Hydrol. Earth Syst. Sci. Discuss., 10, C1401–C1406, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C1401/2013/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Technical Note: A comparison of model and empirical measures of catchment scale effective energy and mass transfer" *by* C. Rasmussen and E. L. Gallo

Anonymous Referee #1

Received and published: 4 May 2013

1 General comments

This paper presents an analysis of the relationship between two methods of estimating a term called the "Effective Energy and Mass Transfer". Several relationships between this term and climate and land cover are also presented. This is a paper is within the scope of HESS but is of interest only to those using the EEMT term.

My main concern is with the ad-hoc nature of the EEMT term, which is described as an "energy and mass balance" but is clearly nothing of the sort (since it makes no attempt

to apply the principles of conservation of energy and mass and omits the inputs of solar radiation energy and outputs of sensible heat energy and runoff mass). I have made a number of specific suggestions below regarding how this terminology can be improved without requiring a re-definition of the term. I have an additional issue with the definition of the E_{PPT} term, but since this method has now been published in a number of places this is not a reasonable basis for rejecting the paper.

2 Specific comments

1. One concern is that the summary and abstract report that the two methods are significantly correlated, but do not quantitatively report the differences between them, which is the main result of the paper.

2. It isn't clear how Equation 1 serves as an energy and mass balance in the sense of being a statement of simultaneous conservation of energy and mass. Each E term has the units of energy flux. Assuming that conservation of mass and energy holds then their sum should be the total change in energy of the system. If steady state is assumed then they should sum to zero. The mass entering the critical zone from precipitation (which determines the E_{PPT} term) is balanced by the mass leaving by discharge and ET. The energy carried away by the ET (which determines the E_{ET} term) is supplied largely by the net radiation. Radiation does not appear in equation 1, nor does sensible heat flux, despite the fact that these plus the latent heat flux make up the majority of the energy balance as it is usually written.

However, my understanding is that this is not an energy balance in the traditional sense, but rather a quantification of something like a "gross flux". That is, it quantifies the rates of fluxes and NOT their balance. The authors must make this explicit to avoid confusion.

Moreover if that is so, then the consistent way to quantify the gross flux in a steady-state system would be to either sum the total energy and mass flux INTO the system, or the total energy and mass flux OUT of the system. Including both terms introduces ambiguity since a portion of the mass or energy introduced by one term is cancelled out by another. If it is not then the flux is double-counted. Equation 2 suggests that the flux IN is the focus. In that case the E_{ET} term should not appear in equation 1, since this is a flux out.

It is also clear though that the authors wish to exclude some of the gross fluxes. For instance if the total 'gross flux' is desired then the E_{Bio} term should be based on GROSS primary productivity not NET, and the E_{PPT} should be based on total precipitation, not PPT-ET. It seems there is an additional assumption being introduced that only the fraction of the gross fluxes of photosynthetic energy that are retained in the system rather than being rapidly transferred back to the atmosphere (i.e. respiration and transpiration) should be considered. This is fine but the reasoning should be clear and should be presented separately from the reasoning to only consider the flux in (or out) to avoid confusion.

It is also not immediately clear why the energy available from precipitation is proportional to the difference in temperature from a reference of zero. I can understand it if only differences in EEMT are considered (in the same sense that the datum for 'gravitational potential energy' is arbitrary since only gradients matter), except that the multiplication by the base flow introduces a confounding factor that precludes this. The thermal energy in the water is not available to the critical zone unless a heat sink at zero degrees C is available.

3. Why are we looking at E_{TOTAL} and the contribution of the ET component, if the definition of EEMT is based on the assumption that this part of the energy flux is not 'effective'? It isn't clear why the results in figure 3 are significant? 3a shows that ET is a much bigger energy flux than the others, but that is hardly surprising. 3b is 3a flipped upside-down. Surely 3c and d would be more relevant if the vertical axis were the ratio with EEMT rather than E_{TOTAL}?

3 Technical corrections

Page 3028:

Line 6: "Point-to-catchment-scale" is better in terms of clarity, but seems unnecessary and ambiguous. Isn't the long-term climate data representative of a catchment?

Line 13: Isn't the RMSE error or R-squared the main result of the paper, rather than the mere presence of a correlation between two methods of estimating EEMT? I'd suggest reporting these numbers in the abstract.

Line 24: I'm not sure what this sentence means. How does the existence of a 'strong correspondence' between the two methods 'agree' with the partitioning and plant cover?

Page 3029:

Line 5: "Recent Studies" needs a reference

Section 2.4

Given the above issues with Equation 1, it isn't clear whether equation 9 has physical meaning. E_{ET} is a flux OUT, and E_{PPT} is either a flux IN (if the 'ppt' part of its name is considered) or a flux OUT (if its calculation by base flow is considered).

Given that the ET term is included and 'PPT' term is actually calculated from base flow discharge, it seems that the best interpretation of EEMT is as a gross flux OUT of the system, in which case sensible heat flux should certainly be included in equation 1 and equation 9, since it is of a similar order of magnitude to latent heat flux. The authors could quite easily estimate it from a radiation balance H   Rn - LE.

Page 3035

Line 3: The focus on a 'strong linear correlation' and reporting p-values seems odd here, since a weak correlation would be surprising. Shouldn't the focus be on the RMS error and R-squared, since this is the error introduced to previous analyses from the original method (taking the presented method as the new 'gold standard')?

Line 5: "Linear"

Line 9: This sentence belongs in the conclusions?

Line 22: There is something wrong with this sentence. What function is the word 'or' playing?

Page 3036

Line 9: This conclusion doesn't follow from the results. The results in this paper show that the monthly water balance based $EEMT_{MODEL}$ used in previous work have an error of 4.68 MJ/m2/y [RMSE] compared to this more detailed estimate. The results have no bearing on the assertion that EEMT represents an upper bound on available energy and mass. This paragraph seems unnecessarily defensive.

Section 3.2

Line 19: It is again not true that evapotranspiration in general accounts for 99.5

Line 24: 'Work available to perform work'. Should be 'energy available to perform work'?

Line 11. If figure 3 is intended to confirm the previously-reported relationship between E_{Bio}/EEMT and aridity index, why present a plot of E_{Bio}/E_{TOTAL} and aridity index, especially when E_{TOTAL} is mainly dependent on E_{ET}?

Page 3037

Line 19: "These data confirm"

It seems like figure 4 represents a relationship between E_{TOTAL} and EEMT and woody plant cover, and do not "confirm" the previously-reported relationship between EEMT and water limited systems unless woody plant cover is assumed to be a good predictor

of water limitation.

Page 3038:

Again, the focus on the mere presence of a strong linear correlation between the two methods for estimating EEMT seems less important than the relative and absolute error between them. Why not conclude with something informative, like "the results indicate a relative average bias of X

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3027, 2013.