

Interactive comment on “Trends in evaporative demand in Great Britain using high-resolution meteorological data” by E. L. Robinson et al.

E. L. Robinson et al.

emrobi@ceh.ac.uk

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The review starts by suggesting we change the title of the paper from ‘evaporative demand’ to ‘atmospheric evaporative demand’ and suggests the paper needs a ‘total major overhauling revision’. This is not justified by the scientific criticisms of the paper and we therefore intend to keep the structure broadly as it is. While we will implement some changes as suggested in this review, many of the comments are merely a different definition or presentation of the material and we believe some are unnecessary. We will address the comments and suggestions individually as follows.

1) Point 1 argues that there is a difference between Reference Crop Evaporation and Potential Evaporation – of course there is if you define it to be so. Potential Evaporation is a concept, not a real thing, and depends on the definition. We have been very careful

C1

to define which Potential Evaporation we mean; calculated using Penman-Monteith for a reference crop surface. This is not a scientific issue, just an issue of definition.

It is also suggested we need to recalculate the potential evaporation accommodating soil moisture status – however, this would no longer be an estimate of evaporative demand, rather it would be an estimate of actual evaporation, which is not in the scope of this study.

2) The aim of this study is to quantify the change in evaporative demand, i.e. a function of the meteorology, not land use change or vegetation response. Therefore we use a constant standard reference surface and investigate only the effect of changing meteorology.

The use of the Penman equation (point 2) would be inappropriate as an estimate of evaporative demand as it does not include the wind-humidity deficit demand.

In response to point 2a, the input data were only available to 2012, therefore we were unable to extend this dataset to more recent years. We intend to extend the dataset to 2015 when all of the inputs are available.

3) Again, we are investigating the effect of changing meteorology, not land use/albedo change. This would be an interesting study in itself, but is outside of the scope of this paper.

4) The data are ultimately derived from station data, as is made clear in the paper. So it is not very helpful to compare the data with the station data used as input – they are similar by design.

5) It is not clear how calculating PenPan evaporation would help to ‘assess the accuracy of the grids’. Again, PenPan is a different estimate of evaporative demand, and is not necessary as a validation of the PM potential evaporation that we are using.

6) Ecology is part of the Earth System, and this is a regional study (many of which have appeared in HESS), so we see no problem with these references which already

C2

cover both hydrology and ecology (i.e. not just ecology). Readers will come with some knowledge of the larger context (that the UK is not alone in experiencing these changes) and we do not feel that more international references will help to introduce this regional study.

7) We will change 'physical drivers' to 'climate drivers', as this study does not include the effects of land use change etc.

8) Points 8 and 9 suggest a restructuring of the paper based on 'Objectives'. We believe that we have structured the paper in a relevant and readable way (note that referee #1 and Dr Weedon have both commented that the paper is well structured). We have made some changes, but do not think that it is necessary to structure the paper in the way that is suggested here.

9) See point 8.

10) Yes, we will rewrite this to make it clearer.

11) Hours of bright sunshine definition will be included. We will change rainfall to precipitation throughout.

12) We will clarify this.

13) This is a standard lapse rate (specifically used by the MetOffice in calculating MORECS), which we have applied here. For future studies it may be a useful extension to consider temporally varying lapse rates, but it was not deemed necessary here.

14) For historical reasons, the code used to create the specific humidity uses a constant air pressure of 100kPa rather than air pressure from Sect 2.8. The difference it makes to the air pressure has been checked for a subset of the data and was found to be small (of the order of a few percent), particularly in lowland areas where the air pressure is close to 100kPa. For any future updates of the data we will revise the procedure to use the varying air pressure.

C3

15) These coefficients vary spatially and by month. They are available from the Met Office, and are reproduced in the originating paper (Cowley, 1978).

16) We would like to have been able to use the daily extremes but MORECS only provides daily mean air temperature, not the minimum and maximum, so we are unable to do this.

17) Indeed, the two models mentioned (Dilley and O'Brien, 1998; Prata, 1996) were used for the clear sky component, and we have calculated the longwave radiation component from cloud cover following Kimball et al. (1982). The subsequent sentence (P7 L6-8) states this.

18) This is an interesting point, but since we do not have any information about the spatial and temporal variation of lapse rates in GB, we use the standard one.

19) Reference will be added.

20) The soil heat flux is generally negligible over time periods of a few days, as stated in Allen et al. (1998):

"As the magnitude of the day or ten-day soil heat flux beneath the grass reference surface is relatively small, it may be ignored and thus:

$G_{day} \approx 0$ (42)"

21) Again, we are interested in this particular definition of potential evapotranspiration, and are not investigating land use or land use change.

22) We are not sure what the intention of this comment is, but thank you for the references.

23) We will update this with a more appropriate reference.

24) This is again interesting, but we are not sure what this comment is asking for.

25) Yes, we will mention this.

C4

26) As noted previously, this is clearly a regional study (within the scope of HESS) and these references are appropriate. Readers will understand that similar effects (e.g. on biodiversity) could occur in other regions too. (See also point 4 above.)

27) We suggest that the final paragraph of the current "Discussion" section functions as a conclusion, so we will add the "Conclusion" title here.

28) True. We will add this to the text (although the negligible divergence of radiative fluxes between the surface and 1.2m means there is little impact of the information).

29) We will use a) to h) and refer to these in the caption/text. We will also alter the scales appropriately.

Areas with high precipitation but low specific humidity - yes, this is correct. These are higher, colder regions, with low saturated specific humidity. Even with high relative humidity the specific humidity remains low.

30) We will consider this.

31) Yes, we will provide these numbers.

32) The left-hand error bars with symbols are the slopes obtained from the linear regression. We have not explained this sufficiently well, so will rewrite the caption.

References

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C5

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C6