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Hydrology and Earth System Sciences
Discussions



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

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We appreciate the detailed review by Anonymous Referee 1 and his/her generally positive impression of our work. Here we briefly respond to the more significant issues raised in the hope of some further discussion.

The reviewers' main comment is that the model requires a better validation. While our main approach was to present a framework that is constrained by observations in all aspects (in fact we are to first to combine available long-term observations made at lysimeter stations across Europe within a modelling framework – a fact that we will emphasize more in a revised version), we agree with the request for a more quantitative



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validation against GLEAM ET and observed streamflow changes. We will look into this as soon as possible. But a perfect match or high explained variance should not be expected because a) our model has higher spatial resolution forcing input, b) our model simulates land use features such as cities that might not be well captured in other products (it should be noted that GLEAM is also a model product rather than an observation), and c) validation against a higher order statistic such as streamflow changes is more challenging (in fact very few studies do this) because changes might be induced by changes in measurement protocol, rating curve, weir conditions etc that are not captured by the model, so no high model performance should be expected (but we agree that it should be higher than observed P).

A second comment relates to the use of the Thornthwaite model for potential ET. We fully agree with the referee that this method should not be a default choice for modelling studies, and we have been explicit in pointing out the limitations of the method in the Methods and Discussion sections. The main reasons why we think the use is acceptable in this case are a) we prioritize maximization of the simulation period since the most significant land use changes occurred before the satellite era, and no gridded observational datasets are available that allow for any other than temperature-based PET methods to be applied before the 1950s, and b) the impact of changes in PET on streamflow and ET was found to be smaller than land use and precipitation changes, and since Thornthwaite likely over- rather than underestimates the true trend since the 1950s (i.e. before the strong dimming period), using another method will not affect these results. The referee indicates that “However, there is data on sunshine duration / cloud cover”, but to our best knowledge such observational datasets are not available in gridded form from the 1950s onwards. However we do take the comments on the PET seriously and will thoroughly check our codes to ensure the values for southern Europe are at least correctly modelled.

The referee also stresses the impact of forest age on ET and movements in the Budyko space, citing the work of Jaramillo. We fully agree with this observation, which is also





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why we decided to include the effect of stand age (see Figure 1). Maybe the referee missed this aspect of our model. Based on previous work on analysis of long-term data from forested lysimeters (Teuling et al., Vadose Zone J 2018, Fig. 5), it was concluded that the most significant changes in ET from forests occur between 0 and 20 years. Hence, we used 3 different parameters for each 10 years in line with the temporal resolution of the HILDA land use dataset (also 10 years). The work by Jaramillo, which we missed in the initial submission, will be referred to in a revised version as motivation for our approach to distinguish 3 different values for the Budyko parameter based on stand age.

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