Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2015-520-RC2, 2016

Hydrology and Earth System Sciences Discussions
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Interactive comment

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

Anonymous Referee #2

Received and published: 23 February 2016

This study derives a new 1km x 1km, daily, gridded dataset of Penman-Monteith potential evapotranspiration (PET), and its input variables, over all of Great Britain for 1961-2012 using existing station-based observational products. It then looks at the trends in this PET, and the reasons for those trends. This is a very useful and insightful study.

However, the procedure for the annual-mean PET trends due to individual variables contains a major conceptual error (and possibly also some sort of numerical or units error), there are no maps of trends (a main purpose for such a dataset), and the procedure for obtaining 1km x 1km specific humidity from the coarser-resolution parent dataset has several potential problems. Therefore, I recommend major revisions. I also have a number of minor and technical comments, listed after these major issues.

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Major comments, beginning with the most important:

***p15 li10-11: It's not correct to multiply the annual mean of a derivative of PET with respect to a variable, by the annual-mean linear trend in that variable. That's because the derivative of PET may be much larger in, say, summer than in winter, causing the summertime trend in a variable to matter much more than its wintertime trend. The underlying mathematical issue is that annualmean(a*b) does not equal annualmean(a)*annualmean(b), because multiplication is not linear. To give a simple numerical example, suppose the derivative of PET w.r.t. X is 1 in winter and 3 in summer (annual mean of 2). And suppose the trend in X is +8 in winter and -6 in summer (annual mean of +1). In your method, the attributed PET trend would be 2*(+1) = +2, reflecting the sign in the annual-mean trend of X. But actually the attributed PET trend is 1*(+8) = +8 in winter but 3*(-6) = -18 in summer, so it is -5 in the annual mean, dominated by the summer trend in X. So both the sign and magnitude end up very incorrect here.

So you need to somehow do the multiplications in eq. 16 *before* you take annual means. Most straightforward would be to take just seasonal means of the PET derivatives rather than annual means (and use your linear regressions of the *seasonal* means of the variables for the rates of change), compute each product in eq. 16 for each season, and then average each product over the four seasons. This is not perfect, since the derivatives will still vary even within a season - but the variation would be much less than the variation over the whole annual cycle and you will have quashed the main potential source of error. You may get very different results after doing this.

All of the above also applies to aerodynamic PET (EPA) and radiative PET (EPR), of course. In particular, I bet this is why you're getting such huge differences between the actual EPA trends (leftmost symbols) and the sum-of-terms estimates (second from left) in the right-hand panel of Fig. 11 - you can particularly see this for England and its lowlands. [And same for PET trends, since EPA+EPR=PET.] The actual EPA trends show that the key statements at p15 li19 and p15 li28 (and thus perhaps

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p16 li9,10,13,18,etc) aren't so solid - e.g. for the English lowlands the actual EPA and EPR trends look practically equal! This is also clear in Fig 10. So you need to change these sentences.

[In fact, all of this nonlinearity stuff also applies *spatially*, not just temporally, especially if there are large spatial contrasts in the PET derivatives like the large seasonal contrasts (you should check whether there are.) So, strictly, it's best to compute the products in eq. 16 separately at each 1x1km point, rather than plugging in regional means. This would require finding linear trends of the seasonal-mean input variables at each 1x1 point, but is probably worth it, and will probably improve the attributions even further.]

Because of this issue, I also think you should give percentages of the *actual* trend in Table 3, not percentages of the total attributed trend. This will both more honestly reflect any shortcomings of the attribution (e.g. when the percentages don't add to 100%, it will mean the attribution wasn't completely successful) and will shrink the oddly huge numbers in Table 3c, which stem from the very small magnitude of the total attributed EPA trend. (Of course, both of these problems may greatly diminish anyhow once you do the annual and spatial averaging in eq. 16 correctly, as described above - so this may not end up mattering so much.)

If in fact the total attributed EPA trend is *still* an order of magnitude less than the actual calculated EPA trend in England, English lowlands and Britain in Fig 11 even after fixing the averaging procedure as described above, then I would guess there is some error in the implementation of the EPA parts of eqs A1-A5, or even a units issue somewhere. It doesn't make sense for the magnitude discrepancy to be this large.

***General: It would be very good to leverage the full resolution of this dataset to make *maps* of the PET trends, not just trends in the geographic mean PET over very large regions as in Fig 8, 10, 11. Perhaps you could include a 5-panel figure showing maps of the trends in annual-mean PET, spring PET, summer PET, fall PET, and winter PET.

And/or a 4-panel figure showing maps of the trend in annual-mean PET, annual-mean PETI, annual-mean radiative PET, and annual-mean aerodynamic PET. (These are just examples; the point is to include trend maps of some sort.)

***Section 2.2: A couple of things seem wrong with this method as stated - first, p* should not be 100,000 Pa, but should be the local surface pressure discussed in section 2.8, which is much lower in high-elevation regions as you can see in Figure 1. (This also applies throughout section 3 where p* is used in the calculation of Penman-Monteith PET.) Second, the units on the vapor pressure lapse rate should be hPa/100m = Pa/m as stated in Hough and Jones (1997), not %/m as you write. I'm not sure whether this is a typo. (Or perhaps Hough and Jones are incorrect, and you are correcting them here?) In any case you should clarify your intent - it's not clear whether you lapsed by 0.025 %/m or 0.025 Pa/m.

More generally, is a vapor pressure lapse rate assumption the best way to extrapolate near-surface humidity in elevation in Britain? Since most parts are well within the boundary layer, and are not interacting with the free troposphere much, I would think qa (plus condensate) in Britain would be fairly well-mixed vertically at a location. So the most reasonable procedure would be to first turn each MORECS vapor pressure into a qa using eq.(1) with the local surface pressure, then horizontally interpolate the qa to 1km resolution using your bicubic spline method, and leave it at that (i.e. no elevation correction.) You would then have to apply a switch to eq.(2) to specify that "qs-qa" should be treated as zero if/when it goes nominally negative (this corresponds to higher elevation, low-qs locations that are above the LCL, or within the cloud layer. In those cases, most of the excess of "qa" above qs would go into cloud condensate.)

[In contrast I think the vapor pressure lapse rate idea comes from thinking about the free troposphere, where qa can more easily have vertical contrasts, and where RH (not qa) is the more slowly varying, vertically-conserved quantity.]

But if you have a good reason to use a vapor pressure lapse rate rather than a well-mixed qa framework (e.g. some earlier literature demonstrating that the former works well for surface conditions in Britain), you should say so. I am not an expert on this issue.

Minor comments:

page 5 lines 10-11: You write "The WFD and CRU TS 3.21 datasets were used where variables could not be calculated solely from MORECS", but the reader is left wondering where/when specifically was this true? What parts of the study used WFD and CRU derived data instead of MORECS, and for what variables? As written, the reader has no idea what to take away from this phrase. So you should be more specific, e.g. "The WFD and CRU TS 3.21 datasets were used for surface air pressure and DTR, since those two variables were not in MORECS" or similar.

p7 li17-19: Does this ever result in negative wind speed, if the topographic correction is negative and is added on a day when the wind speed is much weaker than the time-mean wind speed? If so, this is unphysical - you may instead want to make the topographic correction a ratio rather than a difference (i.e. compute ETSU(1km)/ETSU(40km) instead of ETSU(1km)-ETSU(40km)). Then the interpolated daily wind speed would be multiplied by this ratio. That way, negative values are not possible.

Section 2.7: Any reason why interpolation is not done for DTR, unlike all the other variables? If there is a reason, you should state it in the text.

p8 li9-10: Can you provide a bit more detail on what "lapse the air pressure from the WFD elevation to the 1 km resolution elevation using the temperature lapse rate" means? I don't ordinarily think of the temperature lapse rate as applying to pressure - instead, pressure behaves in the vertical according to the hydrostatic or hypsometric equation, dp = -density*g*dz or equivalently dlnp = -g/(rT)*dz. Is the Shuttleworth (2012) method some sort of fancy integral of this differential equation which takes into account the variation in T with height?

Fig 1: Why does the wind speed scale extend all the way to 10 m/s when there seem to be few or no points stronger than ∼5 m/s (or weaker than ∼3 m/s)? It's hard to see any spatial wind speed contrasts with the current scale, it all just looks green. In particular I can't see the positive correlation between elevation and wind speed (p8 li23) at all with this scale.

Fig 4: Since you discuss the difference in terms of % in the text rather than absolute millimeters per day, it may be better to plot the % difference in the last panel instead of the absolute difference.

Section 3-4 in general: Where is the DTR used?? Why was DTR derived and explained in section 2, if it has no role in the calculation or in the rest of the text at all? Yes, it is included in the figures and tables, but it's never discussed or mentioned in the text after section 2.

Table 2 and Section 4 vs Figs 9-11: The table and body text report trends per decade, but the figures report trends per year. This should be made consistent - pick one or the other.

Or, alternatively, you could just report per-51yr trends over the whole time period (like you helpfully provide at the end of p13 and beginning of p14) in all cases in the table, figures and body text, and skip the per year and per decade numbers completely! This would be perhaps easiest to interpret. The units would then be given as "mm d-1 (51 yr)-1" throughout the tables and figures.

p13 li21-23: I assume "long term" here means going back much further than 1961, since I certainly don't see "drying summers" in Fig 9's precipitation plot. It may be good to replace "long term" with a more specific (but still could be rough) time period, and to explicitly point out that summers don't have declining precip post 1961, so the reader is not confused. (Unless "drying" here refers to declines in something like P/PET or P-ET, not P.)

Figure 11 caption: You need to more clearly state that the leftmost symbols are the regular computed trends from Fig 10, while the second-to-leftmost symbols are the sums of all the pieces attributed to the different variables. I understood this but it should be explicit. See the major comment above.

Technical corrections and/or typos:

Eqs 2, 9, 10: Should be 1 + rs/ra in the denominator, not 1 - rs/ra. (Is this merely a typo, or is the implementation wrong in your product as well? I would assume not, but just wanted to check.)

p9 li9: Should be qa, not q (to be consistent with eq 2 and rest of paper.)

p9 li20 (eq 4): Last factor should be raised to the power i-1, not i. (Again, is this just a typo or also a code error?)

p10 li21-22: And also wind speed, of course. (That makes six.)

p14 end of li23: Should be PET, not P.

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