This study examines free energy conversions associated with subsurface water flows within numerical experiments at a synthetic hillslope. More specifically the author perturbed saturated hydraulic conductivity for a given set of soil hydraulic parameters in search for an optimum value maximizing entropy production/power in subsurface flows and elaborates on the role of the groundwater surface and on the role of purely random heterogeneity in this context. Furthermore, the author proposes an interesting approach how to use MEP for inferring/upscaling functional optimum, effective subsurface water characteristics within larger control volumes. Particularly, the last point is interesting and novel. The proposed study is, hence, without of high potential interest for HESS.

Nevertheless, I got the imprecision that the study is compiled a little too hastily, which is partly reflected in superficial referencing (as explained below) and the too narrow range of properties that is investigated within the experiments. The study would greatly from a thorough revision a) to improve physical rigor in the proposed approach how to estimate "power" within the model domain and to broaden the type and range of terrestrial controls on subsurface water flows and related free energy dissipation within the numerical experiments. I encourage the author, for whom I have high respect, to further optimize this potentially very interesting study by addressing the following points:

Major point concerning referencing:

- I appreciate the authors effort to refer to recent studies dealing with thermodynamic optimality in hydrology (some of those I authored/co-authored myself). The way how these studies are discussed is, however, not appropriate. After reading the introduction, one is left with the imprecision that this is the first study using "physics-based" models instead of bucket models shed light on the role of MEP in hydrology. This is not quite the case, for instance:
  - Porada et al. (2011) used a 1d-implementation of the Richards equation combined with a SVAT-approach and showed for the 35 largest catchments in the world, that the MEP optimized configuration plotted in a short envelope around the Budyko curve.
  - Zehe et al. (2013) used a 2-d Richards-model (coupled with a SVAT and 1 d overland flow) within numerical experiments exploring free energy conversions and dissipation associated with subsurface water flows and different runoff components. More specifically we perturbed the flow resistance in hillslope scale models and found the those values which optimize steady state production of entropy (or equivalently the reduction of free energy), perform acceptable in predicting rainfall runoff behavior two different catchment.
  - Kleidon and Renner (2013) proposed a simple model (based on three parameters) to predict land surface energy exchange based on the idea of maximizing power in the virtual and sensible heat flux and that the sensible heat flux operates at the Carnot limit. The model performs strikingly well when being compared against flux tower data in different land use settings during convective conditions.
- I very much agree that "physics based" models are superior for exploring to which extent thermodynamic optimality indeed applies; however, it is a matter of fairness to acknowledge that this has been addressed by the reported studies. Maybe the formulation PDE based or hyper resolution models is more appropriate as physics-based, as it does not imply the other models are not based on physics.

Major scientific points:

- The author propose essentially to calculate power based on in- and outward soil water fluxes into the model grid elements and local gradients, to summarize these values over time and then compile the spatial integral. With this he adopts a macroscale formulation of power (usually employed at the system scale using fluxes across the boundaries and macroscale gradients) which applies for steady states in gradients and fluxes to the microscale. This is due to several reasons inappropriate because the grid scale gradients get depleted by the fluxes within a simulation time step means there is no steady state. Secondly, soil water fluxes are essentially dissipative by their nature (pushing the system back to local thermodynamic equilibrium) and kinetic energy associated with soil water fluxes is marginal (this where power could be extracted from to perform work). The calculated term is thus not power (a source term in the free energy balance) but dissipation (the sink term in the free energy balance). As eq. 10 deals in fact dealing with dissipation, the proposed summation in is hence not appropriate, as positive and negative terms cancel out. (The latter wouldn't harm when dealing with a system conserving free energy, which does not perform work when traveling along a closed path). One can think about a case where all summands in Eq. 10 cancel, the equations suggests that nothing happened at all; although gradients have been depleted and re-established many, many times and which implies dissipation of energy. Maybe, the way to go is to multiply the net flux into a local grid element with temporal change in the local potential (see post discussion of Zehe et al. 2013 for the details) or to use the method proposed in Zehe et al. (2013).
- The authors needs to show that his analysis reflects steady state behavior by compiling longer simulations using a longer time series reflecting the more than one year of forcing conditions.
- I d' like to encourage the author to better link the numerical experiment to natural systems and to explore a wider range in natural controls. The proposed simulation domain has, expect of a very small and homogeneous topographic gradient, not much in common with a natural hillslope. Also the soil should be better characterized than just dropping the van Genuchten-Mualem parameters (similarly the boundary conditions). Even when the author prefers not to deal with transpiration, it is straight forward to compare for instance fine and coarse porous soils (with different importance of capillary pressure in this concert), different topographic gradients (to come closer to a hillslope) and eventually forms.
- The reported dependence that purely random heterogeneity in ksat yields to an elevated maximum in dissipation is by far too interesting, to treat it in such a brief manner. How does this depend on the variance of ksat (which I missed by the way in the manuscript)? How does this change when adding a spatial co-variance? Is it a steady decrease within increasing correlation length, or is there a maximum dependent on the correlation length? This is particularly interesting as correlation lengths in natural soils are short (several meters).
- The last part dealing with MEP as an inference principle and the suggested approach to estimate a macroscopic gradient and conductance is for me the most interesting part of the study. However, the provide evidence does support the conclusion that MEP applies indeed to hydrological system in the sense that an optimized effective conductance performs well against observations. This implies a) to show that optimum hillslope structure have predictive power against data and that the up-scaled effective conductance works well when being used within a water balance simulations carried out at larger grid cells (as for instance

shown by Lee et al. 2007). Westhoff and Zehe (2013) showed that MEP is not useful for conceptual modelling, as the optimum parameters had not predictive power for the water balance; Zehe et al. (2013) reports to 2 successful cases, which can be just a coincidence). So there is still room for more evidence here.

Technical details:

- Please not that Eq. 1 is only valid, in case of mass flows which are driven by chemical potentials and during steady states.
- Power is as a flux an intensive property and can thus not be imported/exported or balanced. Please refer to in/export of free energy which is an additive quantity.
- Hourly differences of P-ET are not equal to infiltration but equal to infiltration and surface runoff (Eq. 4). Why not using the influx into the model domain?
- How does the model deal with the saturated zone- in an iterative manner allowing for a free surface or by using storage coefficients?
- Is it not really astonishing, that a saturated domain builds up as you use a no flow boundary?
- Page 5131 typo: toward by towards
- The gradient should point into the opposite direction of the flux (which reflects the second law of thermodynamics) not into direction of the flux
- Maybe replace small scale chaos with small scale disorder

References:

Kleidon, A. and Renner, M.: A simple explanation for the sensitivity of the hydrologic cycle to surface temperature and solar radiation and its implications for global climate change, Earth Syst. Dynam., 4, 455-465, doi:10.5194/esd-4-455-2013, 2013.

Lee, H., Zehe, E., and Sivapalan, M.: Predictions of rainfall-runoff response and soil moisture dynamics in a microscale catchment using the CREW model, Hydrol. Earth Syst. Sci., 11, 819-849, doi:10.5194/hess-11-819-2007, 2007.

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