



Reply on RC2

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Author comment on "Hortonian Overland Flow, Hillslope Morphology and Stream Power I: Spatial Energy Distributions and Steady-state Power Maxima" by Samuel Schroers et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-79-AC3>, 2021

First, we thank the reviewer for her/his time, effort and her/his encouraging comments.

The main idea of our work is indeed, as pointed out by the reviewer, to show that the free energy balance of overland flow on hillslopes as runoff-runoff system behaves distinctly different from river systems. Downslope flow accumulation and the decline in geopotential implies downslope increase in potential energy up to a maximum. Honestly, we regard this not as a trivial finding, at least the study of Kleidon et al. (2013) overlooked this difference.

In line with the reviewer, we think that these spatial maxima in potential energy relate to a transition zone and a related switch in dynamic behavior or at least a local hot spot. Such transitions are the onset of erosion, as the critical shear stress is exceeded, a transition from sheet to rill flow and erosion or the emergence of turbulence. The latter relates however more to the Reynolds number and thus overland flow velocity and depths. While velocity is regarded as constant during steady state conditions, it might change with time when exploring the transient evolution of an overland flow wave running downslope. While this is out of the scope of this work, we plan to investigate this in a follow up study.

We will clarify this main point in the revised manuscript, formulate a related hypothesis about the role of the potential energy maxima and test this using available rainfall simulation experiments at 10 m long stripes, which investigated Hortonian overland flow formation and erosion/ detachment in the Weiherbach catchment (Scherer et al., 2012). We already started to simulate those experiments with a spatially distributed numerical model (runoff and erosion). The output is well suited to explore the emergence of these potential energy maxima and their relation to erosion and emergence of rill flow.

We agree that the manuscript is currently overdoing the idea of reproducibility. We will thus reduce the theoretical background to the minimum necessary amount, and present the remaining details in the appendix. Yet we think it is important to show those, as thermodynamics is not part of the standard hydrological curriculum. The advantage of the concept of free energy, is that we can assess Dissipation as a residual of the energy and the free energy balance.

We agree that a hillslope in steady state with $P_{eff}=50\text{mm/h}$ will not stand for a long time.

We will clarify that we refer to the steady state phase during a rainfall event. The aforementioned experiments of Scherer et al. (2012) were carried out using 60 mm of rainfall in 1 h. At some sites the runoff coefficient was 80%, implying an effective rainfall of 46 mm in 1 h. While this caused substantial erosion, the hillslope still exists. We furthermore think that steady state overland flow conditions during events might occur less often than assumed, as we implicitly impose them when using steady state approximations of the Navier Stokes equation to simulate overland flow. Only rainfall events with sufficient duration will cause steady state phase, following on a rise and followed by a recession phase. This question is beyond the scope of this work but will be addressed in a follow up study.

For the presentation of the general theory however we think that it is valuable to explore different effective rainfall intensities/ infiltration excess rates in order to highlight that stream power is direct proportional to rainfall intensities. We will, however, better explain that such high intensities will cause substantial, transient erosion and explain that this will rarely occur.

Referring now to the third and last paragraph of the review we thank Rev 2 for the discovered minor errors. Indeed, since we exclude infiltration P_{eff} and I represent the same value. Next, we are not intending to claim that biological and river networks share significant similarities, rather we intend to note that both fields had a very similar motivation in describing networks, that is thermodynamic principals. In L194 refers to the influx of energy caused by precipitation in comparison to the influx of energy by upslope runoff Q , we agree that this sentence needs reformulation. L207, we are aware of the problematic by describing sheetflow with channel flow equations, however we think that there is a need to develop more robust concepts of very shallow flow, the parameterization of friction and the resulting energy dynamics. The interesting observation for hillslope surface runoff is its transitional nature and we believe that understanding the underlying energetic dynamics might help separating prevalent flow characteristics (such as laminar and turbulent flow). Cases 3 and 4 of table 2, have been added for comparison. One could argue that they add not additional value but we think it is interesting to highlight that both derivatives, of discharge and flow velocity contribute (although to a much lesser extent) to dissipation.

Overall, we thank Rev 2 again for his observations and findings. We think the incorporation of an empirical case is a good idea and further agree that readability would benefit from a more concise straight to the point introduction.