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Interactive comment on "Climate change, re-/afforestation, and urbanisation impacts on evapotranspiration and streamflow in Europe" *by* Adriaan J. Teuling et al.

Anonymous Referee #1

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The paper by Teuling et al., describes a simple modelling study where a one-parameter Budyko model is used to simulate changes in evapotranspiration and streamflow for most of Europe. It uses datasets on changes in precipitation, potential evapotranspiration (as function of air temperature), and landuse change as input and then simulates the change with the model. By reading the title, I Initially thought that this study is an attribution study which uses insitu data of evapotranspiration or runoff to attribute observed change to changes in climate or land use. But this is rather not the case, this study simply simulates changes and therefore the results should be discussed more cautiously.

Since the forest cover effect is hardcoded in the Budyko model, it will simulate changes in ET. However, this remains an extrapolation, which needs a better validation than what is now presented. The authors mention that the modelled ET average agrees well with the patters of GLEAM. Here I would like to ask the the authors to report statistics of this comparison. Then they also report a correlation with streamflow changes of r = 0.34, which corresponds to an explained variance of 12%, leaving 88% unexplained! Please show the scatterplot. Since there is a need for a validation of the model, I think that the model should be able to predict the observed streamflow changes better than a reference, for example using the changes in precipitation and maybe PET. Only if the Budyko model shows a higher skill I see justification to use that model and its change of the landuse parameterisation for the whole of Europe with confidence.

Land-use change is modelled by changing the parameter in the Budyko model using data from lysimeters. This is quite a central methodological step and ignores differences in scale of a lysimeter with that of a heterogeneous landscape. It also ignores that the parameter in the Budyko model can be different due to climatic variation, in particular the seasonality of rainfall to that of evaporative demand and the rainfall frequency. Jaramillo et al., 2018 HESS showed that there are increases in evaporative fraction, not explained by climate for many catchments in Sweden. Yet the link to changes in forest properties was rather weak. In contrast this study prescribes a distinct effect of forest age, hence there is a strong tendency that this study assesses the upper range of changes in water balance (if the HILDA database actually reflects the changes).

The choice of Thornthwaite method for PET is not acceptable for various reasons: a) It underestimates the evaporative demand (PET or Rn/L, see also van der Schrier 2011 or Maes et al., 2018). An annual average of PET of 700mm/yr for Southern Europe is far to low. That is why the authors need to scale it by an arbitrary factor aPET in the Budyko curve. b) Since it is a function of temperature only, it will be overly sensitive to warming trends which is arguably pretty strong for the considered period. It also

misses changes in shortwave solar radiation, see e.g. Wild et al., 2007. c) The authors argue for Thornthwaite because of data availability. However, there is data on sunshine duration / cloud cover. Furthermore, the diurnal temperature range correlates with solar radiation and has been used as a proxy for this, e.g. Wild et al., 2007, Makowski et al. 2008.

Apart from these major issues I enjoyed reading the paper. It is very well written, is well structured and has appealing figures. The topic is of high relevance for HESS. However, I believe that the validity of the Budyko approach needs to be demonstrated and therefore I recommend major revisions.

Minor Remarks:

Introduction, L20ff: it is argued that there are no sufficient studies which treat both landuse change and climate change on streamflow / ET. However, there are studies which indeed try to accomplish this, which I want to bring to the attention of the authors. For example Jaramillo et al., 2018 assessed changes in multiple catchments in Sweden. Renner et al., 2014 assessed observed changes of streamflow in East Germany. Lopez-Moreno et al., 2011 for catchments in Spain.

Figure 3: color of missing values (NA) should not be white, as indicated in the legend

Figures 6,7: there should be a color legend, a 3D color scheme on a map is a beautiful drawing but really difficult to grasp. What is the meaning of grey here? Similar magnitude of all drivers or a missing value? To what reference are the data scaled 2-98%, all of Europe?

The choice of rectangular sub-regions seems arbitrary to me. Why not use relevant river basins, where data is available to see if your prediction is indeed pointing in the right direction. For example on P9L10 it is mentioned that Scotland shows dramatic increases in streamflow, is this finding supported by observed changes?

Table 3: The units in the caption should be km^3/yr and not km/yr. In any case I would

prefer fluxes per unit area to allow comparison. Further I think that the total changes in Q / ET should be reported, not just the contributions.

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