RESPONSE TO REVIEWER #2

We thank the reviewer for the constructive comments and have carried out revisions of the manuscript in light of the feedback. Our response is detailed below.

Major remarks:

The authors present an interesting approach to estimate trends in evapotranspiration (ET). The paper is a valuable contribution to the topic of on-going changes in the hydrological cycle. Especially for ET such changes are rather difficult to quantify, as ET itself is difficult to evaluate on the large scale. The paper is well structured and written concisely. But there is currently one problem in the methodical approach of defining energy- and water-limited basins, which are designated as wet and dry basins, respectively. These are classified according to their value of basin-averaged the potential evapotranspiration divided by precipitation.

1) On one hand, precipitation data are used that are not corrected for precipitation undercatch. This is especially important in mountainous areas and areas with snowfall such as the high latitudes. Consequently, this leads to more dry basins than there actually are. This is probably the case for the erroneous classification, e.g., of several high latitude catchments (Mackenzie, Kolyma….) which are located in areas that are usually classified as energy limited (see, e.g., Fig. 1, Teuling et al. 2009). The authors claim (see, e.g., Sect. 2.2) that they use two independent precipitation datasets (both uncorrected). This statement is wrong as they are both derived from gauge measurements that overlap in many areas. Here, it would make much more sense to use one uncorrected dataset and one corrected dataset, such as GPCP data (Adler et al., 2003) or the WATCH forcing data (Weedon et al. 2011).

> Whilst revising the manuscript we discovered an error in the input data used to calculate potential ET, which led to the erroneous classification of high-latitude basins. This has now been corrected and all high-latitude basins are consequently classified as 'wet'. The classification into wet and dry is now more realistic and only subtropical basins have been classified as dry. All the results and affected figures in the manuscript have been revised accordingly.
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> We disagree that it is "wrong" to state the two precipitation datasets are independent. It is true they are partly based on the same gauge measurements (as are most precipitation datasets, in addition to combining them with satellite data in some cases) but they use a different number of gauges (GPCC "considerably more"; Harris et al., in press) and interpolation techniques to create a global surface. At the basin scale, we found them to disagree on the sign of trend in a number of basins, in addition to showing widely varying patterns and magnitudes of interannual variability in some basins. Kauffeldt et al. (2013) also state that while corrected WATCH precipitation data was generally found to perform better (e.g. in terms of more realistic runoff ratios), the spatial patterns between corrected and uncorrected precipitation were nevertheless similar.
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> Nevertheless, we analysed the (CRU-based) WATCH precipitation forcing

dataset as suggested but found little effect on our results. Basin classification was unchanged, with the exception of two small subtropical basins. We re-ran the multiple regression analyses used to attribute ET trends and whilst R^2 was higher by 10% in wet basins, the same predictor variables were flagged up as significant. In dry basins, the results were virtually identical to CRU-based ET and precipitation. The proportion of interannual variability accounted for was also very similar to CRU and GPCC in both wet and dry basins.

2) On the other hand, an estimate of potential evapotranspiration is used that only depends on temperature and cloud cover. The choice of this approach should be thoroughly discussed, and it should be justified why the authors don't use a more sophisticated approach such as the Penman-Monteith formula that is often used. Necessary data can be taken from reanalysis or Weedon et al. (2011).

The Priestley-Taylor method has been shown to be appropriate for large-scale potential ET estimates by Raupach (2000, 2001) whose work demonstrated how the Priestley-Taylor relation arises for large-area ET, based on modelling of the convective boundary layer. Penman-Monteith is appropriate for estimation of ET at a point, when vapour pressure deficit can be considered as an external variable, whereas the Priestley-Taylor method is appropriate for large-area ET, as the mean vapour pressure deficit over a large area is a function of ET itself is modified by convective boundary layer growth. Other papers (e.g. Guerschman et al., 2009; Zhang et al., 2004) have also adopted this method for catchment-scale studies.

Minor comments:

1) p. 5748 – line 18: The Mezen river is a rather small catchment and relatively unknown catchment. Please choose either a larger and more known catchment as an example, or indicate the Mezen basin in a map, e.g. in Fig. 1. I suggest indicating all basins specifically considered in the paper (in Fig. 3) in this map.

We have indicated the locations of the example basins in a revised Figure 1 as suggested.

2) p. 5748 – line 20-21: These uncertainties could be better dealt with if also corrected precipitation data are used. See major remarks.

This comment is addressed in detail above.

3) p. 5750 – line 2-3, Fig. 4: Are these trends significant? In line 7 you mention that only part of the trends are significant. In my opinion you should only show those trends that are significant. And you should state which measure of statistical significance you are using!

Only a small number of basins have statistically significant trends for certain variables and as such we believe the maps would not be very informative or show large scale patterns if only significant trends were shown. However, in

response to the comment we have revised the figure to show significant basins in shading. We would like to point out that our analysis does not require trends to be individually significant as our approach is based on two *groups* of basins (wet and dry) that may exhibit significant trends and relationships to different drivers, even if individual basins don't.

4) p. 5752 – line 8+: Please discuss also the results of Mercado et al. (2009) in the context of your study. Even though plant photosynthesis tends to increase with irradiance, Mercado et al. (2009) pointed out that recent theoretical and observational studies have demonstrated that photosynthesis is also more efficient under diffuse light conditions. They estimated that variations in diffuse fraction, associated largely with the 'global dimming' period, enhanced the land carbon sink by approximately one-quarter between 1960 and 1999.

We believe this finding is only minimally relevant to our manuscript and whilst increased diffuse radiation has been found to enhance photosynthesis and land carbon sinks, it would only translate to a very weak second-order effect on ET.

5) p. 5763/65 - Fig. 2/4: The panel legends are much too small. For Fig. 4, also the panels are too small. Please improve!

Figure legends have been improved.

References:

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Harris, I., Jones, P. D., Osborn, T. J., and Lister, D. H.: Updated high-resolution grids of monthly climatic observations –the CRU TS3.10 Dataset, Int. J. Climatol., doi: 10.1002/joc.3711, in press.

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Raupach, M. R.: Equilibrium evaporation and the convective boundary layer, Boundary-Layer Meteorol., 96, 107-141, 2000.

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Zhang , L. K., Hickel, K., Dawes, W. R., Chiew, F. H. S., Western, A. W., and Briggs, P. R., A rational function approach for estimating mean annual evapotranspiration, Water Resources Research, 40, W02502, 2004.