

Interactive comment on “Dominant climatic factor driving annual runoff change at catchments scale over China” by Z. Huang and H. Yang

M. Renner (Referee)

mrenner@bgc-jena.mpg.de

Received and published: 7 January 2016

The authors apply the runoff elasticity method of Yang and Yang (2011) to mainland China and thereby extend work by Yang et al., (2014). The method is based on a Budyko framework and a first order derivative of the Penman equation to analyze the effect of observed trends in meteorological variables such as precipitation, net radiation, temperature, wind speed and relative humidity. This manuscript analyzed the same dataset as Yang et al., (2014) who also presented a runoff elasticity method but not with respect to forcing variables of the Penman equation.

Scientific interest

The reported trends between 1960-2010 in these variables are remarkable and de-

C5999

serve attention because they may have direct impacts on potential evaporation and the water balance. The proposed method by Yang and Yang (2011) is a quantitative and theory based way to estimate how runoff might have changed due to these trends. As the authors show in this manuscript these trends vary spatially in China and the sensitivity of the different catchments to change varies as well.

Unfortunately the authors do not discuss their results in depth. For example one potentially interesting point which is somewhat hidden in the results is that decreases both in net radiation and wind speed partly compensate the runoff decline caused by precipitation decreases. Also no discussion or further references on the origin, magnitude of the trends in the meteorological variables such as net radiation or wind speed is presented. Is the reduction in net radiation an result of decreasing solar radiation induced by atmospheric dimming or due to other variables? Such a discussion would help to understand the climatic impacts and their implications on water resources.

Novelty

The manuscript largely builds on previous work. The method, its comparison to hydrological modeling studies and an application to a large set of 89 catchments was presented by Yang and Yang (2011). The same dataset and the elasticity of precipitation and potential evaporation was recently presented by Yang et al., (2014). Some maps shown in this manuscript are very similar to those presented in Yang et al., (2014). For example compare Fig. 7 with Fig. 9 of Yang et al., (2014). Because this overlap is substantial (see also similarity report) I strongly recommend to discuss and explain the novelty and implications of this research.

Comparison vs. validation

The authors only compare their method with hydrological modeling results. This comparison is useful but is not a validation with independent data. Validation of runoff elasticity is generally difficult when other changes on catchment properties, water extraction, ... have been happening at the same time. Within the presented test catch-

C6000

ments the actual runoff change was always quite different to the estimated change by climate in on case even the sign was different (Table 3). In addition, while the data is presented on catchment level, apparently no runoff data was presented. I am wondering why is there no comparison with of the estimated runoff change with the actual runoff changes? This would give an indication on the importance of the climatic factors on actual runoff changes.

Definition of the aridity index / energy limit

Budyko defined the energy limit through the water equivalent of net radiation R_n/λ . Because R_n is not measured densely enough R_n/λ was replaced by some formulation of potential evaporation (UNEP 1992, World Atlas of desertification), which might be estimated by meteorological variables such as was done in this work. Interestingly, by using the approach of Yang and Yang (2011) net radiation reappears as control on evapotranspiration but in a different setting as originally proposed by Budyko's energy limits. Please discuss this aspect.

Format / presentation

The paper is written in rather focused way and is mostly easy to follow for the interested reader. However, the English needs to be improved throughout the manuscript. In particular the results section uses past tense when describing results. Some figures are too small to be able to read annotations and legends. The legends must also be harmonized among similar maps to allow a visual comparison.

Recommendation

While both methods and the dataset are not new, there is a strong scientific interest in how changes in different, albeit not independent meteorological variables might have impacted regional water balances. This imported aspect is somewhat hidden in the results but should be highlighted in an revised version of the manuscript. Given the list of points which need to be improved I recommend major revisions before publication in

C6001

HESS.

Further Comments:

– Section 5.1 Discussion of climate sensitivity estimates:

a) I wonder why other estimates using the same method / data should be different, please clarify!

b) If the cited estimates from the literature are independently derived, I advise to make a table which is easier than having all these numbers in the text.

– Section 5.1 Discussion of temperature sensitivity:

The whole paragraph starting on page 12925L12 is not very clear and needs a better presentation. For example results on $\partial E_0/\partial \Delta$ and $\partial E_0/\partial e_s$ are discussed but I could not find them in the results section.

– The last paragraph of section 5.1 seems to be copied from Yang and Yang (2011)

– Please, provide the reference for Eq.12?

– state that Eq. is an empirical formulation for net radiation

P12917L3: missing word

P12919L21: Maidment

P12920L5: change to “Comparison of the climate elasticity method with hydrological models”

P12920L11 remove and rephrase “provided strong evidence” see earlier comments

P12920 / Figure 2b: What data has been used for figure 2B?

P12921L11: Does it mean that runoff on map in Figure 3f was estimated by a Budyko function, rather than actual data?

C6002

P12921L21: rephrase sentence, avoid "caused" because this is just an estimate.

P12921L25: why is temperature sensitivity reported in °C and not as percentage % ?
In the moment one cannot compare the sensitivities and related attributed changes in runoff. This is related to Eq. 9. Please clarify and adapt.

P12927L13: What is meant by "small hydrology changes"?

P12928L8: unclear, please rephrase

Table 1: Variable z from logarithmic wind profile is not reported.

Table 3: a) column headers mistake b) report units c) Which period is considered for the changes d) Consistent with P and PET report absolute values of R

Figure 1b) only two test catchments are shown. Consider to highlight these test catchments in Fig 1a).

Figure 3: caption delete first wind speed

Figure 4: Do elasticities add up to 1?

Figure 5: Much too small to read! Increase size of plots. Maybe combine 1 and 2 panels by only showing significant catchments or using bold borders. The unit for the temperature trend seems wrong.

Figure 6: Use the same color legend for all panels!

Figure 7: Almost the same as in Yang et al., (2014)!

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 12911, 2015.