We, (Erwin Zehe as EZ on behalf of all co-authors in the following) sincerely thank the anonymous reviewer for his helpful assessment of our opinion paper (CAOS paper in the following).

Reviewer: Is this meant an opinion paper?
EZ: We think this is indeed an opinion paper as it reflects our opinion at two levels:

- We need a publication culture that allows sharing of our scientific failures, because there is much to learn from it. I think an opinion paper is the right tool for this.
- It reflects our opinion on how to advance our predictive understanding of how spatial organization controls intermediate catchment functioning (this is not a novel problem but not solved (we explain this after explaining the first point).

What was our idea on how to address the first point? The hypotheses, ideas and approaches presented in the CAOS paper have been accepted by an international jury of experts. Before we will publish our findings from two years of research work in the forthcoming research papers, we wanted to share the true initial ideas and a priory hypothesis beforehand. The underlying idea this is to share not only the success stories but also what we learned from our failure with the community, because some the findings are line and some are pretty surprising. We did this certainly not to advertise our project and will reduce our referencing to it to the necessary absolute minimum.

We admit that we have maybe overdone it a little with this idea, especially when it comes to section 4. Again this has a good reason. EZ was editor of one of the very first opinion papers by Savenije (2010) introducing his idea of the flex- topo modelling in HESS. A major critique in the first reviewer round was that the author should point out ways to show that his proposed model concept is indeed structurally more adequate than others. Section 4 is part of this paper, because we did not want to stop at the stage of just arguing that the presented EFU concept is (partly) experimentally testable but share our ideas how we are going to do this. This section has partly the character of a proposal, simply because it is a proposal how to tackle this problem. Adding more results to this section is not possible, because it is the privilege of the PhD students and Post Doc to publish their main results with their names at first place. Hence, we will reduce the length of this section to the minimum necessary amount.

Still we would like to stress that the proposed experimental design is at least pretty rare, if not unique with its effort to conduct replicate experiments and monitoring at members of EFU and hillslope that are expected to function similarly (of course there is B2-LEO). We are well aware of Terreno as the two authors Theresa Blume and Peter Dietrich coordinate experimental activities in the Terreno Müritz/Ücker and Terreno Bode observatories.

Reviewer: Past work on similar issues should be acknowledged
EZ: With respect to the second point - the old problem of organized complexity – we present several novel ideas that could due to our opinion bring new momentum to our understanding. These are motivated by the HRU idea and earlier work that proposed simplified but physically consistent model approaches for larger control volumes (REW approach, Bousinesq model) and our working believe that the catchment is an organized fingerprint of past processes. In fact we went back to the classics with our referencing with respect most contributions (maybe not all) which motivate or concept

(organized complexity, HRU definition, the catena concept, the pattern process paradigm, predictive uncertainty and organizing principles). We will add references to the REW and other promising model concepts (hillslope storage Bousinesq model) in the revised manuscript and discuss their pros and cons with respect to the balance of model complexity and simplicity.

Our main points are quickly summarized in the following:

1) A thermodynamic perspective on hydrology is useful (while not novel per se) as it implies/reminds us that a flux is equal to a "potential gradient" $\nabla\phi$ (temperature gradient, water level gradient, concentration gradient, soil water potential gradients) divided by a resistance R (inverses of either heat conductance, surface roughness, diffusion coefficient, hydraulic conductivity…). The former determines the (thermodynamic) force the latter determines dissipative energy losses along the flow path:

$$\vec{q} = {}^{1}/_{R}\, \overrightarrow{\nabla\phi} \text{ (Eq. 1)}$$

In larger control volumes of terrestrial systems both gradients and resistance are fields and depend on almost static controls and on system state variables (all this is well known).

In this framework HRUs or functional units can be defined as classes of landscape entities/control volumes with similar terrestrial controls on the pair of $\nabla\phi$ and R. on the pair of gradients $\nabla\phi$ and resistance R controlling either land surface energy exchange or rainfall runoff. (Note this this is necessary conditions for functional similarity, but not necessary and sufficient conditions as Eq. 1 is not an unique equation). According to Flügel (1996), 'Hydrological Response Units are distributed, heterogeneously structured entities having a common climate, land use and underlying pedo-topo-geological associations controlling their hydrological transport dynamics'. We think the definitions match well, our one has maybe a little bit more physical rigor <u>and</u> that it implies the proposed hierarchy of functional units (EFU and lead topologies) instead of a one fits all HRU and that their dominance changes dynamically with prevailing boundary conditions (this is thoroughly explained in our response to Keith Beven).

2). Eq. 1 is immanently subject to equifinality because it is not an injective function (several elements of the start domain are mapped on the same element in the codomain):

a) Several combinations of gradients and resistance compile the same flux. This might be frequently the case in hydrological systems as the (quasi static) controls especially on gradients driving lateral flows of blue /free water are independent from the properties controlling flow resistances (especially when it comes to vertical and lateral preferential flow paths). In line with the argumentation of Bardossy (2007) the first source of equifinality can be reduced based on observations that characterize at least two out of the three variables (either q and R or q and $\nabla\phi$, or $\nabla\phi$ and R). Current observation technologies allow approximate characterization of the static controls on gradients driving lateral flows of blue water (surface and bedrock topography). This is why we think a coupled treatment of the mass and momentum balance has more pros than cons and stands as the main supportive argument for hypothesis H2.

b) The R term is no unique as resistances in Eq. 1 reflect the spatial heterogeneous and spatial organized arrangement of for instance soil material in the control volume. Subsurface flow resistance depends for instance on soil hydraulic conductivity $1/k(\theta)$, its covariance lengths and soil moisture. Connected structures (preferential pathways lateral pipes, vertical macropores) reduce the control volume resistance at a given driving gradient as they allow for advective flows, resulting in accelerated fluxes and shorter residence time distributions. Preferential flow networks with different topologies and hydraulic properties may result in the same control volume resistance (e.g. Klaus and Zehe 2010). This source of equifinality cannot be eliminated and has to be accounted for. To our opinion there are 2 promising avenues to tackle the resistance problem. As some preferential pathways are created by ecosystem engineers, there is a chance to get approximate information on their spatial pattern using species distribution models as explained in the CAOS paper. Organizing principles allow for a priory optimization of the resistance term at a given gradient, either as a bulk resistance (Porada et al. 2011; Westhoff and Zehe 2013) or the density of vertical and lateral macropores (Zehe et al. 2013; Kleidon et al. 2013). This implies the possibility of independent predictions. These can of course go wrong, but this is testable! And this is the main reason for hypothesis H3.

3) We think that hydrology lacks realistic and falsifiable models at intermediate scales and we share or opinion why this is the case and how to fill this gap by formulating clear criteria. The first is an explicit at least one dimensional accounting for the momentum balance, the second is spatially explicit accounting for preferential flow paths (especially the lateral ones which cause advective exchange between EFU during rainfall driven conditions). In the context we do not think that the problem is in the equations themselves. We have either diffusion like equations for diffusion like problems, advection equations for advection problems and advection dispersion equations. There is a problem when using the inappropriate equation for a certain type of flow problems. In this sense the Darcy equation is inappropriate when flows get preferential (advective) or the use of the advection dispersion equation is in appropriate at low Peclet numbers. So where what is the chicken and what is the egg?

Advection implies high velocities even at small gradients which essentially requires low dissipative losses. We thus regard the presence of connected preferential pathways as the chicken, which has to be well represented in the model, and fast flow as the egg. In line with this we regard the topology of the flow network as the key for a structurally adequate model structure. Otherwise hydrologic routing schemes couldn’t work at all, because they preserve the network topology but violate the “flow law”. Similarly preferential flow and transport in soil can be well predicted based on assuming Darcy flow, which is not correct, when the topology of the flow network is well represented. What is thus falsifiable?

- The spatial structure of the model, (it representation of the covariance structure of textural properties and the topology of preferential pathways),
- How processes interact among different domains (bidirectional, unidirectional)…

Our benchmark for this is spatial transferability of the structural and functional model sets among class members of the same functional unit.

We will revise the manuscript to better explain these main points. We again thank the reviewer for his effort and the helpful comments, Erwin Zehe

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