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Discussions

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

A. J. Dolman (Referee)

han.dolman@vu.nl

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General remarks

We like the approach of calculating physical entities in the simplest possible way, considering only the very essentials. Moreover, the results show an intriguing accordance between some of the modelling results and observation-based estimates. This is of great interest, regardless of the theoretical foundations. Our main problem with the paper however concerns these foundations. The paper, similar to earlier ones, is based on the arguably simple assumption about the value of the "Carnot limit" Jin(Ts-Ta)/Ts

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for the atmosphere, in which Jin is the ingoing energy at the surface, and Ts and Ta are the temperature at the surface and at the average level from which the upward radiation leaves to space. The Carnot limit in the present case (engine without outer work) is the maximum possible rate at which kinetic energy can be dissipated; this is a well-founded thermodynamic principle. The additional assumption made in the paper is that this function itself should be close to the maximum attainable when Jin and Ts are varied (Ta is kept fixed). The variation is done under the constraint of a relation between Jin and Ts , corresponding to surface energy balance. The basic idea behind this approach, maximizing the Carnot limit, has been shown to work for certain (simpler) systems. On the other hand, no hard evidence exists that the atmosphere is working at this maximum. The authors also acknowledge this. We argue that there is a problem in the way in which the hypothesis is applied in the paper. The issue is in the definition of Jin. The authors, in their choice for Jin, 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. This neglect is confusing, as the authors are well aware of radiation exchange in general. Moreover, in Kleidon and Renner (2013a) this term was initially included in Jin, but later on disappeared in that paper without proper explanation. We will show below that the maximization method becomes problematic with this inclusion. With the approximations made in the paper, the maximum in fact would not exist anymore. A minor point without consequences is the confusion (notably in Appendix A) about the reasons why the Carnot limit (not its maximality) should apply to the atmosphere; Kleidon and Renner (2013a) treated this in an arguably better way. This point will also be commented on below. There are a few minor issues with the present applications, which are more of a typically hydrological nature than theoretical. 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 + λE 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. Another issue is that 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.

On the identification of the ingoing energy Jin The most important issue we have with the current paper is: what should be used for Jin ? According to the original concept, it is just the energy flow entering the atmosphere on the hot surface side. In section 2.2.1 of Kleidon and Renner 2013a, Jin is interpreted as the sum of the sensible and latent heat flux at the surface (H + λE), plus the "net radiative exchange between the surface and the atmosphere" (Js,a in the original paper; it is apparently equal to the Rl ,"net exchange of terrestrial radiation" , of the present paper): Jin = H + λE + Rl We agree that this is the obvious choice for a simple model (neglecting the atmospheric absorption of solar radiation for convenience). It is somewhat confusing that subsequently in Kleidon and Renner (2013a) the meaning of Jin changes from section to section (in Eq. 21 it is lateral sensible heat flux, in Eq. 25 it is latent heat flux at the surface, etc.) In the present paper, however Jin is interpreted/defined as Jin = H + λE at the surface, so without the Rl. The surface energy balance is then used to translate the Carnot limit (H + λE) (Ts – Ta)/Ts to a function of Ts : As Rs = H + λE + Rl (neglecting terrestrial radiation passing through the atmospheric window), the function becomes (Rs – Rl) (Ts – Ta)/Ts with Rs (short wave radiation absorbed by the surface) constant, and Rl = kr(Ts – Ta) by assumption. This function has a maximum because of a trade-off: when Ts rises, Ts-Ta rises but Rs- Rl falls (the Ts in the denominator is left constant for convenience). But, as remarked above, the net absorbed terrestrial radiation Rl should be added to Jin. This Rl, like H and heating by condensation, also contributes to the heating of the lower atmosphere, and hence to the expansion of the air which drives atmospheric convection and large-scale flow. But with Rl added to Jin, the limit would become (H + λE + Rl) (Ts – Ta)/Ts which would, again with the surface energy balance, translate to Rs (Ts – Ta)/Ts Now, Jin = Rs and hence a constant ! The trade-off no longer holds in this viewpoint, and the function does not have a maximum (unless one interprets an infinite Ts as such). 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 + λE 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. On the derivation of the Carnot limit 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 Gmax = Jin (Ts-Ta)/Ts with Jin the ingoing energy at the surface, and Ts and Ta 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 Ta (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 Ts) 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 com-

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parison with the derivation in section 2.1 of Kleidon and Renner 2013a, which was correct until the statement "Jin = Jout + G" was invoked. That statement contradicts the earlier statement Jin = Jout , 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.

Antoon Meesters and Han Dolman

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 265, 2014.