

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

### **Anonymous Referee #3**

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I see in the article some interesting aspects that contribute to the literature such as the constraining of the ET from the Budyko model by specific land use-dependent lysimetric data and a detailed analysis of land use changes across Europe, to calculate changes in ET and R. However, I found several weaknesses of the current version that need to be addressed.

1. You say you constrain the  $w$  parameter of Eq. 3 with observations of different land types. If I understand correctly, you constrain the  $w$  parameter in the locations where you have lysimetric measurements of ET and data on PET and P, and then apply that same  $w$  across all the spatial area of that specific land use/cover in combination with

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local PET and P data to obtain local ET rates across that land use/cover extension. But the lysimetric observations as you mention, are located in a very small area of Europe. I think that extrapolating those  $w$  parameter values to regions like Northern Scandinavia and Iberian Peninsula and other Mediterranean areas is unrealistic. Can't you rely on the work by (Sterling et al., 2013) to improve that constraining exercise or other databases of ET rates? I also think that the land use categories used are to course and omit others such as open-water areas or reservoirs, etc.

2. I know that the authors are aware of that (Page 10, line 30), but I see that there is no differentiation between irrigated and non-irrigated agriculture. Studies have found continental (Wang and Hejazi, 2011) and worldwide (Jaramillo and Destouni, 2015) driving effects from irrigation on long term ET and ET/P, from a Budyko perspective, and that are evident even at the large-basin scale. I think that a differentiation between irrigated and non-irrigated crops is compulsory for the constraining of the  $w$  parameter and the estimation of ET for land use/covers. In the same way, I would say that some of the attribution to reforestation in Southern Europe can be actually irrigation or rain fed agricultural intensification. Please check.

3. Why are the authors using the blue/green water framework, if they are also combining the terminology with fluxes, etc. For instance, they use across the text the terms blue water, runoff, water yield (Page 2 line 6), which appear to be referring to the same. The manuscript needs to be consistent in this way and I would say that green and blue terminology is relevant only when water consumption is being assessed. If not in agreement, please justify the use of such terminology and also cite the main source for such (Falkenmark, 1997).

4. It appears that an impact on long-term water partitioning from less now cannot be neglected that easily as stated in Page 6. See (Berghuijs et al., 2014) that also uses a Budyko approach. So at least acknowledge that uncertainty.

5. The authors justify their work "In spite of the direct link between average green and

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blue water fluxes, few studies have addressed changes in both fluxes simultaneously. However, they omitted many works precisely doing that: (Rost et al., 2008; Siebert and Döll, 2010). I also find missing important works on the effects of forest change across Europe and from a Budyko framework perspective that have been omitted here (Jaramillo et al., 2018; Renner et al., 2014). These four studies would for sure enrich the discussion in relation to the attribution of the observed R and ET changes to forest change in Europe. Their findings should also support several of the statements expressed by the authors and interpreted from their results.

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