Referee #2

In the introduction it could be useful to resume what the critical zone is

- A brief definition of the critical zone will be added to the introduction.

Page 3030 line 9, why only heat and no mass term for rainfall? Page 3031 line 23, why in the model you have potential and not effective evapotranspiration? The problem is that you should overestimate even more. Why? Page 3035 line 5 linear, not liner.

- The term balance will be changed. The terms in this equation represent the fluxes from respective energy, water, carbon, and sediment balance important to subsurface critical zone evolution and will be described as such.
- As noted in in this manuscript an in previous manuscripts describing this model, the initial versions of this model were applied to broad regional scales where only coarse climate data were available
- Linear spelling error will be fixed.

In order to have an advancement with respect to the many papers of the authors on the same subject, I should like a more detailed water balance with respect to the quantities of actual ET and rainfall.

- The water balance used in this manuscript was described briefly here and in previous manuscripts describing and using the MOPEX data (Duan et al., 2006; Brooks et al. 2011; Rasmussen, 2012). A catchment mass balance approach to determining catchment scale water balance was applied as described in equations (5) and (6). The MOPEX data are reported elsewhere and available for download at http://www.nws.noaa.gov/oh/mopex/ and hence were not explicitly reported again here.

Referee #1

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 EPPT term, but since this method has now been published in a number of places this is not a reasonable basis for rejecting the paper.

- We strongly disagree with the characterization of EEMT as an ad hoc term. The main problem focused on by this reviewer was one of poor wording in our presentation of the EEMT and ETotal terms in this brief technical note. The EEMT framework was developed based on a rich history of this type of approach in the soil science literature from the initial conceptualization and semi-quantitative approaches describing soil forming factors (Dokuchaev 1886; Jenny 1941; Runge 1973; Smeck et al. 1983) that were formalized into quantitative energy terms by Volobuyev (1964) and later by Phillips (2009) and work in our group Rasmussen et al. (2005), Rasmussen and Tabor (2007), Rasmussen et al. (2011), Rasmussen (2012) and others (Sheldon and Tabor 2009; Gulbranson et al., 2011; Tabor et al., 2013). The ETotal term is the summation of energy fluxes associated with soil development, with soil expanded in this context to include subsurface critical zone development, and where development refers largely to chemical alteration, structural change, and layering/zonation/organization of the weathered regolith. We recognize this was not stated as clear as it should be in the text. Each term in Eq. (1) is derived from the respective energy, water, carbon, sediment, and geochemical balance equations that govern critical zone evolution – and represent the energy fluxes important to soil and subsurface development. Equation 1 is nearly identical to one proposed by Volobuyev (1964) and revisited by Minasny et al. (2008) and Rasmussen et al., (2011). The EEMT term provides ones means, or metric, to quantify the dominant energy fluxes driving soil forming equation, namely the chemical energy flux associated with reduced carbon compounds produced as a result of primary production and the heat energy that flows through the system with effective precipitation (this reviewer has some issues with the later term that we address below). This is not an ad hoc term – but one derived from the fundamental energy and mass balances that drive the functioning of the terrestrial system. The EEMT term has repeatedly proven to be highly correlated to subsurface critical zone properties (including depth, chemical weathering, soil development and taxonomy) and processes (including chemical weathering rates and carbon respiration) (see Rasmussen et al. papers and others referenced above). In addition, EEMT has been used effectively in a predictive sense in numerical models that predict soil depth and topographic development given rates of EEMT and uplift (Pelletier and Rasmussen 2009a; 2009b; Pelletier et al. 2011; Pelletier et al. 2013).

- Much of the above discussion has already been reported in great detail in previous papers – given this is a Technical Note meant to be a short reporting of significant findings relevant to the application of this modeling framework, we limited repeating much of this discussion here to keep the text brief. Clearly by doing that we did not fully and clearly convey the EEMT concept. This will be addressed upon revision.

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.

- RMSE was reported in the manuscript, but we agree that it should be played up and reported more front and center in the abstract and summary as this provides a the best test of the validity of previous modeling approaches to calculating EEMT relative to the catchment scale approach applied here.

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 EPPT term) is balanced by the mass leaving by discharge and ET. The energy carried away by the ET (which determines the EET 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 steadystate 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 cancelledout 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 EET 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 EBio term should *be based on GROSS primary productivity not NET, and the EPPT 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.*

- As addressed above, we agree that the statement introducing Eq. 1 was unclear in using the term balance. Equation 1 represents the sum of the fluxes associated with soil/subsurface critical zone formation. We will address this critical issue in revision. Additionally for clarity, we will state Eq. 1 without the EET term, but will present the original Volobuyev (1964) equation as summarized by Minasny et al. (2008): E = w1 + w2 + b1 + b2 + e1 + e2 + g + v, where E is the energy involved in soil formation, w1 is the energy of physical rock weathering, w2 is the energy for chemical weathering, b1 is the energy accumulating in soil organic matter, b2 is the energy for soil organic matter transformation, e1 is the energy for evaporation from soil surface, e2 is the energy for transpiration, g is the energy losses in leaching of salts and fine materials, and v is the energy expended by the process of heat exchange between the soil and atmosphere (usually negligible over the time scales of soil formation). We will then present equation 1 reformulated to include only the net energy flux terms driving subsurface development including the EBIO, EPPT, EELEV, and EGEO terms with very specific clarification that is the sum of net fluxes into the subsurface.

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.

- The model was initially conceived using the assumption that the majority of majority of precipitation in middle to high latitudes results from the ice-crystal process, such that the temperature of precipitation is at or below 0*C (ASCE, 1996) and is heated to air temperature as it enters the soil system – thus the heat content of the water entering the system was assumed equivalent to the mass of water times the specific heat of water and the temperature of the system at the time of rainfall. There is little to no data that provides the actual temperature of precipitation. We recognize that this estimate is only a proxy for heat energy carried through the system from precipitation and that the most accurate way to characterize heat transfer and exchange between soil and precipitation is to include detailed information on soil profile itself, characteristics such as clay, organic matter, and soil moisture content, at a relatively high temporal resolution. The model at its outset was intentionally developed independent of soil information so that it could be used to predict the development of exactly these types of soil properties that form over geologic time scales.

3. Why are we looking at ETOTAL 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 ETOTAL?

- Given the responses above we will modify this figure and present only 3c and d with the ratio relative to EEMT.

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?

- This will be fixed upon revision.

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.

- Agreed. This will be changed.

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?

- This sentence will be reworded for clarity.

Page 3029:

Line 5: "Recent Studies" needs a reference

- Will be addressed upon revision.

Section 2.4

Given the above issues with Equation 1, it isn't clear whether equation 9 has physical meaning. EET is a flux OUT, and EP P T 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.

- As noted above in the comments regarding equation 1 this equation will be modified to exclude the EET term and simply address the partitioning of EEMT to EBIO and EPPT as presented in previous work.

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')?

- Agreed. As noted above the RMSE was reported in the figures but will be explicitly included in the detail of the text.

Line 5: "Linear"

- Fixed.

Line 9: This sentence belongs in the conclusions?

- We feel this sentence is fine in this section as part of the discussion.

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

- Fixed.

Page 3036
Line 9: This conclusion doesn't follow from the results. The results in this paper show that the monthly water balance based EEMTMODEL 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.

- Agreed. This paragraph will be reworded and/or deleted upon revision.

Section 3.2
Line 19: It is again not true that evapotranspiration in general accounts for 99.5

- In terms of equation 1 it does. This will be clarified in the text in terms of the comments above regarding the nature of equation 1.

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

- Fixed.

Line 11. If figure 3 is intended to confirm the previously-reported relationship between EBio/EEMT and aridity index, why present a plot of EBio/ETOTAL and aridity index, especially when ETOTAL is mainly dependent on EET?

- Agreed. As noted above, the partitioning presented here will be modified to mainly deal with the partitioning of EEMT to EPPT and EBIO.

Page 3037
Line 19: "These data confirm"

- Fixed.

It seems like figure 4 represents a relationship between ETOTAL 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.

- It is assumed that woody plant cover is a good predictor of water limitation. An additional figure (4c) will be added here that explicitly plots a Budyko Curve of these data color scaled by woody plant cover that shows clear relationship of water availability to woody plant cover. Also, Rasmussen (2012) includes a Budyko curve color scaled to EEMT and FBio that shows clear relationships between water availability, EEMT, and FBio that correspond to the same patterns with woody plant cover observed here.

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.

- This will be addressed upon revision.

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