

Reviewer comment

Review comments – “Future shift of the relative roles of precipitation and temperature in controlling annual runoff in the conterminous United States” by Duan et al

This paper addresses the scientifically interesting and practically important issue of the controls on runoff (volume) changes that will be associated with future climate shifts. The general approach is that output from 20 GCMs archived in CMIP-5 were bias corrected and downscaled using MACA, then aggregated to monthly (I’m assuming that MACA was applied at a daily time step) and used to force their hydrological model at the scale of HUC-12 watersheds across the CONUS. They then partition changes in runoff as dependent on changes in precipitation, temperature, and the interaction of P and T. Just how this is done is a bit vague in the text following Eq. 2. I can’t tell whether they remove the annual mean P change for instance from the MACA downscaled P and T time series, and then do the same for T, and then simultaneously P and T? This needs to be clarified.

In any event, what they are after are ΔRP , ΔRT , and ΔRPT in Eq. 2, which, when expressed as fractions, provide the relative magnitudes of the changes in P and T on R, on an annual basis. So far, so good. The problem with the method is well described in Milly and Dunne (*Earth Interactions*, 2011). As described in the paper, their model uses a temperature index PET (Hamon). Algorithms of this class (there are a number), which are fit to site-specific climate data, tend to overestimate the sensitivity to temperature change. So it’s highly likely that their results over-predict the transition from P to T control of runoff that they project – the key result of the paper: “However, the influence of temperature is projected to increase ...” This problem has been pointed out in other contexts (see e.g. Sheffield et al, *Nature* 2012 on global drought). The issue arises because the sensitivity of runoff to future climate change is associated with the balance between P, ET, and R (not T). I suggest the authors read Jim Dooge’s AMS Horton Lecture paper (in *BAMS*, 1991 I believe). ET of course has a dependence on T, but it’s not direct. Temperature index PET algorithms are convenient because T is widely measured, whereas the physical variables that control PET (net solar radiation, net longwave radiation, vapor pressure deficit, and wind) mostly are not. The dominant variable (in the Rnet side of the Penman-Monteith equation, or variations thereof) is net solar radiation, which is not dependent on temperature. So what temperature index algorithms have to do is adjust to some climatology for net shortwave. Temperature clearly is a dependent variable in net longwave, although in a somewhat complicated way, and it’s a dependent variable in the vapor pressure deficit as well. In the Vano et al paper that they reference (although possibly one of the other Vano et al papers around the same time), she uses the VIC model as well as some other LSMs with physically based PET formulations, forced with shortwave, longwave, VPD, and other variables estimated using the daily temperature range and daily temperature (see Bohn et al., *Forest and Ag. Meteorology* 2013 for details). The key point regarding shortwave is that the algorithms estimate shortwave using the daily temperature range, and one typically has to make an assumption regarding how it will change in the future (given that subdaily output isn’t archived for most of the GCMs). She shows that the assumption as to how (or if) the diurnal temperature range changes results in about a factor of two difference in the temperature sensitivity in the Colorado River basin.

My recommendation is that the authors carefully consider the above. The approach they’ve used, based on a temperature index PET, is flawed, and I think has to be replaced. This will require a bit of work, but I think they are on a path to something interesting, and it will be well worth their time to go the extra mile on this. I think the result will be a much stronger paper. In its current form though, I don’t think the paper should be published.

Please feel free to divulge my name, and I’m happy to correspond with the authors if they feel it would be beneficial.

Dennis Lettenmaier

Author reply

We would like to thank Prof. Dennis Lettenmaier for his critical but stimulating comments, which gave us quite a bit to think about. Here are our responses.

(1) We have added more description in Section “2.3.2 Detecting of potential changes in future” to clarify the method and the use of the climate data. We used the 30-yr continuous series in different time periods to separate the changes in runoff. For example, using P data in 1970-1999 and T data projected for 2070-2099 to quantify the effect of T, using T in 1970-1999 and P in 2070-2099 to quantify the effect of P, and using T in 2070-2099 and P in 2070-2099 to quantify the combined effect of T and P.

(2) We agree that radiation and other meteorological factors could be important for estimating PET and bring more uncertainty into the sensitivity test of runoff. We briefly mentioned the uncertainty of PET estimation in the discussion of uncertainty sources. We will further highlight this issue and add more discussion in the revised manuscript. However, we want to clarify two points here.

First, despite using a temperature based PET method, actual ET is not solely dependent on PET (e.g., some conceptual models use $ET = k \times PET$). ET rates by land cover type were modeled as an empirical function of precipitation, PET, Leaf Area Index, and soil water availability. The Hamon’s PET is only used as a reference, or intermediate variable, for estimating ET. The algorithms of ET estimation, and monthly water balance among precipitation, ET, and runoff have been well verified over the CONUS in our previous studies (Sun et al., 2011; Caldwell et al., 2012). Essentially, ET is controlled by more than PET alone, so even there are prediction errors involved in PET estimates, the model might still work given that other controlling variables reflect the future conditions.

Second, we indeed considered the uncertainty from selecting PET method when the WaSSI model was first established. Hamon’s equation was chosen due to the availability of input data and the good correlation with actual ET. We believe that the projected fast increase in T is the major cause of the shift discussed in the manuscript, rather than the possible overestimation of PET. In an earlier study (Lu et al. 2005), we compared six different PET methods in southeastern U.S., and found that the result of temperature-based Hamon was similar to that of radiation-based Priestley-Taylor on annual basis, but lower than temperature-based Hargreaves. Hamon equation was proven to be useful, and not necessarily overestimating PET than radiation-based method (Federer et al., 1996; Vorosmarty et al, 1998). More uncertainty would be brought into the results if more variables were included. We will try to collect more data from other sources and account for the roles of other important factors in our future work. As for this study, we intend to focus on the relative importance of P and T. The evaluation of PET methods, and the impact of future changes in other meteorological variables (e.g., radiation, wind, humidity) are out of the scope. Besides, we believe that the large uncertainty from the projections of P and T is likely to overwhelm the uncertainty from using different PET equations.

References

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