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Interactive Comment

Interactive comment on "Thermodynamic limits of hydrologic cycling within the Earth system: concepts, estimates and implications" *by* A. Kleidon and M. Renner

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We thank the reviewer for his thoughtful and constructive comments. In the following, we respond to each of the reviewer's points. The individual points are taken from the review and listed in the following in *italic*, with our response following in plain text.

comment 1: *... the paper would be improved if these sensitivities were related more directly to the difference between the observed and estimated fluxes.*

In the revised version we will include a discussion on the difference to observations, along the lines of the comment that we posted in the discussion forum. We will include

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a different sensitivity in which we show that most of the discrepancy to observations is likely due to the effect of large-scale motion on surface exchange.

comment 2: *... the complexity of the argument coupled to the somewhat non-standard notation can make it difficult to follow the details of the argument and to judge the limiting assumptions used.*

We are aware of the non-standard notation, which we used because some letters are used for entirely different variables in thermodynamics and in micrometeorology. We hence used the letter J to refer to all radiative and heat fluxes. To avoid the overlap of P for power and precipitation, we used G as the generation rate of kinetic energy, so that at least E and P are used in their typical way for the major hydrologic fluxes.

In the revision, we will comment on this notation when it is introduced at the beginning of section 2, make a few adjustments to use more common terminology, and summarize the main assumptions that are being made (see also reply to comment 9 below).

comment 3: *The equivalence between the effective velocity used in the drag law in equation 16 and the exchange velocity used in Equation 19 is a major assumption. Please provide some support for why these two velocities should be the same in the turbulent atmospheric boundary layer.*

We were unclear in the manuscript regarding the use of velocities and eqn. 19 – we do not assume that the effective velocity is the same as the v in eqn. 19 even though, unfortunately, the same v was used. Eqn. 19 is used only to illustrate that through the intensity of friction (in form of the drag coefficient C_d), the value of velocity is being determined. In the following maximizations, the velocity is used directly as the variable that is being optimized so we make no explicit use of eqn. 19 in the remainder of the manuscript. In fact, by expressing surface drag as a momentum flux, $F_d = \rho C_d v^2 = (\rho v)(C_d v)$, the exchange velocity relates more closely to $(C_d v)$, although this would assume that all dissipation would occur at the surface. In the revised manuscript we will clarify that we do not make explicit use of this equation, that we determine velocities directly by maximization, and that we interpret that this maximization is accomplished by adjustments in the frictional drag. This interpretation is consistent with GCM sensitivity simulations in which we showed a related thermodynamic maximization (MEP, maximum entropy production) with respect to boundary layer friction (Kleidon et al., GRL, 2003, 2006).

comment 4: *It is not clear why the various effects listed in the second to last paragraph of page 3200 (lines 10-17, directly beneath Eq. 24) are all accounted for by J_{lh}. In particular, lines 15-16 argue that J_{lh} includes "the work done in lifting water from the surface to the level at which it precipitates". However, in the discussion of power that follows, G_{lh} and G_{lift} are treated separately.*

These various effects associated with hydrologic cycling are accounted for by the *entropy exchange* associated with J_{lh}. In the setup used here, entropy produced by these hydrologic processes are exchanged with the environment only by the latent heat flux. The entropy exchange of the system is thermodynamically constrained by the overall heating rate, J_{in}, and the difference in temperatures, T_h and T_c. This overall entropy exchange contains the entropy being produced by radiative transfer, the entropy produced by frictional dissipation associated with J_{sh} (eqn. 20), so that the entropy production by hydrologic cycling is all contained by the entropy exchange associated with J_{lh}. In the following, we are interested in those contributions to the entropy production that are associated with performing work, in terms of lifting and in terms of moist convection. We then consider the ideal case in which the entropy being produced by hydrologic processes results only from the dissipation associated with the two terms G_{lh} and G_{lift}. We will clarify this statement in the revision.

comment 5: *Please explain how the power in Figure 3 is calculated in the manuscript text.*

Figure 3 is an illustration of the trade-off that is being discussed in section 3.1, and as such is not calculated. We will clarify this in the revision by labeling the solid line and

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by referring to the Figure more explicitly in the main text.

comment 6: *In deriving equations 31-35, the authors "neglect the dependence of Ts on the heat fluxes in the denominator [of eq. 30]." However, Figure 4 shows that the driving temperature difference, and therefore presumably Ts can vary dramatically as the exchange velocity and convective heat flux change. Please provide a more detailed justification of this assumption. Is it just based on the relatively small sensitivity to Ts for Ts near 300 K, or is there a physical reason?*

Yes, this is exactly the reason why we neglect the dependence on T_s in the denominator. While the variation in T_s is < 30 K, and $T_s \approx 300$K, the variation of $1/T_s$ is rather small. We will add this clarification in the revision.

comment 7: *The observed average energy balance components listed in Table 3 are rather dated, since the Kiehl and Trenberth study was published more than 15 years ago. More recent estimates can be found among others in Stephens G.L et al. An update on Earth's energy balance in light of the latest global observations. Nature Geoscience 5, 691–696 (2012) doi:10.1038/ngeo1580. In particular, the listed observed net terrestrial radiation is too high, while the listed latent heat flux is too high.*

We will use the more recent values in the revision of the manuscript (see also separate comment, in which we already use the more recent study).

comment 8: *Given the large amount of averaging and assumptions used to derive the estimated fluxes in Table 3, I agree with the authors that they match the observed values closely enough to support the interpretation that the hydrologic cycle is operating at least 'near' its maximum limit. However, it is not possible to get any estimate or feeling for how 'near' the limit the cycle is really acting. In order to get a better sense of exactly how near to the limit, it would be useful to see how the observed values of Jsh and Jlh compare to Figures 4 and 5. That is, it should be possible to calculate an expected exchange velocity based on observations, and to compare its value and the associated power fluxes to those of the maximum. Although it may be hard to partition* *the observed values into fluxes associated with large-scale circulation and fluxes associated with vertical motion, this could be avoided by simply assuming that the observed sensible and latent heat fluxes are the sum of the horizontal and vertical terms for each. That is, it should be straightforward to create an additional figure that represents the summed fluxes and power of figures 4 and 5, and on which the maximum power value can be compared to that associated with the observed Jsh and Jlh.*

In the revision, we will include the observations in Figures 4 and 5. Regarding the combined effect of the large-scale circulation and vertical motion, we attempted this combination in a simple way which we post as a separate comment.

comment 9: *It is exceedingly difficult to keep track of the meaning of all the different fluxes through the many subscripts and combinations of subscripts used for J. For example, what does the c subscript in Eqn 28 refer to? I would suggest adding an extra column to Table 3 with definitions of all the different versions of J used, or to use additional variables so that the distinction between different fluxes does not have to be made through similar-looking and ill-defined subscripts.*

We will clarify this issue in the revision by adding a table with the description of the indices being used. We will also add the symbols to the results shown in Tables 2 and 3. The subscript c is indeed used twice, to refer to the cold reservoir (e.g. T_c), but also to the variables associated with vertical convection (e.g., G_c, $J_{sh,c}$ etc).

minor issues:

Page 3201, Line 8: Gamma should be defined as the moist adiabatic lapse rate to avoid confusion.

Yes, the lapse rate needs to be clarified. Because we assume that condensation takes place at a temperature T_a, it is more consistent with the setup to use the dry adiabatic lapse rate.

Why is $J_{s,net}$ negative in Equation 2 but positive in Equation 1? Is this a typo?

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There are two different terms, J_{net} and $J_{s,net}$, in equations 1 and 2. J_{net} is the net heat flux, $J_{s,net}$ the net entropy exchange, which is related to J_{net}. We will clarify this distinction in the main text.

It may be useful to clarify explicitly somewhere that all forces and fluxes are calculated per unit area

Indeed. We will clarify this in the revision at the beginning of section 2.

The variable P is used twice, for both power and precipitation.

As far as we can tell, we use the variable G for power (for generation rate) and P for precipitation to avoid this confusion (except for the label in Fig. 3, which we will correct).

The x-axis of both Figures 6 and 7 should be changed to reflect the specific variables that are plotted. The current labels are very confusing.

We will add the variable names in the revision for clarification.

What is plotted on the x-axis of the bottom row of Figure 7? Page 3212 suggests it is $J_{in,t} - J_{in,p}$, but that is not the radiative conductance mentioned in the label of the x-axis.

This was erroneously referred to as the sensitivity to $J_{in,t} - J_{in,p}$ in the text, but it reflects the sensitivity of the large-scale model to k_r. We will correct this in the revised version of the manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3187, 2013.