

## ***Interactive comment on “Satellite based analysis of recent trends in the ecohydrology of a semi-arid region” by M. Gokmen et al.***

### **Anonymous Referee #1**

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#### Overall

This study shows a nice example to analyze actual ET trends and its impact factors in a semi-arid Turkey basin. This is also an important topic in Eco-hydrology, i.e. understanding the relationships between hydrological cycle and vegetation dynamics. The literature review has been nicely conducted. The methodology presented is overall sound, however is not clearly explained in some sections. The results are nicely presented, and reasonable discussion has been done. It is overall a good quality paper. I recommend publishing it in HESS, subjected to a minor to moderate revision, if authors can address my following comments and suggestion. General comments

1. Page 6202 lines 25-28: For the missing LAI input data during 2000-2002, the authors

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used formula by Wang et al. (2005) to estimate LAI from NDVI. I suggest the authors use this formula to estimate LAI for the period 2002-2010 as well, and then conduct a bias correction when compared to MODIS-LAI. This makes sure no biases caused.

2. Page 6203: The potential ET the authors calculated is not spatially explicit since they interpolate pan evaporation which is observed in 18 sites. They have gridded energy balance data. It is good to estimate potential ET using the Priestley-Taylor method which mainly considers energy balance. This method should give more accurate estimate on PET for each grid cell.

3. Quality codes on NDVI. MODIS NDVI/LAI data not only include data, but also quality code layer as well. It is not clearly if the quality codes are considered, i.e. data with poor quality codes excluded before the HANTS algorithm is applied.

4. Page 6207 section 3.3: This study does not quantitatively separate the anthropogenic effects from the climate-driven change in ET. It can estimate how much ET trends are contributed by anthropogenic effects and how much are contributed by climatic drivers. It is just a qualitative analysis across a large basin. This should be clarified in title.

5. In discussion. The author should discuss the limit of the current framework, i.e. the cause analysis of eco-hydrological variables – P, PET, ET, NDVI, LAI etc – is still qualitative.

6. In discussion. The author should discuss the limit of the data length. As the authors cite in introduction that the trend analysis for a short period is very useful if strong anthropogenic impacts are identified. Otherwise, the uncertainty for the regions not subjected to strong human influences should be discussed.

7. ET is calculated using remote sensing data including LAI, NDVI/fc, SM etc. It is surely that there exists a relationship between trends in ET and NDVI (as shown in Fig. 14), and between trends in ET and each of other RS data. Based on the model

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SEBS-SM the authors used I assume that ET is highly sensitive to SWR/LWR in energy limited region, i.e. in the south-west of the catchment, and is highly sensitive to SM and LAI in the eastern water-limited grid cells. Therefore, I suggest the authors conduct a sensitivity analysis for the key variables controlling ET processes.

Specific comments

1. Please rephrase the terminology 'separating' for anthropogenic and climatic impacts on ET. It is misunderstanding.
2. Page 6213 line 23 to Page6214 line 3. Please delete this paragraph since this research is nothing related to health of the ecosystems and it not necessary implication.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 6193, 2013.