Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-441-RC3, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Sensitivity of potential evapotranspiration to changes in climate variables for different climatic zones" *by* Danlu Guo et al.

Anonymous Referee #3

Received and published: 22 November 2016

1. General comments

The manuscript presents a sensitivity analysis of potential evaporation (PE) estimates to changes in climate variables by using two different PE formulations. This issue is clearly not novel but to my opinion, the wide range of climatic settings of the studied sites and the fact that no clear consensus emerged from the literature on this issue justify the proposed manuscript. The paper is easy to follow and the discussion is interesting and nicely put into perspective with other related recent studies. My main concern is on the likelihood of the way the authors dealt with sampling the climate perturbations and on the potential impact of these choices on the proposed sensitivity analysis. In principle, Sobol analyses should be applied on models with non-correlated inputs, which is not the case of PE climate inputs. This does not mean that the analysis proposed is wrong but that a careful attention should be paid on these correlations

Printer-friendly version

Discussion paper

and on the way they can be reduced/ taken into account. To shed light on this issue, I suggest the authors show the correlations between variables on the studied sites. The other related major comment is on the way the authors sampled the climate perturbations. As far as I understand, they sample individually the perturbation for each climate variable by ignoring the interactions between variables. This is a strong assumption since some perturbations are likely to be interdependent. For instance, RH is often estimated on the basis of dew-point and air temperatures. Consequently, the perturbations should concern dew-point temperature (or water vapour pressure) and air temperature rather than relative humidity and air temperature. Besides, the range of possible might be criticized since some perturbations might not be realistic (e.g. an increase of Rs will likely not be possible with a decrease in temperature).

2. Specific comments

- I suggest the authors change the term potential evapotranspiration into potential evaporation that is more consensual.

- There are some typos in the text, e.g. Priestly-Taylor is often used instead of Priestley-Taylor and Figure 1 includes many typos (equatorial, temperate).

- I suggest the authors include in the manuscript the equations of the two PE equations.

- Priestley-Taylor equation is simple and some results might be discussed on the basis of the equation directly (e.g. by deriving analytically sensitivity coefficients).

- The time period used as the baseline is relatively short and this might be helpful to give some information on the climatic specificity of the time period.

- Are wind speed and air temperature measured at 2m for all locations ?

- P.8 l.136 ET-related -> PET-related?

-P.14 l.260 “,.” -> “.”

- The distinction between ‘energy-limited’ and ‘water-limited’ sites is interesting but not clearly defined: from the legends of Fig. 3-6, it appears that a studied site might be energy-limited for some months AND water-limited on some other month, which is non sense. From Fig 1, it appears that a given site is water-limited OR energy-limited. This need clarification and the threshold of aridity index value between the two classes should be defined.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-441, 2016.