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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Trends in evapotranspiration and its drivers in Great Britain: 1961 to 2015" *by* Eleanor M. Blyth et al.

Anonymous Referee #2

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General comments

The paper analysed the evaporation trend and its drivers in Great Britain using the JULES model. I found the subject and research questions relevant for publication in HESS, and the discussion section is rich. However, I found the research design fundamentally flawed for answering the research questions posed. My main concerns are:

1. **Fixed land cover (and biomass?) over the study period.** The land-use change is fixed in the JULES model, but large scale agricultural abandonment and forest regrowth has occured in the Great Britain during the study period (19.8 million ha agricultural land in 1961, 17 million ha in 2005) (Rounsevell and Reay, 2009). Despite the fundamental role of land in partitioning the precipitation into runoff

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and evaporation, the study did not include any sensitivity analyses of the land-use change effect.

2. **Unreferenced key assumptions.** The authors identified interception as the main increasing evaporation component. However, this is based on modelled results and can be biased by for example how interception is modelled and the equation governing the spatial distribution of rainfall intensity (Eq. A1-A3 are all without references, assumptions of the type and spatial distribution of input rainfall on p. 41 are also not referenced or tested). Precipitation data are also subject to large uncertainties. While simulated hydrological fluxes are discussed in comparison to other published results, the trend detected can nevertheless be biased due to model equations and data uncertainties due to e.g., trends in precipitation patterns. The authors appear to be aware of related issues, through e.g., reference to this in the introduction (p. 3 l. 29), but do not discuss or analyse further how different assumptions might interfere with trend analyses. (The authors compare their output with other modelling estimates in Sect 2.3., but focus on evaporation quantities and not trends. Issues of different data use in e.g., GLEAM in different time periods that might compromise its usability for trend analyses are not raised.)

3. **Lack of consideration of alternative explanations.** The authors attribute the change in total evaporation to interception change and precipitation change. However, this is done solely by correlation with radiation and precipitation. It is not motivated why these two have been considered the two main drivers (p. 26 l. 3), despite that many studies in the past have studied the role of e.g., winds (McVicar et al., 2012), land-use change (Sterling et al., 2012), soil moisture limitation (Jung et al., 2010), and CO_2 fertilization on evaporation. The effect of CO_2 is discussed in Sect 4.6, but could better earn a place in a dedicated section on sensitivity analyses that is cross-referenced to where relevant. Understanding the importance of other factors beyond precipitation and radiation is critically

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important for answering research question 3.

4. **Sensitivity and uncertainty analyses could be insightful.** Due to these fundamental issues, rather than trying to answer all three research questions for the entire GB unconvincingly and inadequately, the study could potentially be more insightful if the authors instead tried to answer only one or two of the research questions (even for a more limited region if necessary), but thoroughly. A different research approach could for example have been to use a range of precipitation products, precipitation distribution assumptions, and evaporation modelling approaches to answer question 1-2, rather than simply relying on the CHESS results without testing its sensitivity to a range of different underlying assumptions. Which or what kind of assumptions might overturn the current conclusions?

Specific comments

The term “evaporative loss” is useful only in very small systems where the moisture return to other terrestrial areas are of no importance (e.g., loss from a small aquaculture pond), but confusing when used in large natural systems where moisture recycling can occur.

The term “evapotranspiration” could be replaced by total evaporation, see also (Savenije, 2004).

PET could preferably be written as E_{pot}.

Given the central role of trend for the paper, I would suggest the authors to report trend detecting methods and significance more thoroughly.

Sect. 2.2 and 2.3 could go to the Appendix.

Sect 2.4 Can the discussed biases here influence estimates in trends? Perhaps

refer back to this in the discussion section?

P. 21 L. 2 “four” - > “three”

Sect 3.4. Please specify the sample size and p-value of the correlation.

P. 28 Perhaps just briefly remind the reader what type of methods the referenced publications used to arrive at their trend estimates.

The presentation of the results is repetitive and can be further condensed. Please also double-check that all abbreviations and notations are explained.

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