

# ***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 thank the reviewer for the useful and constrictive comments, which will help us to improve the manuscript further. Below are some quick replies to some of the issues raised.

1. We agree there is a considerable degree of uncertainty in the exact value of the Budyko parameters used. However we also believe that constraining our model on urban data from 2 sites (the reviewer erroneously noted that we used a single site only, but Table 1 is spread over 2 pages and Rotterdam is in fact listed as

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- a second urban site) is better than not constraining it at all and assuming that model parameters are known which many other studies do. The low value of the Budyko parameter for urban land use is also in line with the common knowledge that urbanisation leads to a considerable increase in runoff. We will discuss our results considering the literature mentioned by the reviewer in a revised version.
2. The reviewer mentions that “the comparison to GLEAM cannot be viewed as a strict validation since GLEAM relies on hydrological modelling”. We respectfully disagree. Not because we disagree with the observations that GLEAM is largely a model product, but with the fact that validation can also be performed on model simulations (of course this is generally less useful, but it classifies as validation nonetheless). We added the comparison because GLEAM is a de-facto standard of gridded ET estimates, so we believe it is relevant to confront our simulations with GLEAM. But we fully agree that this comparison should not be over-interpreted. In fact, we believe our model produces more realistic estimates over urban areas because of the higher resolution (1 km) and the fact that our Budyko parameter for urban areas has been constrained by observations of actual ET over cities. We will also revisit our validation with streamflow changes once we have redone our simulations with a different PET forcing (see replies to other reviews).
  3. The reviewer also states that “the impact of urbanization is probably the most sensitive land use change impact on hydrological processes and it is discussed in the early hydrological literature (Leopold, 1968)”. While we don’t disagree with the importance of changes in urban area on streamflow (in fact this was one of our main motivations to carry out the work) and we value the contribution of Leopold to this area, we believe the size of the impacts are comparable to impacts of deforestation, which have been subject of extensive study since the beginning of the 20th century (see for instance the early deforestation experiment conducted between 1910 and 1926 at Wagon Wheel Gap, Colorado). The suggestion to

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include a review on the history of attribution in hydrology is useful. We will make sure that the suggested references are included in a revision. Also we will consider the good suggestion of using a contingency table rather than correlation. In fact we had our reservations on whether the correlation was the best measure for the validation given the difference in units (note that the streamflow data used in Stahl et al. is not freely available, so we have to rely on the values of the changes per basin as used in the paper).

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