In the following, comments by Referee S. Schymanski are indicated with [S] and replies by the author are indicated by [K].

[S] *The paper contains a range of interesting results and discussion, but I believe that the insights with respect to maximum entropy production or power presented in this study could be enhanced a lot and need some clarifications. I also found some potential flaws that need to be dealt with.*

[K] I would like to thank the reviewer for the constructive comments, questions and suggestions, which help to improve the manuscript.

[S] *First of all, the author did not explain how water fluxes are calculated in the model, whether hydraulic heads used for calculation of entropy production include gravitational potential and how fluxes out of the domain were modelled.*

[K] In the model, the fluxes are calculated based on Richards equation. Entropy production calculations are based on hydraulic head, which includes the gravitational potential per definition. Fluxes in and out of the domain are modeled as sources and sinks that come from the coupling with CLM. This will be elaborated in detail in the revised manuscript.

[S] *Power (representative of entropy production) is only calculated for internal flow processes, no calculation of entropy exchange between grid cells or for water exchange across the boundaries is presented. As the author points out on P. 5126 L1, entropy production or power is a positive quantity by definition, so I am uncertain how to interpret Fig. 2 with positive and negative values of average net power. In fact, the big red block with sharp boundaries in Fig. 2a looks like an artefact to me and should be analysed/discussed in more detail. At steady state, entropy in each grid cell must be constant, meaning that the entropy produced internally must equal the net export of entropy to the surroundings Schymanski et al. (2010). For a dynamic steady state, as assumed in the present paper, this must be true for long-term average entropy balance, so I believe that a detailed calculation of the entropy balance, not only entropy production, may give an additional indication of the consistency in the calculations.*

[K] The entropy balance is

$$\frac{\partial s}{\partial t} + \nabla \cdot J_s + \Gamma_s = \sigma \qquad \text{(i)}$$

where s is the entropy, t is time, J_s is the entropy current, Γ_s is the entropy source/sink, and σ is the internal entropy production of the macroscopic domain (always positive).

In our case, the divergence of the entropy current can be expanded as follows

$$\frac{\partial s}{\partial t} + \frac{\mu}{T}(\nabla \cdot J) + J \cdot \left(\nabla \frac{\mu}{T}\right) + \Gamma_s = \sigma \qquad \text{(ii)}$$

where μ is the chemical potential, J is the flux at the macroscopic scale.

Applying equation (ii) over one full cycle with a periodic source/sink leads to the temporally integrated form with ds/dt = 0 indicated by the overbar

$$\overline{\frac{\mu}{T}(\nabla \cdot J)} + \overline{J \cdot \left(\nabla \frac{\mu}{T}\right)} + \overline{\Gamma}_s = \overline{\sigma} \qquad \text{(iii)}$$

In equation (iii), at the microscopic scale, the entropy production and source/sink term at the land surface, because of evaporation/infiltration, can be expanded as follows

$$\overline{\frac{\mu}{T}(\nabla \cdot J)} + \overline{J \cdot \left(\nabla \frac{\mu}{T}\right)} + \overline{\sum_i \gamma_i \left(\frac{\mu_\gamma}{T}\right)_i} = \overline{\sum_i F_i J_i} \qquad \text{(iv)}$$

where F_i and J_i are the microscale forces and fluxes; and γ_i is the strength of the local sources/sinks, and μ_γ is the chemical potential of the sources/sinks. Thus, equation (iv) incorporates two scales: the divergence of the entropy current at the macroscale (first two terms), and the sources/sinks and entropy production at the microscale (remaining two terms).

In order to explicitly resolve the microscale terms, the hillslope is discretized at the microscale using the uniform grid of ParFlow (finite control volumes with two-point flux approximation). The fluxes in the domain are calculated based on Richards equation at isothermal conditions. Thus, the chemical potential is the hydraulic head (though I stick to μ here). The time series of sources/sinks (i.e. infiltration/evaporation) are obtained from the coupling with CLM, which calculates soil moisture dependent evaporation based on the Monin-Obukhov similarity principle.

Additionally, the first macroscopic term on the left hand side can be expanded to

$$\overline{\frac{\mu}{T}(\nabla \cdot J)} = \overline{\sum_i \left(\frac{\mu}{T}\right)_i (\nabla \cdot J)_i} \qquad \text{(v)}$$

where the divergence of the entropy current due to the divergence of the flux is calculated over individual grid cells and integrated over the full domain i.e. the hillslope (or the subdomains i.e. the recharge and discharge zones).

This part of the divergence, which can be positive or negative locally, was the variable to be plotted in figure 2 and may show different patterns based on the local dynamics of the hillslope and the periodic sources/sinks. In my opinion, these patterns reflect, which parts of the domain act as effective entropy exporters or importers. At dynamic equilibrium over one complete cycle, the periodic entropy sources/sinks are balanced by (1) the microscale entropy production (right hand side of equation (iv)), (2) the macroscale chemical potential and divergence of the flux (first term left hand side of equation (iv)), and a mean macroscale gradient and flux across the hillslope (second term left hand side of equation (iv))

$$\overline{J \cdot \left(\nabla \frac{\mu}{T}\right)} \neq 0 \qquad \text{(vi)}$$

because of

$$\overline{\sum_i \gamma_{in,i}\left(\frac{\mu_\gamma}{T}\right)_i} > \overline{\sum_i \gamma_{out,i}\left(\frac{\mu_\gamma}{T}\right)} \qquad \text{(vii)}$$

where $\gamma_{in,i}$ and $\gamma_{out,i}$ are the periodic infiltration and exfiltration fluxes at grid cell *i*, respectively.

This is proposed to be exploited in the upscaling of a macroscopic force across the hillslope and ultimately the derivation of an effective exchange coefficient. The complete entropy balance calculation is ongoing, and the expanded and revised theory and analysis will be included in the revisions of the manuscript.

[S] *Secondly, the author presents the mere existence of a maximum in power with varying conductance as indication that "power is indeed maximized" in the simulations. For this statement to be substantiated, it would be necessary to demonstrate that the conductivity resulting in maximum power is indeed the one a hillslope naturally assumes. This has not been done and hence all that could be concluded from this study is that maxima exist along the range of simulated conductivities. In this context, I am uncertain how to interpret the deduced "effective" conductivity and the fact that only one maximum in power is expressed over the range of calculated effective conductivities.*

[K] I agree that the maximum in power has only been shown along a range of simulated conductivities. No connection to reality has been established at this point. This will be clearly stated in the revised manuscript. Based on the comments of all Referees and the additional calculations performed so far, the analysis needs to be revised, the relationships need to be re-established including the interpretation of the results.

[S] *According to P. 5137 L4, the author calculates the effective conductivity as the ratio of average power to the square of average hydraulic head difference. This derivation seems flawed, as the mean of a ratio is not equal to the ratio of two means, and the mean of a square is not equal to the square of a mean. For a correct calculation, the variances and covariances of the variables need to be considered.*

[K] I calculated the effective force from the ratio of the average power to the flux, and with this force in hand I calculated the effective conductivity from the ratio of the average flux to the force.

[S] *I also failed to see the merit of deducing effective conductances and pressure heads from the numerical simulations, given that both of these vary in space and time. I think that derivation of effective static soil properties and effective hillslope or catchment geometry would be more useful. Or, even better, if a global optimum in Ksat with respect to macroscopic power or entropy production exists, it would be very interesting to assess if real hillslopes tend towards such an optimal value.*

[K] I agree that relating a global optimum Kast with respect to macroscopic power to a real hillslope and deriving static soil properties would be extremely useful. I fell that the derivation of effective conductances and forces is a useful step in this direction.

[S] *I hope that these and the below step-by-step comments will help to improve the manuscript and make it an inspiring and useful addition to the scientific literature.*

[K] I would like to thank the Referee again for his useful comments and suggestions

[S] *1. Throughout: The use of the term optimization is confusing. Entropy production is maximized by optimization of some system properties such as effective conductance or spatial arrangement. If entropy production were to be optimized, as stated in this manuscript, I would expect that something else was the objective function to be maximized or minimized, while entropy production was the adjustable lever. On P5125L3 the term "entropy production optimization (EPO)" is introduced. How does this differ from "maximum entropy production (MEP)"?*

[K] I agree that the term optimization is used inconsistently. This will be clarified in the revised manuscript. The idea of introducing the term entropy production optimization was, because apparently there is still debate about minimization and maximization of entropy production in natural systems.

[S] *2. Throughout: It would be helpful to remind the reader every now and then what the different abbreviations mean, e.g. S1 and S2 being homogeneous or heterogeneous Ksat respectively.*

[K] The suggestion will be honored in the revised manuscript.

[S] *3. Throughout: Please do not use the word "chaos" when referring to heterogeneity. Chaos has a specific mathematical definition and could confuse readers.*

[K] The suggestion will be honored in the revised manuscript.

[S] *4. P5124L13: inference tool*

[K] This will be corrected in the revised manuscript.

[S] *5. P5125L10: The property of being well mixed or not is unrelated to being stationary or non-linear. To avoid confusion, this section should be re-written and a discussion of macroscopic variables could also be added here.*

[K] With well-mixed I mean that there are no internal gradients. This section will be revised.

[S] *6. P5125L15–: The discussion of entropy production and associated variables is very confusing. What kind of entropy production is maximised, what are relevant system* *boundaries? The units given here are not consistent: Chemical potential should be energy per mass or per mole, entropy production should be energy per Kelvin per time, power should be energy per time by definition. Using the notation here, energy should be ML2T−2, but this is neither reflected in the units of entropy production, nor in the chemical potential or power.*

[K] The internal entropy production of the system (hillslope) is maximized, which is bounded by no-flow conditions along the bottom and vertical faces, and a free surface overland flow boundary condition at the top (that reduces to no-flow under unsaturated conditions, Kollet and Maxwell (2006)). The hillslope exchanges mass with the outside via sources/sinks (infiltration/evaporation), which are calculated by the land surface model CLM. I followed the convention by Westhoff et al. (2104), equation (1), which the Referee coauthored. Following this convention, entropy production has the units $ML^2T^{-3}K^{-1}$.

[S] *7. P5126L5: Clearer formulation: is optimised to maximise P.*

[K] The suggestion will be honored in the revised manuscript.

[S] *8. P5126L14–16: Kleidon and Schymanski (2008) did not show that entropy is maximised, they hypothesised that this might be the case. I was hoping that the present manuscript would test this hypothesis.*

[K] The statement will be revised in the manuscript.

[S] *9. P5126L24: Linearity is not needed to use an idealized box model.*

[K] The statement will be revised in the manuscript.

[S] *10. P5127L6–10: Need to explain what are the state variables that are supposed to be optimised for maximum entropy production or power.*

[K] Optimization will be clarified in the manuscript.

[S] *11. P5127L16: 2D horizontally or vertically?*

[K] 2D vertically, which follows from "cross-section of a synthetic hillslope" in the same sentence.

[S] *12. P5128L10–16: A conceptual drawing of the system boundaries and exchange would be helpful here. Why no-flow conditions? Where does the water go? How are the fluxes within the system and across boundaries computed?*

[K] A drawing will be provided. No-flow, because a cross-section of a closed basin or symmetric valley with a (dry) stream in the center of the valley was considered. The fluxes are computed with the coupled model ParFlow-CLM, in which Parflow simulates variably saturated subsurface flow and overland flow, and CLM calculates the infiltration/evaporation fluxes at the land surface. Both are coupled via sources/sinks in ParFlow and the soil moisture in the top ten model layers. There are a number of references on this; the most important details will be repeated in the revised manuscript for completeness.

[S] 13. P5128L17: maximum in P (not optimum)

[K] This will be corrected in the revised manuscript.

[S] 14. P5131L16: This implies no drainage, however on P5529L7 it was mentioned that drainage does occur in some simulations.

[K] A single simulation showed minor surface discharge out of the domain. The equation (4) and language will be revised accordingly.

[S] 15. P5131L19: What does it mean that infiltration equals negative evaporation? Why negative?

[K] At dynamic equilibrium, infiltration $Q_{inf} = Q$ equals evaporation $Q_{ev} = -Q$. This will be rephrased.

[S] 16. P5133L7–11: Would this stepwise representation of topography lead to an overestimation of power compared to a more smooth representation?

[K] I will perform additional simulations in order to interrogate this question.

[S] 17. P5133L15–19: I was not able to follow here. I did not see the circulation patterns or understand whether these bands in Fig. 2 have a meaning or are an artefact.

[K] The circulation refers to figure 1, where a net flow occurs from right to left (from the recharge to the discharge zone), which is expressed in the lateral gradient of *H*. The bands are explained at 5133, 5-11.

[S] *18. P5134L8–9: I see no evidence for the statement that there is some sort of maximization in the critical zone. Figs. 4 and 6 show that there are maxima as Ksat is varied, but the evidence that the hillslope tends towards such states is not given.*

[K] This will be revised in the manuscript.

[S] 19. P5135L13: These circulation cells sound interesting, but I was not able to see them.

[K] They are expressed as gradients in *H* in figure 1 (color gradients).

[S] 20. P5135L15: This is interesting. Does a random perturbation generally result in larger effective conductance, then?

[K] In this presented analysis yes. This must be double checked in the revised analysis.

[S] 21. P5136L1–3: This has actually been done in Schymanski et al. (2010): A "microscopic" spatially resolved ecohydrological model was transferred into a macroscopic 2-box model with effective parameters, which were optimised using the MEP principle, and the model results turned out to be very similar to the microscopic model simulations. Could this be attempted here as well?

[K] Yes, this could be attempted here as well in my opinion.

[S] 22. P5136L22: The equation given here is wrong, as the mean of a quotient is not equal to the quotient of two means. The mean H is equal to the mean of P/q, not to the mean of P divided by the mean of q, as long as P and q are not independent variables. Therefore, the interpretation of these results is likely flawed.

[K] This will be reconciled in the revised manuscript.

[S] 23. P5137L4: Again, mean should be calculated as the mean of all values of , not as the mean of P divided by the square of the mean H.

[K] This will be reconciled in the revised manuscript.

[S] *24. P5137L18–20: This conclusion seems unjustified. All the figure shows is that there is a maximum but not that the hillslope tends towards such a maximum.*

[K] This will be rephrased in the revised manuscript.

[S] *25. P5138L9–10: Again, the results do not indicate to me that a hillslope tends towards an MEP state, they just show that there are maxima in power for certain values of saturated hydraulic conductivity.*

[K] This will be rephrased in the revised manuscript.

[S] 26. P5138L20–24: I don't see the use of deriving "effective gradients in case of known net fluxes". Both fluxes and gradients vary in time, so what would we gain by this?

[K] For example, if we obtain effective gradients and exchange coefficients that can be related to observable quantities then we may be able to predict net fluxes based on these variables at different scales in the natural system.

[S] *27. P5140L9–11: It would be interesting to add an entropy balance to these calculations, in order to test whether the theoretical framework is indeed consistent.*

[K] This is already in progress.

[S] *28. Fig. 1: Why qualitative and not e.g. a logarithmic colour scheme? Shurely, the colour scheme does follow some mathematical transformation anyway. What is the meaning of evaporation greater 0 and infiltration lower than 0 for discharge vs. recharge? This distincition did not make any sense to me.*

[K] Hydraulic head may be negative in parts of the domain, therefore no logarithmic transformation. The sign of evaporation and infiltration follows the convention that the flux is positive along the positive z-axis.

[S] *29. Fig. 2: I had to look twice in the original manuscript to verify that the red bar was not a printout error. It may be good to mention in the caption that this is the actual result, and discuss in the text what caused it. Here, the scale could also be made quantitative.*

[K] The red bar is related to $\frac{\mu}{T}\left(\nabla \cdot J\right)$ in the divergence of the entropy current J_S, which has been discussed above. More explanation will be provided in the revised manuscript.

[S] *30. Fig. 6: The arrangement of a-d is different to Fig. 4, while the shape of Fig. 6a is similar to Fig. 4a. Is this coincidence or a mix-up of the axes labels?*

[K] This is a coincidence.