

Interactive comment on “Thermodynamic constraints on effective energy and mass transfer and catchment function” by C. Rasmussen

Anonymous Referee #2

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This paper builds upon earlier work by the author on open system thermodynamic principles and his EEMT metric. This paper's particular contribution is that it provides an understanding of the physical limits and interrelationships of the equations used to derive EEMT at regional scales and provides three specific physical limits for the current EEMT metric. This will be useful to others using EEMT as well as others developing similar metrics. In addition to the specific points raised below, there are three general areas that should be addressed in future versions:

Geographic context: While the paper explicitly discusses three physical limits for EEMT, it does not put them in context of commonly used life zones or biogeographical regions. In general the paper could be greatly improved by adding some simple text or informa-

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tion in graphs that converts units into metrics that are commonly used by most readers (i.e. K to C, precip in mm/day instead of $\text{kg m}^{-2} \text{s}^{-1}$). For example the explanation on top of page 7336 “These upper limits represent cold/dry and hot/dry conditions. . .” could be improved by providing some context. For example, where do Tibetan Plateau, Amazon, or Basin & Range of North America fit in the “292 K EEMT” world that is described in paper? Clarifying if the temperatures and precipitations are annual means or monthly values would also help the reader.

Carbon bias: P7324 L5 and elsewhere “the function and state of the critical zone may be directly related to fluxes of water, radiation, carbon, and sediment”. Paper should briefly acknowledge the potential of nutrient limitations on growth and EBIO; N and P and micro-nutrients have been shown to limit NPP in cases where water and radiation do not. Likewise, carbon production and flux are products of the interaction of water & radiation and not independent fluxes. The term “sediment” and “denudation” are used and are general enough that it could be expanded to include nutrients and address this concern.

Steady state bias; the data and analysis are based on estimated monthly values and relevant to “steady time frames” where landscapes are in equilibrium with forces acting upon them; the introduction should acknowledge this and the role of disturbances in sculpturing of landforms via landslides, floods, earthquakes etc. This is partly acknowledged on P7326 L10 where it says “critical zone energy balance includes physical and chemical weathering, and the transfer of sediment associated with tectonic uplift and gravitational forces (Phillips, 2009; Volobuyev, 1964). However, these fluxes may be orders of magnitude less than EPPT and EBIO.” Evidence should be presented to justify the statement they are orders of magnitude less. Uplift and gravitational forces operate over longer time frames but when calculated on a W m^2 basis they can be much larger than EPPT and EBIO; see Odum’s calculations in the referenced Science paper and elsewhere. Fig 7 also indicates that EBio can be relatively large

Specific comments and questions:

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P 7333 and elsewhere: What is so magical about 292K (19C) and VPD of 1200 PA? As most plants grow well at VPD of between 800 to 1000 Pa, is 1200PA high? What areas of the planet have these conditions?

P7333 L5 suggests “Direct empirical measures of EEMT” are needed. I agree!

P 7336 L10 “The median absolute latitude for these locations was 42, indicating half of the locations were at high latitudes where solar radiation may be limiting.” OK but $\frac{1}{2}$ are at latitudes less than 42 and there is not enough information on the elevations of the other sites to make this statement convincing.

P 7338 L25 “Rasmussen et al. indicate” provide date of publication

P 7340 L 5: Conclusions of paper are optimistic (but worth developing.). Specifically the ability to scaling down monthly and regional temperature, precipitation, and derived VPD data to develop EEMT for individual hillslope catena’s to help understand “pedon scale heterogeneity” is optimistic at best; accurate estimations of EEMT will ultimately need to include wind speed (for VPD and ET) and local topographic controls on drainage. Also note that the relationships in Fig 4 and 5 suggest that EEMT may not be as sensitive as implied over the common range of biomes. Nevertheless, the theoretical framework and underlying principles are strong and should continue to be developed and local and regional EEMT metrics should be tested.

Fig 2B: caption “using a modified from Clausius-Clapeyron..” needs re-writing

Fig 3: These are so closely cross-correlated because NPP and Eppt are both derived from similar empirical temperature dependent relationships. This is acknowledged on P7333 L5 “Temperature is a primary parameter in the calculation of both EPPT and EBIO through temperature controls on PET, the specific heat of water in the calculation of EPPT, and the temperature dependent modified NPP equation of Leith (1975).”

Fig 4C Location of “break-point” is not very apparent nor is it explained how this was determined. Nevertheless, the 292 K point is a central part of the paper. Describe how

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this break point was determined.

Fig 6; The points cluster in a nearly vertical pattern at temperatures below 292K and EEMT < 5. Does this figure imply that EEMT is not sensitive below 292K?

Fig 7: This is not that useful. This also implies that over the wide range of temperatures (275-300K or 2 to 27C) Fbio increases at a constant rate. Is there empirical evidence for this? Caption for 7b indicates that exponent vary as a function of temperature. This is not apparent in the graph.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 7319, 2011.

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