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Hydrology and Earth System Sciences
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Discussions

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

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Response to Reviewers: In this letter we would like to respond to the questions of the reviewers.

Reflections to the comments of Reviewer # 1:

The authors would like to thank Reviewer 1 for his/her valuable and constructive comments. We have considered the comments of Reviewer 1, and hereby try to correspond to them within our knowledge. Based on the comments, we also made the necessary revisions in the manuscript, the quality of which has improved considerably. We hope that our replies and revisions will satisfy the Reviewer.

Anonymous Referee #1 (Received and published: 18 June 2013) 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 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.

Reply of the Authors (1):

To be able apply a bias removal procedure, we compared the two LAI inputs (i.e. LAI from MODIS and LAI from NDVI formula) for different months of 2010, when LAI could be derived both from MODIS and the NDVI formula. However, as shown in the Reply Figure 1, the scatter plot comparisons reveal that there is generally very low correlation and high variation between the two LAIs data, without any systematic over- or under-estimation. Hence we could not implement simple bias removal methods like linear rescaling between the two LAI data.

On the other hand, based on our sensitivity analysis of SEBS-SM to input parameters (the details are shown in our Reply #7), we found that LAI is not among the most sensitive input parameters. Therefore, considering the marginal effect of LAI on the ET estimation by SEBS-SM, we suggest not to carry out further bias-removal procedures.

[Figure 1 somewhere here] Fig. 1 Scatter plot comparisons of LAI-MODIS and LAI from NDVI formula on different months of the year in 2010.

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.

Reply of the Authors (2):

As the Reviewer pointed out, the interpolation of point-based PET trends is prone to errors spatially. The comparison of the original point-based results of PET trends and the interpolated are shown in the below figure. According to the Reply Figure 2, except for two stations (shown inside squares), both the original results by point data and the interpolated map agree on a clear spatial pattern of PET trends in the basin: increasing trend in the southwest of the basin, while no significant trend on the northeast side, with only exception on the east end. Quantitatively, the overall average of the PET increasing trend by the interpolated map was about 30% lower than the average of the stations that had significant increasing trend (Avg. PETinterpolated=9,2 mm y-1, PETpoint=13,8 mm y-1).

[Figure 2 somewhere here] Fig. 2 The comparison of the interpolated and the original point-based PET trends (both the significance and magnitude of trends). Note that, with respect to point data, dark green: significant increasing trend (p<0.1), light green: moderately significant increasing trend (p<0.25), dark red: significant decreasing trend (p<0.1), and grey: no significant trend. The numbers indicate the magnitude of the trend (mm yr-1)

Despite the errors due to the interpolation process, the main reason for assessing the PET trends from point based panA data was that it is a totally independent field-based data. If an energy balance-based PET method (e.g. Priestley-Taylor) was used, there would be overlap of some important input data for ET and PET (e.g. daily temperature,

net radiation) as both SEBS (for actual ET) and Priestly Taylor methods rely on energy balance principle. This would cause correlation problems in the inter-comparison of the trends between ET and PET, which is already an issue for NDVI and ET as iterated by the comment #7 by the Reviewer.

Therefore, to avoid further complications in the inter-comparison of the trends, we suggest adding the following explanation to the end of the Discussion section (p.6212 line 25) about the errors caused by the interpolation of point-based PET data:

"Secondly, as the distribution of potential evapotranspiration (PET) was obtained from the point-base pan-A evaporation data of 18 meteorology stations (Fig. 1), there is certain errors attached to the interpolation of the point-based data. According to the inter-comparison of the results by the original point-base data and the interpolated map, the significance/signs of trends agreed on all the stations except two (i.e. Konya and Nigde stations in Fig. 1), and quantitatively, the spatial average of the PET increasing trend by the interpolated map was about 4.5 mm lower than the average of the stations that had significant increasing trend. In overall, such a difference of PET would cause an additional total PET of about 15 MCM in the significant change areas (in both energy- and water limited parts), which would still correspond to less than 10% the total ET increase in these areas (Table 2)"

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.

Reply of the Authors (3):

We applied the HANTS algorithm to the 16-daily NDVI product without considering the quality code layer. In HANTS algorithm, there is the option to reject certain data before applying the algorithm (e.g. based on some quality codes). However, the application of the HANTS algorithm is already meant to remove the "poor quality" data. Therefore, we did not need to consider the quality code layer.

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 clariïňAed in title.

Reply of the Authors (4):

We observe that the understanding of not quantitatively separating anthropogenic/climate driven changes is caused by not presenting the quantities of trends in PET and P (climate driven variables) in Fig 12, but only showing the distribution of significance of trends in ET, PET, and P.

The reason of not presenting a detailed assessment of quantitative P, PET trends was mainly due to the consideration that the distribution of significant P, PET trends were very limited in the water limited part (Konya plain polygon), the main focus of the study. To improve the quantitative assessment of separating the anthropogenic/climate driven changes:

- We firstly revised Fig. 12 (attached as Fig. 3 in the reply) to include the magnitude of PET, P (new figs 12c and 12d) trends,

- Secondly, we added a new Fig. 13 (attached as Fig. 4 in the reply) which combined the old Figs. 12c and 12d (became Figs. 13a and 13b) and histograms of ET, PET and P for the areas of significant ET trend ($p<0.25$), separately for water and energy limited parts (new Fig 13c and 13d), so that to show how much of the ET trends are explained by the climate related trends (P and PET) and the anthropogenic causes (the rest of ET trends).

- Finally, we added Table 2 (attached as Table 1 in the reply) which presents the quantitative summary of partitioning of the anthropogenic effects from the climate-driven change in ET (based on the histograms in Figs 13c and 13d).

-In accordance to the newly added Figs and Table, we revised the explanation in Section 3.3 in the manuscript (p.6208, starting with line 5 in the Discussion paper).

[Figure 3 somewhere here] Figure 12a. The distribution and direction of PET trends, b) the distribution and direction of P trends c) the magnitude of PET trends, d) the magnitude of P trends.

[Figure 4 somewhere here] Figure 13a. The cross-relation between ET vs. PET trends, b) cross-relation between ET vs. PET trends (Note that "N.S." represent "Not significant trend" in the legends of Figs. 13a and 13b), c) histograms of ET, PET and P trends for the energy-limited part (outside Konya-plain polygon), d) histograms of ET, PET and P trends for the water-limited part (inside Konya-plain polygon). Note that the histograms of all the three variables represents the areas with significant ET trend ($p<0.25$) both in the energy- and water limited parts.

[Table 1 somewhere here, attached as supplementary material] Table. The quantitative summary of ET, PET and P trends for the areas with significant ET trend ($p<0.25$) in the energy- and water-limited parts, separately.

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.

Reply of the Authors (5):

To address the limitation about the lacking quantitative cause analysis of eco-hydrological variables, please refer to our Reply #4 and the revised Section 3.3, where we present an improved quantitative partitioning of anthropogenic/climate driven trends, and cause analysis of eco-hydrological variables.

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 identiïňAed. Otherwise, the uncertainty for the regions not

subjected to strong human inïňĆuences should be discussed.

Reply of the Authors (6):

As the Reviewer required, we added below explanation to the Discussion section (starting with p.6211 lines 8) to elaborate on the possible implications of the data length limitation for the regions not subjected to strong human influences:

However, in case of applying such a framework for regions not subjected to strong human influences, one must pay special care on the length of data that allow the detection of trends with high statistical confidence (i.e. detection time). As stated by Leroy et al. (2008), it is obvious that the longer the time series, the easier it should be to distinguish a trend from natural variability (and measurement uncertainty), because shorter periods of record generally have small signal-to-noise (S/N) ratios (Allen et al., 1994). The strong timescale dependence of S/N ratios arises primarily because of the large decrease in noise amplitude as the period used for trend fitting increases (Santer et al., 2011). Based on a hypothetical dataset with certain statistical characteristics, Leroy et al. (2008) determined the minimum detection time as about 33 years for detecting a global warming signal of 0.2 K decade-1. Similarly, assessing the trend consistency over a range of timescales (from 10 to 32 years), Santer et al., 2011 states that multi-decadal records are required for identifying the human effect on the climate variables (e.g. temperature) with high statistical confidence.

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 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.

Reply of the Authors (7):

Conducting a sensitivity analysis for a complex model like SEBS is not straightforward because there are also inter-dependences between the input parameters (e.g. vegetation parameters influence the emissivity calculation and thereby Tsurf), which make it difficult to isolate the effect of individual parameters on the model outputs. There are already dedicated studies such as van der Kwast et al., 2009 and Gibson et al., 2011 who assessed the sensitivity of SEBS to important input parameters.

Still, as suggested by the Reviewer, we performed a sensitivity assessment for a number of key remote sensing (SWR, NDVI, LAI, Tsurf, SM) and meteorological (Tair, Pressure, Wind) parameters on different seasons (spring and summer).

The below set of figures show the mean sensitivity of SEBS-SM model to the input parameters in spring (i.e. May) and summer (i.e. August) at basin scale, energy limited part and water-limited part, respectively. The figures indicates that, incoming radiation (SWR) and surface temperature (Tsurf), air temperature (Tair), pressure and wind are relatively more sensitive parameters irrespective of the season and energy- or water-limitation. The highest sensitivity to SWR, Tair, Tsurf and wind are as expected (see also: Su, 2002, van der Kwast et al., 2009, Gibson et al., 2011) because incoming radiation, temperature difference (Tsurf-Tair) and wind are the main drivers of heat fluxes. In comparison to these main drivers, sensitivity to the surface parameters such as vegetation (NDVI, LAI, fractional coverage-fc) and soil moisture are relatively secondary.

The figures also reveal that the magnitudes of the sensitivity increases in water-limited part for most parameters (especially in summer), while maintaining the same relative order between parameters. This is mainly caused by the fact that the magnitude of ET flux is lower in water-limited part, which causes higher relative difference for the same amount of absolute change.

Lastly, with respect to soil moisture, it can be observed that the sensitivity of SEBS-SM to soil moisture increases from spring to summer, and from energy-limited to water-limited part.

For spring: [Figure 5 somewhere here] Fig. The sensitivity of SEBS-SM to some input parameters in spring season (May).

For summer: [Figure 6 somewhere here] Fig. The sensitivity of SEBS-SM to some input parameters in summer season (August).

Note that, for estimating the sensitivity, each parameter (except for Tsurf, Tair and SM) was changed %25 one by one, and the corresponding percentage change in daily ET output was evaluated. The parameters Tsurf and Tair were only changed 2 degrees to be in the physical limits, while relative SM value was only changed 0.1. Each figure represent the average changes at basin scale, and also separately for energy-limited and water-limited parts.

Specific comments

1. Please rephrase the terminology 'separating' for anthropogenic and climatic impacts on ET. It is misunderstanding.

We propose to replace it with the term "partitioning" of climate-driven and human induced trends.

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.

The mentioned paragraph was removed in the revised manuscript.

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van der Kwast, J., Timmermans, W., Gieske, A., Su, Z., Olioso, A., Jia, L., et al. (2009). Evaluation of the Surface Energy Balance System (SEBS) applied to ASTER imagery with flux-measurements at the SPARC 2004 site (Barrax, Spain). Hydrology and Earth System Sciences, 13, 1337–1347.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C3833/2013/hessd-10-C3833-2013-supplement.zip

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

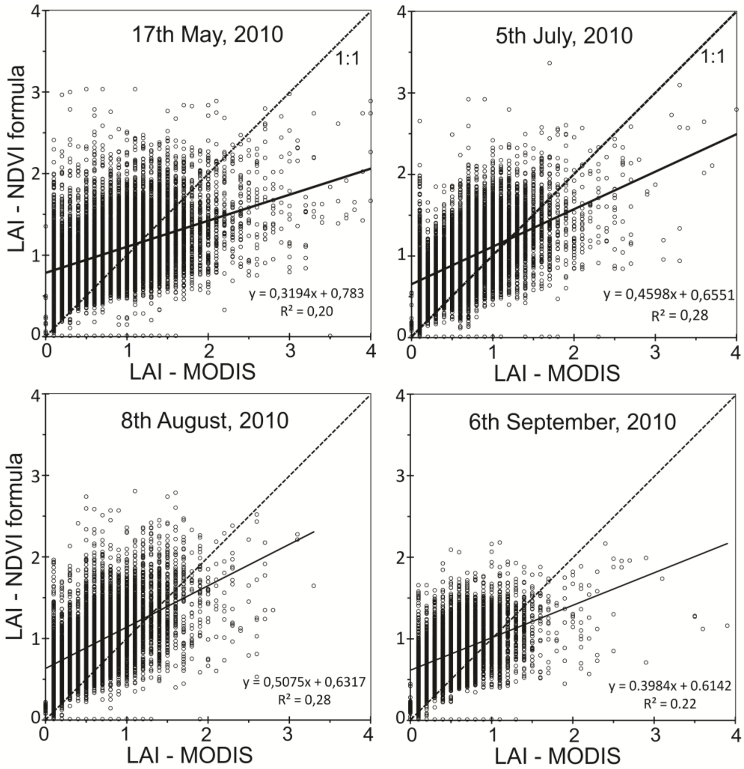

Fig. 1. Scatter plot comparisons of LAI-MODIS and LAI from NDVI formula on different months of the year in 2010

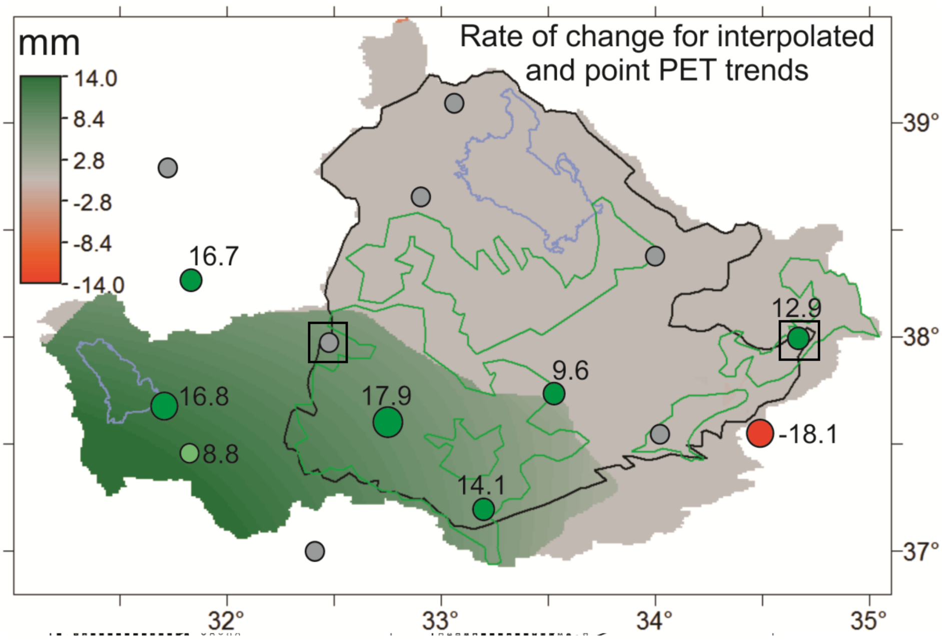

Fig. 2. The comparison of the interpolated and the original point-based PET trends (both the significance and magnitude of trends)

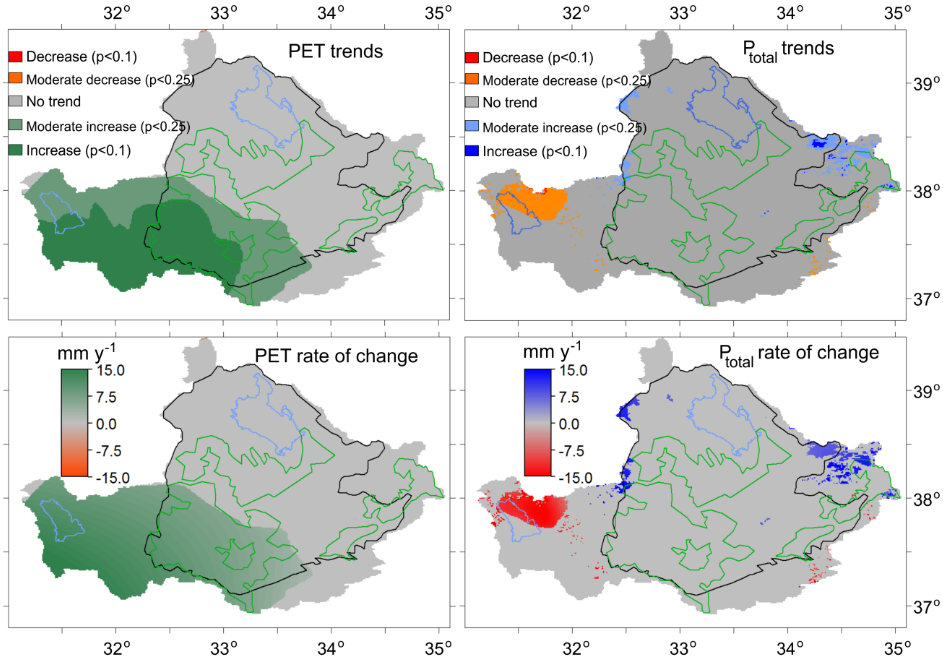

Fig. 3. The distribution and direction of PET trends, b) the distribution and direction of P trends c) the magnitude of PET trends, d) the magnitude of P trends

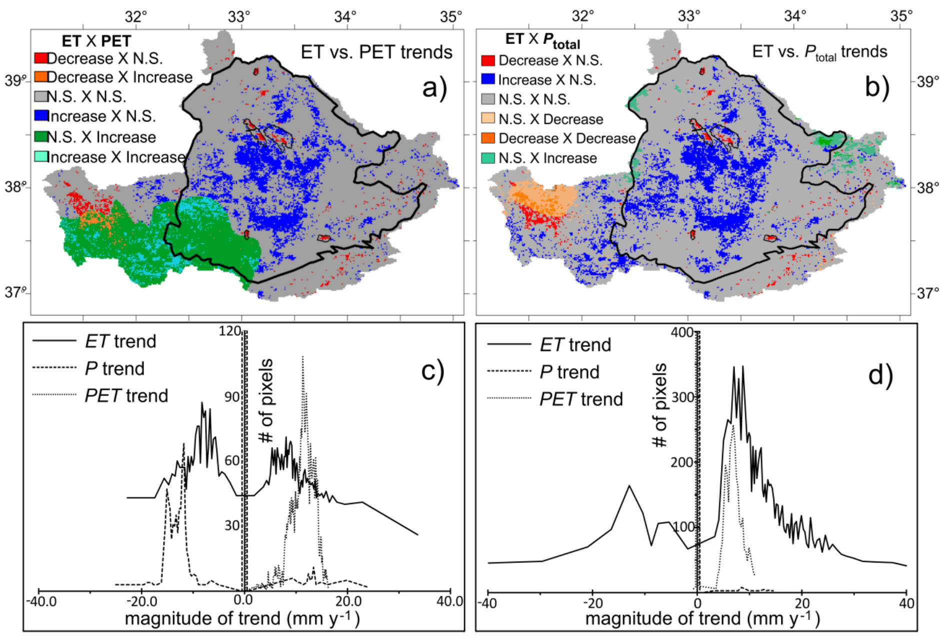

Fig. 4. The cross-relation between ET vs. PET trends, b) cross-relation between ET vs. PET trends, c) histograms of ET, PET and P trends for the energy-limited part, d) histograms of ET, PET and P trends for

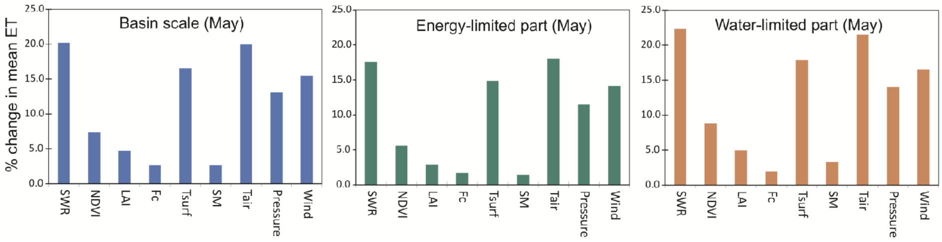

Fig. 5. The sensitivity of SEBS-SM to some input parameters in spring season (May)

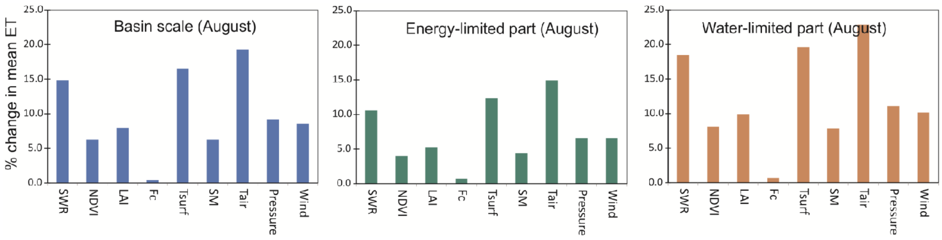

Fig. 6. The sensitivity of SEBS-SM to some input parameters in summer season (August)