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Discussions

Interactive comment on "A simple tool for refining GCM water availability projections, applied to Chinese catchments" *by* Joe M. Osborne and F. Hugo Lambert

Anonymous Referee #2

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The manuscript entitled "A simple tool for refining GCM water availability projections, applied to Chinese catchments" by Joe Osborne et al., presents a simple tool to post-process CMIP5 model output in order to constrain projections of water availability. The tool is based on the Budyko framework and uses bias-corrected estimates of modeled precipitation and potential evaporation to obtain catchment runoff for two large basins in China: the Yangtze and the Yellow River. The approach was shown to substantially reduce uncertainties in the runoff projections, especially for the Yellow river basin. It was further shown that observed runoff changes within the second half of the 20th century are primarily caused by human interventions.

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Discussion paper

The manuscript is generally in a good shape, well structured and well written. The overall presentation of the results is good with concise and high-quality figures. The methodological approach seems to be technically sound, but (due to its complexity) needs to be explained better. Maybe some sort of conceptual figure or flow chart would help! Further, the obtained results depend on many assumptions which potentially not permit a robust interpretation of the results. The authors already discuss some limitations of their approach, but it is my assessment that this discussion needs to be extended before final publication. Also, some important references are missing to better outline some issues and limitations of their approach.

Major comments:

1) I would be very careful with separating the measured change in runoff into the individual components as done in eq. 7. If you assume such a linear relationship, you also assume the individual components to be independent, which they are clearly not! Especially the separation into Qh and Qo is potentially dangerous. Please also be aware that in the context of the Budyko framework, aridity is solely defined through the notional, dimensionless ratio Ep/P, which has no direct physical meaning. Everything else besides mean annual Ep/P is actually integrated into w. Also, w and Ep/P are not necessarily independent (please see Padron et al, 2017). It would be nice if you could try to determine if there are dependencies between Qa, Qh, and Qo. Is it possible to plot these against each other? In case there are large dependencies and interrelationships the obtained results might be less meaningful.

2) In the main text, the authors assume w (omega) to be constant. In the Supplementary they further present results obtained for a (time-)varying w. I actually leave this to the authors, but I would almost prefer to present the time-varying more prominently. A certain variation around the original Budyko curve and thus a variation of w is actually inherent to the Budyko framework. This was already stated by Budyko himself. Hence, the Budyko framework is not necessarily deterministic. There is a growing body of literature interpreting the Budyko framework in a probabilistic sense (e.g. Greve et al.,

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2015, Singh and Kumar, 2015, Gudmundsson et al., 2016, etc.), thus accounting for the spread in w and taking into account the complex interplay of all other factors (besides the aridity index). By using the time-varying approach you basically account for these variations, which in my assessment is more realistic.

3) In this context, please be aware and discuss that you are considering temporal variations here. Most of the referenced Budyko-based studies actually consider spatial variations. It is important to note that these are not necessarily tradable (Berghuijs and Woods, 2017).

4) You use the Budyko-based equation 8 to compute Q* for each year. One of the main assumptions of the Budyko hypothesis is stationarity, i.e. storage changes are negligible. This might, however, not be the case here due to interannual variations in storage. Have you maybe tested to use longer time periods to smooth out long-term variations in water storage? How would your results look like when computing decadal Q*?

Minor comments:

p. 1, l. 3: I would not use "describe" here, it might be better to use something like "represent". The Budyko framework does not mechanistically describe the relationship between the aridity and the evaporative index.

p. 1, l. 16-19: There is some recent literature which challenges the referenced papers and the ubiquitous increase in aridity in a warmer world (Roderick et al., 2015, Scheff et al., 2017, Greve et al., 2017, Scheff, 2018). Might be worth to mention that this is an ongoing debate.

p. 2, l. 1-3: It might be better to rephrase this a bit. Depending on the context, the terms "water supply" and "water demand" are interpreted differently. Water supply is not necessarily just atmospheric water supply. Water supply can also be runoff. Some people also consider groundwater or water from other, unconventional sources

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(water transfer, desalination, etc.) as water "supply". Also, water stored in reservoirs is some sort of water supply. Water demand is often used in terms of human water consumption, including domestic and industrial water use as well as water used for irrigation.

p. 3 l. 7: For w=2.6, please cite Yang et al, 2008.

p. 4, Fig. 2: The basin outlines look a bit strange. I know that this is based on grid cells, but why don't you just outline the grid cells instead of interpolating the basin borders. This would also help to avoid the gap between both basins

p. 4, l. 3: Among seasonality, snow dynamics and storminess it is also many other factors that are related to a hydroclimatological change. Maybe just add an "etc."

p. 5, l. 24-26: Is this shown by Sitch et al., 2013. In this case, it might be better to add the reference at the end of the sentence. Or is this based on a separate analysis? In this case, you might add something like "not shown"?

p. 6, eq. 4: Milly actually proposes an ad-hoc multiplier of 0.8. Have you checked if you can maybe reduce some of the biases in Ep by adjusting the multiplier?

p. 8, l. 30: See my first major comment. This might also be due to dependencies between each factor.

p. 18, Fig. 10: Is the time series for Q* computed annually? And is the 5yr running mean subsequently computed from the annual time series? Or is Q* computed from the 5yr running mean time series of P and Ep?

p. 19, l. 30-32: Please note here that Li et al. (2018) is potentially only valid at large catchment scales. At other scales, this might not be the case. You might also note here or somewhere else that the interpretation of w is still largely and inconclusive and sometimes also contradictory (Padron et al., 2017).

References:

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