabilistic, ie: it is not intended to give an indication of likelihoods. It is an ‘ensemble of opportunity’, using all models that happened to be available in the community, and the levels of skill for regional climate change in China will vary somewhat arbitrarily. The models themselves have not been specifically chosen or varied in order to systematically explore regional climate changes. Likelihood statements generally require further backing-up with understanding of model performance and the simulated climate processes in the region in question. Therefore I would encourage the authors to avoid terms such as “climate change will likely cause an obvious increase (decrease) of R” – the simulations are not intended to give guidance on likelihoods. (2) It is also not clear to me whether the catchment properties term includes plant stomatal responses to CO₂. (It could do in theory). Two recent papers (Milly and Dunne, 2016, Nature Climate Change, and Swann et al, 2016, PNAS) showed that projected runoff changes in the GCMs tend to show a greater increase or smaller decrease in runoff than many hydrological models, because the GCM land surface schemes tend to include this term whereas hydrological models do not. It is not clear whether the VIC model includes this here or not. (3) The method used here does not, I believe, include other drivers of hydrological change eg. Land cover change, groundwater and river water extraction, irrigation etc. I think that in theory the catchment properties quantity could account for this, but it has not been applied to this here. We cannot assume that climate change is the only driver of hydrological change, and hence the interpretation of the results should bear this in mind.

Response: Thank you very much for your nice comments. For the question 1, we quite agree with

your points that the multi-model ensemble is not designed to be probabilistic and is not intended to give an indication of likelihoods. Therefore, likelihood statements, which generally require further backing-up with understanding of model performance and the simulated climate processes, are not appropriate here. According to your good suggestions, we have changed the statements of some sentences in the revised manuscript to avoid term such as ‘climate change will likely cause an obvious increase (decrease) of R ’ (changed to ‘climate change is projected to cause an increase (decrease) in R ’).

For the question 2, thank you for providing these two very nice references (Milly and Dunne, 2016, Swann et al, 2016), which showed a very important information that the plant responses to increasing CO₂ tend to increase the amount of water on land, leading to a greater increase in runoff. We note that the VIC model used for the calculation of runoff does not include the schemes of the plant stomatal responses to CO₂. Therefore, under high CO₂ condition, neglecting the plant stomatal responses to CO₂ would lead to the underestimation of runoff in the hydrological model. According to your good comments, we made a discussion on this point to highlight the importance of the plant stomatal responses to CO₂ in the assessment of hydrological impacts of climate change. In addition, the empirical parameter in the Budyko equations well accounts for the effects of catchment properties (e.g. land surface characteristics, the average slope, and vegetation type) on the water-energy balance. Therefore, the catchment properties term could include plant stomatal responses to CO₂ in theory. This is a very nice suggestion for us to try to characterize the plant stomatal responses to CO₂ using the catchment properties term in the future work, especially under high CO₂ condition.

For the question 3, we quite agree with your comments that there are also other drivers of hydrological change in addition to climate change. Our method only considers the hydrological change due to climate change but neglects the effects of the variability of catchment properties (e.g., land cover change, groundwater and river water extraction, urbanization, irrigation, etc.) on the hydrology. According to your good comments, we made a discussion on the other driver (catchment properties) of hydrological change for the interpretation of the results in the revised manuscript.

2. The authors do acknowledge some of these issues to some extent at the end of the paper, but this is after the earlier discussion which often uses language of prediction, which I think goes too far. I would suggest terms such as “Climate change is projected to cause an increase (decrease) in R : : :.” Also I suggest the authors address the above points in more detail, highlighting the limits to the interpretation of the CMIP5 ensemble in terms of likelihoods.

Response: Thank you very much for your nice comments. According to your good suggestions, we have changed the sentence “climate change will likely cause an obvious increase (decrease) of R ...” to “climate change is projected to cause an increase (decrease) in R ...”. We also addressed the above points in more detail in the revised manuscript to highlight the limits to the interpretation of the CMIP5 ensemble in terms of likelihoods.

3. My other concern is why the authors chose to use the Thorthwaite method for PET. It is stated

on page 14 line 4 that this is because there is a “lack of meteorological data (such as relative humidity) in the GCM data. This is not true – GCMs are meteorological models, and indeed some of the CMIP5 GCMs are used in slightly different variants for numerical weather prediction. A huge range of meteorological outputs is available, including RH – see here http://cmip-pcmdi.llnl.gov/cmip5/docs/standard_output.pdf.

I recommend that the authors use the data portal http://cmippcmdi.llnl.gov/cmip5/data_description.html at PCMDI, who organised CMIP5. The Canadian Climate Centre webpage used by the authors only has a very limited number of variables.

Response: Thank you very much for your nice comments. In the original version (i.e. initial submitted manuscript), the *PET* of GCM for the baseline 1971–2000 and the future period 2071–2100 is estimated by the Thornthwaite method. We noted that the temperature-based Thornthwaite method is lack of physical basis, and it is necessary to justify the use of the Thornthwaite method and the use of more physically *PET* calculation methods. Thank you very much for informing us that the meteorological data used for the calculation of *PET* is available from the CMIP5 output http://cmippcmdi.llnl.gov/cmip5/data_description.html at PCMDI. Indeed, there is a huge range of meteorological outputs (including RH) from the CMIP5 models, which are enough for the calculation of *PET* by the Penman method. However, due to large amounts of data needed to be processed (including (1) download the 28 GCMs meteorological data, (2) statistical downscaling of the 28 GCMs meteorological data over China, (3) bias correction of the 28 GCMs meteorological data, (4) calculations of *PET* for the 28 GCMs, and (5) bias correction of *PET* for the 28 GCMs), it is difficult for us to complete it in a short period.

However, we tried our best to correct the *PET* of GCMs calculated by the Thornthwaite method, and made a detailed comparison of the corrected *PET* method with other *PET* calculation methods to justify the use of *PET* calculation of the GCMs. In particular, there are three main changes for the *PET* calculations in the revised manuscript, which are as follows:

(1) We used a more physically *PET* data that estimated by the Penman equation (data during the period 1960–2008 provided by the Hydroclimatology Group of Princeton University) to calculate the climate elasticity (i.e. *PET* elasticity) over China instead of the *PET* data from the FAO Penman-Monteith method. We believe the climate elasticity would be more accurate in the revised manuscript than in the original version.

(2) We used a multiplicative correction method to correct the *PET* data of GCMs calculated from the Thornthwaite method as follows:

$$PET_{cor,GCM,i} = PET_{Th,GCM,i} \times \frac{\overline{PET}_{Pen,obs,i}}{\overline{PET}_{Th,obs,i}} \quad (1)$$

where $PET_{Th,GCM,i}$ and $PET_{cor,GCM,i}$ are annual *PET* from the Thornthwaite method and the bias-corrected annual *PET*, respectively, for the i th grid point of the GCM data. $\overline{PET}_{Pen,obs,i}$ and

$\overline{PET}_{Th,obs,i}$ are the 49-year averages of PET calculated from the Penman method and Thornthwaite method, respectively, for the i th grid point for the period 1960–2008.

Based on the monthly data of temperature covering the period 1960–2008 provided by the Climatic Research Unit (CRU), the PET was calculated by the Thornthwaite method and then corrected by the equation (1) to test the applicability of the multiplicative correction method. The results indicated that the corrected annual PET shows a good agreement with that calculated by the Penman method (as shown in Figure R1). These two methods are quite consistent at both basin and grid scales, suggesting that the equation (1) above is acceptable for the bias correction of PET of the GCMs.

(3) We compared the four PET calculation methods (i.e., the Penman method, the Thornthwaite method, the FAO-56 Penman–Monteith method, and the Thornthwaite method corrected by the equation (1)) to test the robustness of the PET elasticity result subject to PET uncertainties. The results indicated that the mean annual PET by the Penman method, the FAO-56 Penman–Monteith method, and the Thornthwaite method corrected by the equation (1) are quite consistent, and the PET elasticity calculations from these three methods give very similar results in all 14 basins (as shown in Figure R2). That is to say, the Thornthwaite method corrected by the equation (1) significantly improves the accuracy of PET and can be acceptable for the PET calculation of the GCMs.

In the future work, we are going to calculate the Penman PET using the meteorological data from the CMIP5 output and make a comparative analysis to fully understand the PET calculation uncertainties in the projections of climate change.

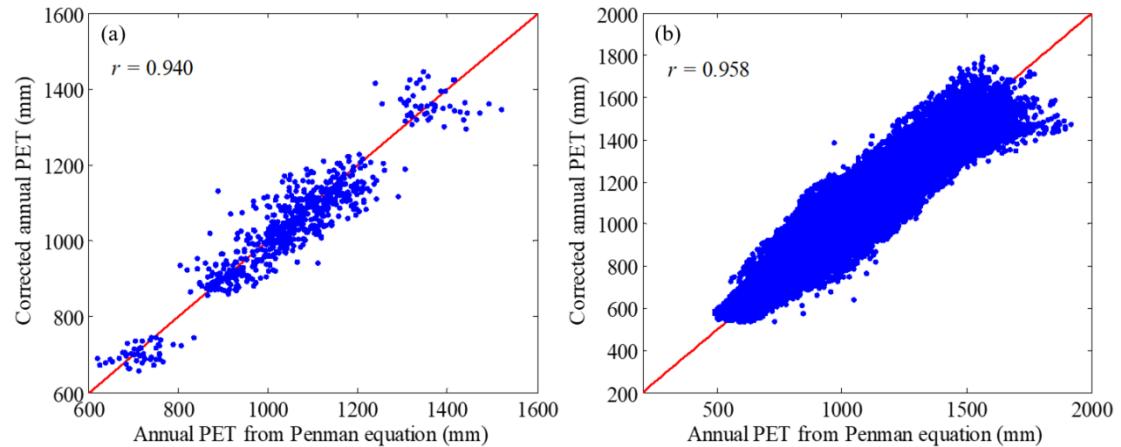


Figure R1. Comparison of annual PET calculated from the Penman method and the Thornthwaite method corrected by Equation (1) during the period 1960–2008 for (a) the 14 river basins and (b) all 0.5° grid points over China.

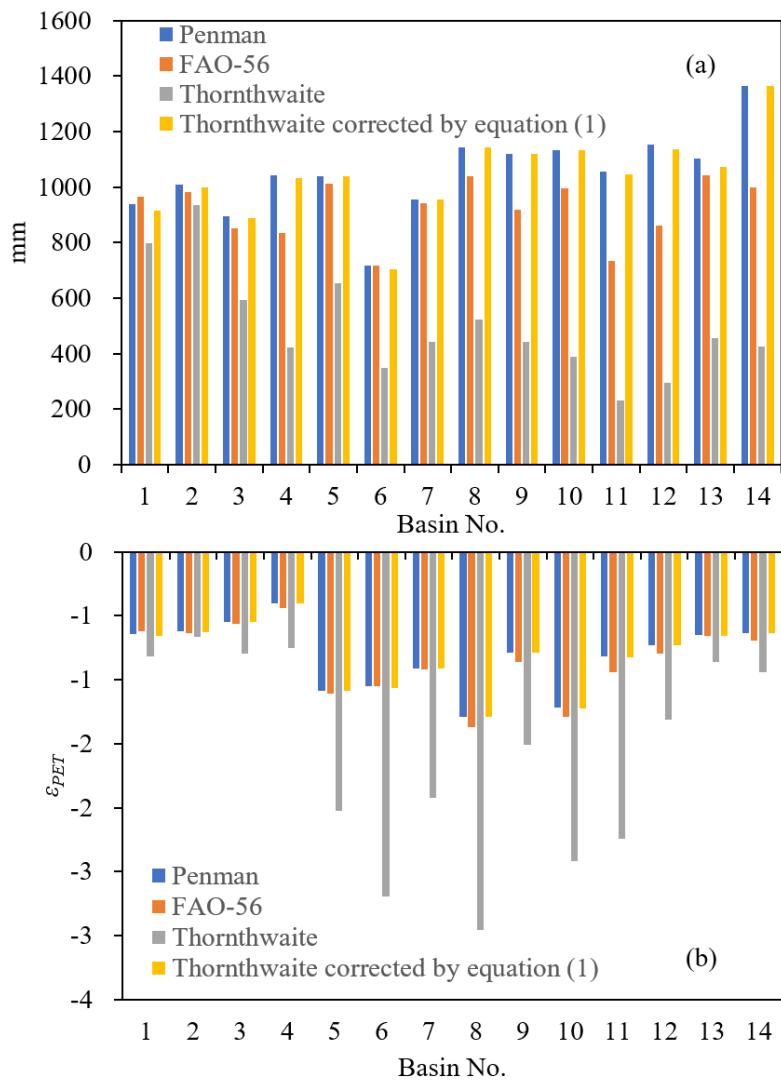


Figure R2. (a) Mean annual PET calculated from the four methods for the 14 river basins of China during the period 1960–2008. (b) PET elasticity calculated based on the four PET data for the 14 river basins of China during the period 1960–2008. The basin number is as follows: 1, Southeast Drainage; 2, Pearl River; 3, Yangtze River; 4, Southwest Drainage; 5, Huaihe River; 6, Heilongjiang River; 7, Liaohe River; 8, Haihe River; 9, Yellow River; 10, Inner Mongolia River; 11, Qiangtang River; 12, Qinghai River; 13, Xinjiang River, 14, Hexi River.