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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.

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We thank the reviewer for his constructive comments on the manuscript. His comments concern mostly clarifications of three issues that we address below. The third issue was also raised by Meesters and Dolman in the other review, to which we replied more extensively and so we only post a brief summary here.

Issue 1: *To make the system work, the Bowen ratio in the form of Eq. 4 is introduced as a deus ex machina. However, looking at the the form of the equation it turns out that it is consistent with the Priestley-Taylor equation for evpotranspiration. So, the*

question is: how physical is Equation (4), necessary for the further derivations, or is it an assumed parameterization that is not mentioned in the paper. If this is the case, the results are not as much derived from first principles as stated. Please explain where the assumed form of the Bowen ratio comes from?

Response: Indeed, the Bowen ratio (eqn. 4) seems to reflect an assumption that we have made, but it is actually derived from the result of the maximization and was misplaced in the manuscript. One can derive equation 4 directly from the optimal partitioning given by eqn. 9:

$$B_{opt} = \frac{H_{opt}}{\lambda E_{opt}} = \frac{\gamma}{f_w s} \quad (1)$$

In the revision, we will place the expression for the Bowen ratio at a more adequate place immediately after eqn. 9 and mention that it is derived from the optimum expressions of H_{opt} and λE_{opt}.

Issue 2: *There are quite a number of simple reference evaporation equations that are driven by radiation (incoming radiation that is), and temperature only (e.g. Makkink, Hargreaves: see e.g. http://folk.uio.no/chongyux/papers_SCI/HYP_4.pdf). It would be good to take one of those simple equations and use it as a benchmark and drive it with the same surface temperature and precipitation and a radiation dataset for incoming radiation (see e.g. http:/wui.cmsaf.eu/ safira/ action/ viewDoiDetails? acronym=RAD_MVIRI_V001). The authors could then compare their method to the following primitive model: if Eref < P->E=Eref and Q=P-Eref; else if Eref > P -> E = P and Q = 0. There method should at least do as good or better than this primitive model that follows the outer lines of Budyko.*

Response: That's a very good suggestion. Since some of the approaches, in particular the one by Makkink, is very close to an equilibrium evaporation formulation and expressed in terms of R_s, we will comment on these empirical methods in the discussion section. The Makkink method, for instance, uses $E = 0.61s/(s+\gamma)R_s/(58.5J/(l/d))$, which is almost identical with our expression except for being multiplied by a factor of 0.61. Hence, this method would yield consistently lower estimates than ours, but since ours are already on the low side, the method of Makkink would underestimate it even further. Hence, we think that there is relatively little extra value if we repeated the analysis with this method. Instead, we think that adding a discussion on the relationship to empirical methods is better suited for the paper.

In the revision, we will add such a discussion on some of the empirical methods in the discussion section of the manuscript.

Issue 3: *A downside of open review like this is that I just noticed the review of Han Dolman which draw my attention to the same question about deriving the maximum power limit in appendix A: when looking at the energy input (Jin) to the atmosphere, only the turbulent heat exchange is considered and not the net longwave radiation exchange (L_up-L_down) which also depends on (Ts-Ta). Is this term small compared to LE+H? Please elaborate on this.*

Response: We use the heat fluxes as input to the heat engine because they are *heat* fluxes, as opposed to *radiative* fluxes. We do consider longwave radiative exchange in the context of the surface energy balance (eqn. 1), which constrains the magnitude of the turbulent heat fluxes. In the energy balance, we combine the longwave radiative exchange in the term R_l, with $R_l = R_{l,u} - R_{l,d}$ (see also our response to the review by Meesters and Dolman). Because the surface temperature is in general warmer than the atmospheric (radiative) temperature, as it must be, R_l is generally negative. There may be some exceptions to this, e.g. in high latitudes or during nighttime, but these exceptions are either temporary or maintained by lateral heat transport. What this means is that $R_{l,d}$ is not an independent forcing of the surface, but depends on the absorption of emitted surface radiation and on the heating by $H + \lambda E$. We may also note that in principle, the surface energy balance (eqn. 1) would allow for all of the absorbed solar radiation to be converted into turbulent fluxes (i.e., $R_s = H + \lambda E$), which would then result in $R_l = 0$. Yet, such a condition would require that the temperature

difference between the surface and the atmosphere to be zero, which would not allow for any generation of power by the heat engine.

In the revision, we will clarify the treatment of longwave radiation in our approach, and adjust Fig. 2, which may be misleading in this respect because it only includes an arrow from the surface to the atmosphere while we consider both directions.

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