

## ***Interactive comment on “Dominance of climate warming effects on recent drying trends over wet monsoon regions” by Chang-Eui Park et al.***

### **Anonymous Referee #1**

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This paper attempts to quantify relative importance of different climate drivers on dryness trends over continental East Asia. The authors find that the drying trends in arid regions are mostly explained by reduced precipitation but it is due to the increase in atmospheric water holding capacity in humid areas. While the topic that aims at understanding influence of different aspects of the climate on dryness is interesting, this paper has number of problems and is not of the quality acceptable for publication. My main concern is related to methods used in the study.

1. It is unclear how the computation is conducted. In particular, how did the author derive the numbers used in Fig. 1? Did they computed the station values first and then average over the region for PET and P separately or did they compute PET/P at individual station and then average over the region? The order of calculation would have an impact on the time series used to plot Fig 1a.

2. It is unclear how the statistical significance of the change point in Fig. 1 was determined. What kind of test for statistical significance was employed for equation (6)? Would the error term  $\epsilon$  in (5) follow a Gaussian distribution? More importantly, as the authors moving  $i$  in (4), the authors are conducting multiple tests. This means that the statistical significance would be incorrect if multiple testing (which the author did not mention) is not explicitly considered. Additionally, Fig. 1 does show long-term trend but the model (5) only considered a step function which is not correct. If a linear trend is considered in (5), would the authors still find a change point around 1980? Note that if there is a long-term trend in the series and if that trend is not considered in the change-point detection, one would always detect a change point in the middle of the time series. This is not useful and it seems that this is what the authors were doing. There is a body of climate literature discussing proper models and tests for the detection of change point but authors do not seem to be aware such studies.

3. The PET calculation (1) involves non-linear interactions among different drivers in particular wind, vapor pressure, and temperature. However, in order to derive the relative importance of different drivers, the authors simplified such interaction by using a linear regression (8). Is such simplification justified? Are the interactions among different drivers too small to be ignored? A proof or references supporting this approach is required. Also, are the regression estimated for individual stations separately or on the regional mean series? These details need to be clearly described for the work to be reproducible. Even if the interaction term among different variables to be small, the variables in (8) may not be independent (e.g., there must be some correlation between radiation and temperature, between temperature and humidity because a day of clear sky would correspond to high radiation, high temperature, and low relative humidity). So how did the authors test the significance of regression?

4. How did the authors estimate the confidence interval in Fig. 3?

5. Fig. 1 does not support the use of step regression of (5). It looks more like a long term trend with the last few years reversed that trend rather than an abrupt change

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in the 1980s. This would also invalidate the subsequent analyses regarding different impacts of precipitation and temperature change before and after 1980 as discussed in the paper.

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