Hydrol. Earth Syst. Sci. Discuss., 10, C2363–C2368, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C2363/2013/

Hydrology and Earth System Sciences
Open Access
Discussions

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

A. Kleidon and M. Renner

akleidon@bgc-jena.mpg.de

Received and published: 7 June 2013

We thank Hubert Savenije for his supportive, constructive, and helpful comments. In the following, we respond to each of his 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: *I still have a problem with the loose treatment of the second law of thermodynamics. In Lines 6-15 on page 3193 it is implied that: $TdS = dQ$, where $dQ/dt = J_{net}$. Yet, as also stated in Kleidon et al. (2013), $Q = TS$ and hence: $dQ = TdS + SdT$. What has happened to this second term SdT, which we see nowhere*

in the derivations? Is it maybe replaced by the term D included in (1)? Does D represents SdT/dt?

That is a good and valid point, and we should have been more precise in the text. The second term is zero for the steady conditions that we consider in the most parts of the manuscript. Note that in textbooks, entropy is defined by $dS = dQ/T$ in the context of a very small and slow addition of heat, dQ, to a very large heat reservoir, so that T does not change. This approximation we do not need to make because of the steady state assumption, for which, by definition, the temperatures of the system do not change, although entropy is still being exchanged. This can be shown as follows. The system consists of two heat reservoirs, one being heated and the other being cooled. In steady state, both temperatures are constant, i.e. $dT_h = 0$ and $dT_c = 0$. The steady state implies that the heat added to the hot reservoir of the system, dQ_{in}, balances the heat transfer within the system from the hot to the cold reservoir, $dQ_{h,c} = dQ_{in}$, and the heat transferred to the cold reservoir within the system balances the removal of heat from the system, $dQ_{out} = dQ_{h,c} = dQ_{in}$.

To evaluate the entropy exchange in these heat transfer processes, we use $dT_h = 0$ and $dT_c = 0$ because of the steady state assumption. Then, $dQ = d(TS) = TdS + SdT$ simplifies to $dQ = TdS$. The heat transfer process between the two reservoirs removes heat from the hot reservoir, $dQ_{h,c} = T_h dS_h$, and adds this heat to the cold reservoir, $dQ_{h,c} = T_c dS_c$. The increase of entropy, $dS_{h,c}$, caused by this transfer of heat from hot to cold within the system is hence $dS_{h,c} = dS_c - dS_h = dQ_{h,c}(1/T_c - 1/T_h) = dQ_{in}(1/T_c - 1/T_h) > 0$.

When we consider the entropy that is exchanged by this system with the surroundings, then the addition of heat to the system adds entropy to the system. From $dQ_{in} = T_h dS_{in}$ we obtain $dS_{in} = dQ_{in}/T_h$. The removal of heat removes entropy from the system, which we obtain from $dQ_{out} = T_c dS_{out}$ so that $dS_{out} = dQ_{out}/T_c$. Overall, the net entropy exported by the system due to the heat exchange with the surroundings is $dS_{ex} = dS_{out} - dS_{in} = dQ_{in}(1/T_c - 1/T_h) > 0$ (since $dQ_{out} = dQ_{in}$, and $T_h > T_c$).

Furthermore, we note that $dS_{ex} = dS_{h,c}$, that is, the entropy produced within the system balances the net entropy exported by the system to the surroundings.

Overall, this analysis does not neglect changes in dT in the derivation, but this rather follows from the steady state assumption. In the revision, we will rewrite the paragraph to make this steady state assumption and the derivation more explicit as described here.

comment 2: *The two balances (vertical and horizontal) are global, not distinguishing between continents and sea. The vertical balance is for the entire Earth. I am wondering what spatial heterogeneity does, since the equations are not linear. Would the optimum exchange velocity in Fig. 4 (now about 2 mm/s) be different as a result of spatial heterogeneity or longitudinal variation? And similarly for Figure 5, would the difference between ocean and land lead to a different optimum (also at about 2 mm/s)? The authors deal with this issue in section 4, but I am not sure if this is the same thing.*

We will change section 4 (also in response to reviewer #1) and deal with this coupling between vertical convection and large-scale motion more explicitly. We post this new analysis as a separate comment in the discussion, and include this in the revision. This comment already captures some aspects of spatial heterogeneity, although it would probably require a more complete analysis to fully address the role of spatial heterogeneity.

We did not (yet) find a simple and consistent way to couple the two gradients, so what we do instead is a simple combination of the exchange velocities to estimate the effects on surface exchange. Even though we cannot show this with the model, we think that the coupled system would likely have similar maximum states. This is because (1) the optimizations have quite different time scales, with vertical convection taking place on time scales of minutes while large-scale motion typically operates on a time scale of a week, and (2) because the vertical difference in radiative forcing is quite a bit larger than the horizontal difference. We also attempted to include the effect of horizontal heat

transport on vertical convection. What we found is that the maximization is essentially unaffected and that horizontal heat transport affects mostly the absolute temperatures of the surface and the atmosphere, but not the vertical temperature difference.

We feel that a better coupling between vertical and horizontal motion is an aspect that should certainly be explored further. However, for the current manuscript, we think that (a) we cannot address this aspect in a consistent way with the current models (we wish we knew how to do this properly) and (b) that the preliminary attempt described in the note addresses it sufficiently for the scope of this manuscript.

comment 3: *It is not clear to me how the horizontal and the vertical balances are coupled. The authors consider them independently. However, I presume that the sum of (9) and (15) would be the total entropy production. Should that not be equal to σ in (2)? Now the two systems are optimized independently, but would a combination of the two systems lead to another optimum?*

Yes, in principle the total power should be maximized, rather than the separate contributions. What this essentially means is that more power can be derived from the temperature difference between the tropical surface and the extratropical atmosphere than if motion would only be generated locally. Otherwise, we mostly respond to the effect of this coupling in our reply to comment 2 above.

comment 4: *It is nice to see that indeed the general evaporation equation is obtained, as in (35). This equation is very close to the Penman equation (commonly used in hydrology), where we use the sum of the net radiation reaching the surface and the turbulent wind energy. If we would want to equate the two equations then we obtain the following expression: $J_{net} = R_N + c_p \rho_a / s r_a (e_s - e_a) = R_N + J_{turb}$ whereby: $R_N = (1-r)R_C - R_B$ where r is the albedo, R_C is the net incoming short wave radiation at the surface and R_B is the net outgoing long wave radiation from the surface. R_C is calculated by an empirical formula. For an average climate it is given by: $R_C = (0.25 + 0.5n/N) J_{in,s}$ where n/N is the proportion of sunshine on an average day. If* *this $n/N = 0.5$, then $R_C = 0.5J_{in,s}$. Hence $J_{net} = 0.5J_{in,s} + (J_{turb} - 0.5rJ_{in,s} - R_B)$. If the term between brackets cancels out (meaning that the energy required for wind-driven evaporation is equal to half the reflected incoming radiation and the outgoing long wave radiation), then we have the same result. Since the turbulence is generated by the energy reflected from the surface, this looks plausible.*

The main difference to Penman is that in our approach we do not treat the second term in the Penman equation (J_{turb}) as an independent forcing, but consider motion of being a dependent process that is generated by horizontal heating differences. In the vertical case, motion is only generated by surface heating, while horizontal differences in radiative heating generate additional motion. We attempted to evaluate this effect of additional motion in the separate comment that we posted, in which we added the exchange velocities. What this analysis suggests is that more turbulent exchange is allowed for by the extra contribution of larger-scale horizontal motion, which is consistent with the second term in the Penman equation (also in terms of the magnitude of the Priestley-Taylor coefficient, see comment). Even though we used a different line of reasoning, we think that this addresses your comment regarding the extra contribution by J_{turb}.

Regarding your reasoning, note that we consider $J_{in,s}$ already as the absorbed solar radiation, i.e. it already includes the effect of the albedo, and the term n/N in the empirical formula also captures cloudiness, which is already included in the value of $J_{in,s}$ that we use (note that we consider the *absorbed* radiation as the influx of heat, rather than the incident solar radiation of which a fraction is reflected). So we are not sure how comparable these derivations are to our results. It would certainly suggest to compare our estimates at a more detailed level, including geographic variations, to observations. We plan to do this in the near future, but think this is beyond the scope of this current manuscript.

comment 5: *Regarding the derivations in 2.3, I have a few issues: 1. I think that (16) is not the "momentum" balance, but the balance of forces per unit area. The unit is*

[N/m2].

Equation (16) is indeed a momentum balance (per unit area), noting that a change of momentum in time is determined by the sum of the forces acting on the flow. We will clarify this in the revision.

As also noted by reviewer #1, we will state in the revision that all properties are per unit area.

comment 6: *Next, to derive (17), it is said that the forces are multiplied by the velocity to obtain the equation for kinetic "energy". First, I think it should be kinetic "power per unit area" rather then energy. Second, I think that the power is obtained by integration, and not by multiplication. Hence I suspect that the last term of (17) should be divided by 3, and that (18) should contain 3 between the brackets. Of course this does not change the argument, but it may have an impact on the magnitudes (if I am right).*

Eqn. (17) constitutes a balance equation for kinetic energy per unit area. The left hand side that is zero is the change of kinetic energy in time. The right hand side includes the power (or generation rate of kinetic energy) and the dissipation by friction.

Power is work performed through time, and is derived not by integration, but by differentiation, i.e. $P = dW/dt$. Since $W = \int F dx = \int F(dx/dt)dt = \int F v dt$ (with $v = dx/dt$), we obtain $P = dW/dt = Fv$. However, since eqn. (18) is not used further in the text, these expressions do not affect the results in the remainder of the manuscript.

In the revision, we will clarify these aspects.

minor issues: We will fix these issues in the revision.

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