Hydrol. Earth Syst. Sci. Discuss., 10, C6408–C6410, 2013
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Hydrology and Earth System Sciences
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HESSD
10, C6408–C6410, 2013

Interactive Comment

Interactive comment on "Improving the complementary methods to estimate evapotranspiration under diverse climatic and physical conditions" *by* F. M. Anayah and J. J. Kaluarachchi

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Received and published: 20 November 2013

My comment on this manuscript does not strive to be comprehensive. I just list a few problematic issues.

1/ In lines 27-29, pg. 13598 the authors mention some previous studies that used the complementary methods (CM) with "little success" and they list two of my recent works I was the principal author of: Szilagyi and Kovacs 2010, 2011. I am totally confused because in these studies the application of the CM was a clear success, as anyone can

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check. They also list in this context the recent study by McMahon et al. (2013) who concluded that the CM-based ET estimation methods are the best available practical ET estimation methods.

2/ In the Penman equation the second, aerodynamic term accounts for local advection and not for "large scale advection effects" as the authors claim in line 21, pg. 13602. What accounts for large scale advection is the value of the Priestley-Taylor parameter, alpha, being larger than unity.

3/ In line 15, pg. 13604 the authors claim about the GG method that it does not need "surface parameters (temperature and vapor pressure)". I am asking them: which CM method asks for such values, because I am not aware of it, at least what concerns the CRAE or AA methods.

4/ It would have been much more informative to use a mean BIAS value, not an absolute one, to see where the models overestimate and where underestimate EC-derived ET rates. From the published BIAS values this cannot be deduced, since they are all positive values, yet the authors discuss under and overestimation of the different models under different climates before they do their analysis with the model-components.

5/ In line 28, pg. 13608 the authors say that the GG method has the lowest bias, but I do not think a value of 15.7 vs 15.5 marks a statistically significant difference, considering the errors in the EC measurements.

6/ In lines 23-27 the authors discuss the study of Szilagyi and Kovacs (2010) and they say that at the third EC site the CM-based model gave a difference of 44% in ET rates in comparison with EC measurements. Unfortunately, they do not tell the reason why, which when explained turns out to yield the best ET results of the three sites. As is discussed by Szilagyi and Kovacs (2010, 2011), at that site the EC instruments were installed on a radio-transmitter tower at a height of 82 (as in eighty-two) m above the ground. Under certain wind directions the instruments were in the wind-break of the tower making the method unusable in such periods. Consequently, the derived ET and sensible heat rates added up to 44% less than the energy balance. Accounting for it, the CM-based monthly ET estimates explained 95% of the variance found in the EC measurements, with practically no bias. And this leads us to the question of footprints. The 82 m height of the EC instruments above ground translates into a footprint really comparable to the scale of the CM-based ET estimation methods: most likely the reason for the best, unbiased performance in comparison with EC data.

7/ The CarboEurope site (Bugac) from Hungary, listed in Table 1 has a measurement height of less than 2 m above the ground. I am not familiar with the other sites listed in Table 1, but I would risk to say that they may have comparable heights (i.e. a few meters). I ask the authors to list these values in Table 1. If I am correct then the footprints of the majority of these sites are just a tiny fraction of the scale the CM-based ET rates represent. Since surface properties, soil moisture status, vegetation may vary significantly at this fine scale (a few hundred meters) how representative are they then at the scale of the CM-based method? In my opinion a better validation would have been for the CM-variants to use water-balance data for the involved catchments.

8/ I wish the climate of Hungary were Mediterranean as Table 1 claims. It is still continental despite all climate change claims.

9/ The winning GG18 variant is only slightly better than the original CRAE model. The R2 value is the same, the absolute BIAS value is 11 vs 15.7 mm/mo, and the RMSE value is about 20 vs. 27.8 mm/mo. Yet there is a big difference in input data requirements between the two models: the former (GG18) needs wind velocity measurements, while the CRAE model does not (every other model inputs are the same). So the CRAE model performs almost the same as the GG18 model with fewer data input. Wind data is something not at all universally available historically. I still wonder if the GG18 model would outperform the CRAE model with the help of watershed water balance data.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 13595, 2013.