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Earth System Dynamics
Discussions

Interactive comment on "Diurnal land surface energy balance partitioning estimated from the thermodynamic limit of a cold heat engine" *by* Axel Kleidon and Maik Renner

Axel Kleidon and Maik Renner

axel.kleidon@bgc-jena.mpg.de

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We thank the reviewer for the thoughtful and constructive review of our manuscript. In the following, we summarise the referee's comments in *italic*, provide our reply to each point, and suggest how we address these points in the revision. Note that the numbering is somewhat different to the referee's numbering to separate the referee's comments into separate points.

Reviewer comment 1: *The authors refer to the atmospheric engine with heat storage as a "cold" heat engine because of its lower efficiency compared to a usual heat engine without such storage (page 5, line 25). I have a different opinion on this point because*

the actual power of this engine is not very low for the following reason. It is true that an increase in heat storage in the engine ($dU_a/dt > 0$) results in lower power generation because the heat stored in the engine ($T_e \approx T_s$) is not transferred to the atmosphere with lower temperature (T_a), and thus cannot produce work according to the second law. This heat storage increase therefore results in a reduction in the power generation rate in Eq. (4). However, for the same reason of this heat storage, the reduction in the surface temperature against Jin is reduced when there is such heat storage increase ($dU_a/dt > 0$). Thus, the optimum-state surface heat flux (J_{opt}) becomes larger when $dU_a/dt > 0$, as is shown in Eq. (7). When we combine these two effects (negative and positive effects in the heat flux) together, and substitute J_{opt} (7) into (4), we get the optimum-state power generation rate, with the total energy balance condition (6), as

$$G_{opt} = \frac{R_{s,avg}}{2} \cdot \frac{(T_s - T_a)}{T_a}. \tag{1}$$

This means that the large variation in R_s is cancelled out by the buffering effect of the heat storage, and G_{opt} is nearly constant over time, which is indeed identical to the steady-state generation rate found by Kleidon and Renner (2013). So the heat storage seems to act to regulate the rapid change in R_s (the driving force) to realize a nearly constant rate of power generation by storing heat in daytime and discharging heat in nighttime. I would therefore suspect a "buffering" effect of the heat engine with the heat storage rather than a "cold" heat engine.

Reply: Thank you, these are very good and thoughtful points.

In terms of referring to our approach as a "cold" heat engine, this concerns the first term in the expression of power (Eq. 4), not the efficiency. With "cold", we do not refer to the comparison between heat storage changes taking place below or above the surface, but rather to the case where one uses the turbulent heat fluxes J_{in} directly in the Carnot limit (i.e., $G = J_{in} \cdot (T_s - T_a)/T_a$), as one may naively do (and what we initially did, and which results in optimum heat fluxes that are notably too small, see light blue dots in Fig. 3).

Our motivation to refer to our Eq. 4 as the Carnot limit of a "cold" heat engine is similar to a cold car engine in winter. When the car engine is still cold in winter just after it has been started, one needs to hit the gas harder to get the same power. Our expression captures this effect: The heat flux needs to be larger to get a certain power, because the term dU_a/dt reduces the effect of the heat flux on the power. As this is a storage effect similar to a cold car engine heating up, we think that the term "cold heat engine" nicely captures this storage effect.

This aspect of heat storage change alters the Carnot limit, as derived in sect. 2.1, and as such is independent of the feedbacks with surface temperature that come into play when this limit is evaluated in the surface-atmosphere system. So while the explanations that the maximum power limit actually results in the same limit of power, as nicely shown by the reviewer, we feel that the term "cold" heat engine is nevertheless appropriate, as we do not refer to the outcome in terms of maximising power, but rather to how the Carnot limit plays out in the context of heat storage changes.

In the revision, we will motivate and clarify the justification for the "cold" heat engine terminology in greater detail in the introduction and section 2.1, and will include some of the outcomes as pointed out by the reviewer in the discussion section.

Reviewer comment 2: *It may also be interesting to see if G_{opt} of the atmospheric system really shows such regulation by using the observed data (R_s, dU_s/dt and T_s).*

Reply: Indeed, this is an excellent point. In principle, one should be able to diagnose this from the eddy flux data (i.e., the generation rate of turbulent kinetic energy), but this likely requires more data and information, and, quite frankly, we are not quite sure how this aspect should or can be evaluated.

We suggest to include this aspect in the revision as a motivation for future research in the discussion section.

Reviewer comment 3: *One of the most striking results of this paper is that the pre-*

dicted J_{opt} shows remarkable agreement with the observed J_{obs} (Fig. 3). Also, one can see that J_{opt} (and J_{obs}) is generally smaller than R_s by a nearly constant value of about 50-100 W/m^2. This result can be explained directly from Eqs. (6) and (7) as

$$J_{opt} = R_s - \frac{R_{s,avg}}{2} - \frac{dU_s}{dt} \approx R_s - \frac{R_{s,avg}}{2}. \tag{2}$$

The last approximation ($dU_s/dt \approx 0$) is written in the text as a limiting case (page 6, line 26). Eq. (2) clearly shows that the optimum-state turbulent heat flux should be R_s minus a constant value ($R_{s,avg}/2 \approx 80 - 100$ W/m^2) for the most of the observation sites (Fig. 3A-D, F) where dU_s/dt is nearly negligible. I think Eq. (2) captures valuable information on the turbulent heat flux at the optimum (maximum power) state, and may be explained in the results or discussions of this paper. The readers of this paper will then be able to use it as a diagnostic tool for future investigations on turbulent heat fluxes obtained from observations and GCM simulations.

Reply: Thanks for pointing this out. We agree that this expression is very useful and will add this to the results and discussion section in the revision.

Reviewer comment 4: *I am somewhat skeptical about the region where $R_s \leq R_{s,avg}/2$ and J_{opt} becomes negative (Fig. 3). This region implies a situation of "inverse" heat flux from the atmosphere to the surface, and convective motion as well as convective heat flux cannot occur in this situation. The validity of the assumptions used in this study becomes questionable (even G and D become negative) under such situation. I think this can be a cause of the failure in predicting Jobs in this situation, in addition to the effect of the prevalent stable nighttime stratification in the boundary layer (page 8, line 6).*

Reply: Thank you very much for pointing this out. We fully agree and will mention and discuss this restriction in the revision.

Reviewer comment 5: *Page 1, line 21: "in by biases". Just typo.*

Reply: Thank you, we will correct this in the revision.

Reviewer comment 6: *I cannot see what is the meaning of the rectangular boxes (blue) in Fig. 2B. Perhaps a range of standard deviation? It may be good to explain this in the caption. Also, the "minus" sign in "$R_s - R_{s,avg}$" should not be in the lowercase.*

Reply: The blue boxes mark the first and third quantile, with the horizontal bar showing the median of the analysed data. We will clarify this in the figure caption in the revision.

Reviewer comment 7: *This figure should be made larger so that one can see the details of the results. Also, some of the arrows of J_{obs} and J_{opt} are not properly located for the corresponding lines and circles. It seems good to adjust them.*

Reply: Agreed. We will enlarge the figure and locate the labelling more properly in the revision.

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