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Interactive Comment

Interactive comment on "Estimates of the climatological land surface energy and water balance derived from maximum convective power" *by* A. Kleidon et al.

A. Kleidon et al.

akleidon@bgc-jena.mpg.de

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We thank Antoon Meesters and Han Dolman for their thorough review of our manuscript. The main points they raise regarding the theoretical basis of the max. power limit likely results from a misunderstanding of how we treat longwave radiation in our approach. In the following, we address this misunderstanding and explain that our approach is valid and well founded in thermodynamics. We also address the important point regarding the convergence of atmospheric moisture transport, how this can be included into our approach, and show that it does not affect our estimates.

Issue 1: theoretical foundation. *The authors, in their choice for J_{in}, effectively neglect the longwave radiation exchange between surface and atmosphere, although this is just as well a form of heat input, and the radiation absorption in the air that occurs mostly at lower levels.*

Response: It is not true that we neglect the exchange of longwave radiation because we do include it explicitly in our approach. Our model explanation in section 2 may be not clear enough as we refer to "net exchange of terrestrial radiation" without further explaining it and Fig. 2 may be misleading in this respect as the arrow of terrestrial radiation only points in one direction. Our treatment of net exchange of terrestrial radiation does, however, include both directions, thus also the downward component of terrestrial radiation. It is, however, critical to acknowledge that the downward terrestrial radiative flux to the surface is *not* an independent forcing of the surface as it strongly depends on the emission of terrestrial radiation from the surface in the first place. We will show below that the net flux of terrestrial radiation at the surface (our variable R_l) has to cool the surface in order to (a) comply with the second law of thermodynamics, and (b) result in a state in which power can be generated to drive convective exchange.

To start, we first want to clarify our parameterization of terrestrial radiative exchange. Instead of distinguishing between the emission of terrestrial radiation from the surface and the absorption of downward terrestrial radiation from the atmosphere, we combine these two fluxes to a net terrestrial exchange and express it as $R_l = k_r(T_s - T_a)$. This simple expression was derived in Kleidon and Renner (2013a) from the linearization of the Stefan-Boltzmann law (Appendix A2 in Kleidon and Renner, 2013a). We linearized the Stefan-Boltzmann law using the same reference temperature, so that both radiative fluxes, the emission of terrestrial radiation by the surface, $R_{l,u}$, and by the atmosphere, $R_{l,d}$, have the form

$$R_{l,u} \approx R_{l,0} + k_r(T_s - T_0) \tag{1}$$

and

$$R_{l,d} \approx R_{l,0} + k_r(T_a - T_0) \tag{2}$$

where $R_{l,0}$ is the emission of terrestrial radiation at a reference temperature, T_0. The net exchange of terrestrial radiation at the surface then has the form:

$$R_l = R_{l,u} - R_{l,d} = k_r(T_s - T_a) \quad (3)$$

which is eqn. (2) of our paper. This expression considers both, the upward as well as the downward flux of terrestrial radiation at the surface.

To illustrate that the downward terrestrial radiative flux does not act as an independent forcing, let us consider the following, extreme case. Let us imagine that a heat engine would be driven by all absorbed radiation, that is, the absorption of solar radiation, R_s, and the absorption of the downward terrestrial radiative flux, $R_{l,d}$. The heat flux that would enter the heat engine would then be $J_{in} = R_s + R_{l,d}$. Since in our simple model, the atmosphere emits equally to space and to the surface, and because of the global energy balance, we have $R_{l,d} = R_s$, so that $J_{in} = 2R_s$, which is four times the value we find for the maximum power state (cf. eqn. 8). Yet, when all of the absorbed heat goes into the heat engine, our surface energy balance would reduce to $R_s - J_{in} = 0$, and $R_{l,u} = 0$. This case can only be met if the surface temperature is $T_s = 0K$, while the atmospheric radiative temperature would be unaffected and have a value of $T_a = 255K$. The heat engine would then need to transport heat from a very cold surface to a much warmer atmosphere. Such a state cannot be maintained by a heat engine as it violates the second law of thermodynamics.

To fulfill the second law, the surface temperature needs to be at least as warm as the temperature of the atmosphere, $T_s \geq T_a$. If it is equal to the atmospheric temperature ($T_s = T_a$), then $R_{l,u} = R_{l,d}$, so that $R_l = 0$. Then, the heat flux driving the engine would be $J_{in} = R_s$. This is the example described in the review of Meesters and Dolman, and would yield a heat flux that is twice the value of the maximum power limit. However, this large heat flux would not result in any power of the engine, because the temperature difference is zero.

It is only in those cases where net longwave radiation cools the surface, $R_{l,u} > R_{l,d}$ or

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$R_l > 0$, and the surface is warmer than the atmosphere ($T_s > T_a$) in which the heat engine can produce any power to drive a convective heat flux.

The other extreme case permitted by the second law is $R_s = R_l$. This case would result in the greatest temperature difference, $T_s - T_a$. But because there is no convective heat flux ($J_{in} = 0$), the power would, again, be zero. What we show in our paper (Kleidon and Renner, 2013) is that the maximum is attained when the absorbed solar radiation is partitioned equally into net cooling by terrestrial radiation and convective fluxes (i.e., $R_l = R_s/2$ and $J_{in} = R_s/2$). It is because of the tradeoff between the convective heat flux and the temperature difference that results in the maximum power state (as shown in Fig. 3 in Kleidon and Renner (2013)).

In summary, our considerations do include the potential of terrestrial radiation as an energy source for the convective heat engine. However, as we showed above, the surface needs to be warmer than the atmosphere for a heat engine to be able to generate convective heat fluxes and, therefore, net terrestrial radiation needs to cool the surface. Since R_l is not an independent variable, it cannot simply be added to the turbulent heat fluxes as in the example of the reviewers, but rather needs to be seen as a term of the surface energy balance that shapes the temperature difference and that constrains the magnitude of convective heat fluxes.

In the revision, we will add some of these explanations to the manuscript to clarify the role of terrestrial radiation in our approach. Also, we will adjust Fig. 2 and indicate that our parameterization of terrestrial radiation covers both directions of the exchange of terrestrial radiation.

Issue 2: Scaling issues/effects of divergence of moisture transport. *One point is that scaling issues arise if one applies a principle which holds for the whole atmosphere, to the atmosphere over continents or regions. For instance, the ingoing energy Jin is assumed to be the $H + \lambda E$ at the surface, but for applications to find local hydrological values, one has to cope with lateral flows of heat and vapor which are not* *necessarily negligible compared to the surface flow. If they were negligible, one would have to infer that $E = P$ etc. which in reality only holds for averages over a very large scale, i.e. the entire globe. As this point gets attention in the discussion section of the paper (and in the discussion about Kleidon and Renner 2013a), we won't comment further on it here.*

Response: This is a very good point and we fully agree that we neglected convergences or divergences of atmospheric moisture transport in our approach and that this transport would affect the energy balances that we consider.

The effect of atmospheric moisture transport can easily be incorporated into our approach and we can show that it does not affect our estimates of the energy partitioning at the surface. To do so, we rewrite the atmospheric energy balance as

$$R_l + H + \lambda P - \sigma T_a^4 = 0 \tag{4}$$

The difference to Kleidon and Renner (2013a) is that we use P to express the release of latent heat in the atmosphere, which does not need to balance E at the regional scale due to atmospheric moisture transport.

To evaluate the effect of moisture transport, we slightly rewrite this equation as

$$R_l + H + \lambda E + \lambda(P - E) - \sigma T_a^4 = 0 \tag{5}$$

From the surface energy balance we know that $R_s = R_l + H + \lambda E$. We thus obtain for the atmospheric temperature

$$\sigma T_a^4 = R_s + \lambda(P - E) \tag{6}$$

Hence, we notice the effect of the convergence of atmospheric moisture transport, $P - E$, directly as an additional term in the atmospheric energy balance and that it affects the estimate for the atmospheric temperature, T_a. This, in turn, affects the surface temperature, T_s, in our approach because $T_s = T_a + (R_s - H - \lambda E)/k_r$ (eqn.

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A5 in the manuscript). Yet, the expressions for the energy partitioning at the surface between radiative and convective cooling is not affected by the $P-E$ – after all, the surface is still heated by R_s and so it is this energy that is partitioned among R_l and the convective heat fluxes. The effect of $P-E$ on surface temperature is already accounted for in our estimates because we use observed surface temperatures. Overall, even though there is an effect of $P-E$ on the atmospheric energy balance, T_a and T_s, our estimates of surface energy balance partitioning remain unaffected because this effect does not alter the equal partitioning into radiative and convective cooling while the effects of surface temperature are already accounted for by the use of observations.

In the revised version of the manuscript, we will point out this effect of the moisture convergence on our approach, include the above derivation in the Appendix and adjust the description of the model in section 2.

Issue 3: Bowen ratio. *It is assumed that without water limitation, the Bowen ratio should approach γ/s, with γ the psychrometric constant and s the slope of the saturation vapor pressure curve. This is based on the assumption of saturation of the air at reference level (see Kleidon and Renner), which may be very crude in practice.*

Response: Actually, eqn. (4) in the manuscript is not an assumption, but follows from the partitioning of heat fluxes at maximum power (eqns. 9). We fully agree that the equation is currently misplaced and appears like it is an assumption.

In the revision, we will place the equation at the more adequate location, which is right after eqn. 9.

Issue 4: Atmospheric window. *In reality things are more complicated: part of the radiation from the surface escapes through the atmospheric window, and another part should not count as entering the atmosphere at the surface because its absorption occurs at much higher levels. Accounting for this would lead to a maximum, but with Ts and Rl much higher and $H + \lambda E$ consequently lower than calculated in the discussion paper. But even more important is that the application of the maximization principle* *may become unpractical. The authors do apologize for neglecting the part of the long-wave radiation emitted by the surface which is not absorbed by the atmosphere (page 282, lines 11-12). Actually, in the balance for the atmosphere, it is the part which is absorbed which they neglect; their calculation would be valid if all the radiation would pass through the window.*

Response: As we explained above, we do include the exchange of terrestrial radiation in our approach and, in fact, assume that *all* terrestrial radiation emitted by the surface is absorbed by the atmosphere. This is not mentioned explicitly in the current methods section of the paper (although it is described in Kleidon and Renner (2013)). Our assumption is evident when we write down the energy balance of the atmosphere, which is given by (see also above regarding issue 2)

$$R_l + H + \lambda E - \sigma T_a^4 = 0 \tag{7}$$

The effect of an atmosphere which is partly transparent can easily be evaluated when we include another parameter, f, in the model which describes this partial transparency (similar to the ϵ in Kleidon, 2004, Climatic Change) and which would be related to the optimal depth of the atmosphere for terrestrial radiation. With this parameter, the atmospheric energy balance would change to

$$f R_l - f \sigma T_a^4 + H + \lambda E = 0 \tag{8}$$

while the surface energy balance would be (using above linearizations of terrestrial radiation)

$$R_s' - k_r'(T_s - T_a) - H - \lambda E = 0 \tag{9}$$

where $R_s' = R_s - (1-f)(R_{l,0} + k_r(T_s - T_0))$ and $k_r' = f k_r$. This formulation results in the original model when $f = 1$, and represents a partially transparent atmosphere for cases in which $f < 1$.

The formulation of the surface energy balance is practically identical to the case of a fully absorbent atmosphere except for using the somewhat different parameters R_s' and k_r'. Since $R_s' < R_s$ because a fraction of emitted terrestrial radiation from the surface is subtracted from R_s for $f < 1$, the sum of the turbulent heat fluxes at maximum power is $R_s'/2 < R_s/2$. This is going to be expected as some of the emitted radiation from the surface passes the atmosphere without absorption, so that less of the absorbed solar radiation is exchanged between the surface and the atmosphere (just as the reviewers state in their review). Hence, our formulation can incorporate the effects of a partially transparent atmosphere and yields consistent results. Because we currently do not account for a partially transparent atmosphere for matters of simplicity, our estimates can be too high in such areas (as already pointed out in the discussion section of the manuscript).

In the revision, we will expand the discussion section in the revision to clarify this point further.

Issue 5: Derivation of the Carnot/max. power limit in Appendix A. *This is a critique on the derivation in Appendix A. The result of the derivation is not disputed. For an engine which performs and dissipates its work internally (as the atmosphere) the maximum kinetic energy production is $G_{max} = J_{in}(T_s - T_a)/T_s$ with J_{in} the ingoing energy at the surface, and T_s and T_a the temperatures at which energy goes in and out. We note for completeness that for this kind of engine, there is some ambiguity about the denominator, which depends on the temperature where the dissipation takes place; if most dissipation would occur close to the surface, then the denominator would have to be T_a (which would be advantagous for maximality computations). See also section 2.1 of Kleidon and Renner 2013a (before Eq. 5)). A source of confusion in Appendix A and elsewhere is that the equation (with unambiguous denominator T_s) also holds for an engine which, unlike the atmosphere, performs its work externally: this is the much more classical case that was considered by Carnot and subsequently in all textbooks. The derivation in Appendix A of the discussion paper is starting from* *assumptions pertaining to this classical type of heat engine, as can be seen by comparison with the derivation in section 2.1 of Kleidon and Renner 2013a, which was correct until the statement $J_{in} = J_{out} + G$ was invoked. That statement contradicts the earlier statement $J_{in} = J_{out}$, which should hold since there is no long-term increase of internal energy, nor work done on the surroundings. The term G has to be left out because the work is done by the atmosphere onto itself, unlike with a classical Carnot engine. Now proceed with "In the case of the atmosphere . . ." and use Eq. 3 immediately, instead of Eq. 4, to derive Eq. 5. Their derivation however still works: the first error is compensated by a second error: the assumption that entropy exchange is zero (which also holds only for the classical heat engine, and which re-occurs in Appendix A of the discussion paper). If correct, this would mean that there is no entropy production by dissipation of kinetic energy at all, contradicting the (correct) Equation 5.*

Response: In the Appendix, we only deal with the maximum power limit, but do not consider the full treatment of the entropy budget as in Kleidon and Renner (2013). We agree with the reviewers that by adding some explanations, the description of the thermodynamic limit can be made more complete.

The equation $J_{in} = J_{out} + G$ for the description of the energy conversion from heat to kinetic energy is correct because the term G represents the generation of kinetic energy, while J_{in} and J_{out} are heat fluxes that enter and leave the heat engine. When kinetic energy is generated, it must come at the expense of heat to conserve energy, and this is what this equation expresses. It does not reflect the full balance of heat fluxes in the atmosphere, as it does not account for the dissipation D.

As the reviewer rightly state, this formulation seems to imply that there is an accumulation of energy, but this is resolved when the dissipative heating of kinetic energy by friction, D, is being accounted for. There are two extreme possibilities for this accounting: D can be added to J_{out}, resulting in entropy production of D/T_a, or it can be added to J_{in}, resulting in entropy production of D/T_s. In the latter case, the T_s in the denominator of the Carnot limit would be replaced by T_a, resulting in slightly more power (and which then does not require any approximation regarding the denominator for the maximization). It may be noted that this case is equivalent to the concept of the dissipative heat engine (Renno and Ingersoll, 1996, J. Atm. Sci.; Bister and Emanuel, 1998, Meteorol. Atmos. Phys.). However, both cases result in approximately the same partitioning of energy fluxes at the surface at maximum power in our model, as the reviewers write as well.

In the revision, we will reformulate the summary on the derivation in the Appendix taking these aspects into account, and being closer to Kleidon and Renner (2013a), as suggested by the reviewers.

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