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Hydrology and Earth System Sciences
Discussions

Interactive comment on "HESS Opinions: Functional units: a novel framework to explore the link between spatial organization and hydrological functioning of intermediate scale catchments" *by* E. Zehe et al.

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I have to say that as I read through this paper I became more and more disappointed. There seems to be an increasing tendency in the hydrological literature of papers that claim to present novel concepts when they are really mostly reinventing or restating past concepts. In some cases as here (but it is certainly not the only example I have seen recently) they do so without citing earlier seminal papers. This may not, of course, be deliberate (though it is really hard to see how this group of authors can fail to give

any mention at all in this paper to the REW work of Paulo Reggiani et al. – especially in relation to Hypothesis H2 - when some of them have been involved in implementations of the REW concepts) but it is (to be generous) a failure of scholarship. The driver for this seems to be a need to designate a paper as "novel" so that people will cite it as the origin of a research theme in future.

But there is not much that is novel here. While I agree with the authors that the designation of HRUs has often become a GIS clipping exercise, this is not necessarily the case. I do not have access the Flügel Erde paper that the authors cite as the origin of the HRU concepts (it is of course much older than 1996, see below, even if the name was introduced then) but other papers by the same author and in the PMRS implementation make it quite clear that there was always an intention that HRUs should be linked downslope (just look at the pictures!!). Nor is there any mention of the approach of Dynamic Topmodel that defines similarity at the point scale and groups "similar" points into calculation units with surface and subsurface downslope routing.

So what is added as novel in the EFU concept as an alternative? It seems to me that it is only the idea of the self-organisation of soil and vegetation units in "similar" situations. As some of the authors know very well I am not yet convinced of the value of self-organisational principles to practical hydrological modelling, when (a) we cannot know the past history of changing boundary conditions, including the widespread effects of man on "natural" vegetation communities on what we see now, and (b) self-organisational changes in response to externally imposed boundary conditions becomes almost circular in its reasoning. This has been discussed in exchanges in HESSD in the past (http://www.hydrol-earth-syst-sci-discuss.net/7/7779/2010/hessd-7-7779-2010-discussion.html) in which I suggested that more practical demonstrations of the utility of the concepts were needed. There is still nothing in this paper that goes beyond the statement that these concepts will be useful other than citations to past papers that I, at least, have not been convinced by.

This is a very distinguished set of authors but I would respectfully suggest that they overstate their case in terms of both novelty and the utility of the concepts they suggest and I think the paper should be rejected in this form I would be happy to see a revision of this paper that demonstrated that the novel part of what is being suggested (the utility of optimality principles at the catchment scale) could be useful, but I cannot otherwise see anything in this paper that goes further than past work in terms of different process representations, that properly allows for both hydrograph and tracer responses, or that solves the closure problem for their units, or the inverse problem. Interestingly, they do not put their suggestions in the context of my 2006 Holy Grail paper that discussed just what was needed for a description of processes in a hydrological functional unit.

Some specific comments

3253 L16. There are useful summaries of subsurface preferential flows in the 2010 HP Annual Review Special issue – eg. Jones (2010) and Chappell (2010)

L22. Entropy is commonly used in this context but is really not a great analogy here since it deals only with a distributional form. The change of that distribution over time, given the uncertainties in describing the system, is only a very crude reflection of the system dynamics.

3254 L10 That depends - it could surely be argued that Imbeaux (1897) was also already recognising that spatial structure leads to complexity of responses. See also my 2004 paper on Horton's work on spatially distributed responses.

There are some phrases I do not understand – perhaps a problem of translation into English? Superordinate gradients? Lead topologies? Superordinate lead topologies?

3256 We postulate that a hierarchy of functional units, lead topologies and elementary functional units (EFU), compile the main catchment functions in a given hydrogeological setting by spatially organized interactions at and across different scales

Why should these be units as in HRUs rather than similarity of "points" as in Topmodel similarity (not just the topographic index but also the more flexible definition of similarity

based on other relevant criteria in Dynamic Topmodel, e.g. Freer and Beven, 2001, where similar points are grouped together into units that might not be contiguous in space because of the expected functional similarity.

3257 it offers a quite unique range of physiogeographical settings???? Really? Somewhat typical of sites in the Ardennes perhaps but only unique in the sense that every catchment unique in its details that might affect the hydrological response.

And in the Attert, is not everything dominated by the geology, including the lack of water balance in some subcatchments? This clearly (together with the Pleistocene and holocene history) will have affected the soils and natural vegetation but appears today to be the dominant boundary condition. And what we see now is not a natural vegetation but a managed landscape, continually subjected to changing anthropogenic boundary conditions. Where is the self-organisation evident in this example?

3259. Transport distances are thus too small to treat flow and transport in the hillslope subsurface as being well mixed . . .. This seems to be a misinterpretation of preferential flow as pipeflows. But the preferential flow pathways within the soil (and sometimes at the surface) are not generally continuous. Plenty of evidence that hydrographs involve displaced "well mixed" water of longer residence times - the Kirchner double paradox. Need to consider difference between velocities and celerities here and, in fact, whether the preferential flows are really important in controlling the residence times. They are certainly important to creating non-Gaussian residence time distributions but how much evidence is there that they dominate contributions to the hydrograph.

P3260 Mobilization of pre-event water by means of pressure transduction is controlled by the specific storage coefficient of the aquifer (Tetzlaff et al., 2012). Again, you should really recognize earlier contributions to this idea (which were available as an explanation long before the Kirchner paradox paper)

3260 Also the problem of equifinality (Beven and Binley, 1992) can partly be attributed to a lumped treatment of "driving gradients" and "flow resistances", as explained in

Sect. 2.2.1.

ok this statement is qualified by partly but as the authors should know I have often pointed out that even the “perfect model” which would have many parameters to characterize the flow pathways would not be free of equifinality if those parameters cannot be estimated independently (and accurately). The momentum equation is actually more difficult to close than the water balance equation given both aleatory and epistemic uncertainties and therefore will not provide strong additional constraints. It is a real paradox that the more process information we build in to a model, the greater the possibility of equifinality might be.

And Beven and Binley (1992) did not actually mention equifinality. The authors should again be more careful about their citations.

L26 where green water

3261 and latent heat and finally transpiration. But transpiration often controls latent heat flux through stomatal resistance not as a consequence of the partitioning.

3263 This section is annoyingly similar to Beven (JH 1989). These problems were recognized a quarter of a century ago.

3264 Is falsification really impossible because no single person understands all the reductionist model process? Are not all models based on Darcy-Richards easily shown to be false in that they are based on the wrong experiment and wrong concept of equilibration of potentials and gradients? Is it not also true of the use of sheet flow equations to real surface runoff? And that is at the small scale, let alone at the scale of an EFU that is being suggested here with all the underlying heterogeneities. At that scale the nonlinear physics means that Richards should not hold, even if it is valid at the local scale (unless we have a homogeneous porous medium with no structure and no roots etc). Where is the problem of falsification?

H1. OK, but why is it necessary to invoke similarity here rather than a simple hierarchi-

cal structure? I can think of a very good reason but it is not actually stated here.

H2. OK, but what about the closure problem, especially for energy and momentum in EFUs of limited extent.

H3. OK, it is only stated as a hypotheses, but I remain dubious about its value as a constraint given the potential for unknown past external perturbations.

3268 are controlled by gradients in soil water potentials – perhaps but not in the way you suggest when the roots grow towards the water faster than the water can move to the roots. . ..

And what is your REV here? Are your potentials and gradients definable in practice?

3269 L25 This is simply wrong – HRUs do not deny lateral exchanges, only some implementations of such a concept.

3271 consist of several EFUs??? Defined how? And as with the REW concepts you cannot test any of the balance equations for “similar” sites without some means of closure. What will make these tests meaningful?

L10-25 How is any of this relevant to testing the EFU concept?

3272 Eventually, we expect to infer from information related to storage, release and isotopic signatures (in precipitation and stream flow) in our nested catchment set-up to gain new understanding on what controls dominate storage, mixing and release differences across catchments and scales.

Same question for all of this 3.3.2. There will be differences, largely geologically controlled. That we already understand so just how is this new understanding going to be developed?

Lead topologies again? Still not clear why word lead is used.

3274 they are deemed to be of minor importance for green water fluxes sustaining the energy balance during radiation driven conditions. – DURING radiation driven conditions yes, but the partition between blue water and what is available to become green water is surely important here – the plants can only work with what is left in the soil after drainage, either as direct storage, or through capillary rise where water table remains close to the surface especially at the bottom of slopes where it may be the network of bedrock fractures that makes water available over a longer period. But this will be dynamic - How can you extract distinctly different EFUs ?

L26 where ranks

3275 soil moisture variability would be depleted by lateral soil water flows driven by lateral matric potential gradients. OK, so that then implies that there is inhomogeneity of soil moisture characteristic curves if gradients really are small, or is it that you are apparently thinking that potentials and gradients can be defined at some EFU scale when it might be quite local gradient interactions with the roots of different plants and losses at the surface that maintain the ranking.

3276 Reductionist models are thermodynamically consistent – yes but hardly explicitly in that much of the slack in closing the energy and momentum balances is taken up in various (interacting) parameters. It is still not clear to me about how adding one of the various optimality constraints will help in this, with different criteria leading to different trader-offs.

3277 Much of the evidence presented here is not convincing. In particular why should such systems that are event/relaxation driven work to satisfy some steady state condition, especially when extreme events might change the boundary condition (through debris flows, channel changing floods etc). In particular why should preferential flow pathways that might be formed by external factors that are only directly hydrologically related - death of trees during the chestnut blight of 1937 in the Appalachians - or the pipe systems of mid-Wales that might arise from soil cracking in dry summers with return period of 20 years or more – satisfy some thermodynamic optima?

3277 In summary we propose that a thorough understanding of the behavior of a few representatives of the most important EFU classes, their interactions within lead topologies,as well as of how and when the lead topologies connect to the river, contribute to close the gap in our understanding of how distributed dynamics alongside with spatial organization translate into integral catchment behavior.

I really do not see how what has been presented before allows this conclusion. Nothing has been given as to how to define an EFU except in very broad brush "similarity" terms, nothing has been suggested as to how to effect closure of fluxes between EFUs (as is the case for the REWs before) which is surely required for any testing of the concepts "on replicates"; the acceptance of optimality principles as a useful constraint is still a matter of belief (with to my mind some strong counter arguments in favour of event-relaxation concepts); and there is no mention of how to carry out hypothesis testing in the face of different sources of uncertainty. Replicates will be different – but how to tell if they are significantly different, or how different explanations of their response or more or less acceptable?. These questions are not answered in what follows in section 4. Instead we have a rather qualitative classificatory approach to the results obtained in the Attert Basin.

3284 Section 4.2.1 This is not really an adequate description of how the CHAOS model might relate to the definition of EFUs and how the process descriptions might be different from previous HRU models. In particular, if partial differential equations are not going ot be used, but gradients are really important in the thermodynamics, just how are the gradients going to be incorporated? Also how are the celerities going to be handled relative to the velocities?

3285 Richards? But Richards is, in fact a partial differential equation, and is already falsified for dynamic flow conditions.

Jackisch et al. is not in the list of references but particle tracking approaches have been used before this for flow and transport by Ewen and Davies and Beven.

3286 Where is the novelty in this formulation? And you slip from preferential flow to a pipe flow network without justification. Preferential flow pathways on hillslopes are not necessarily continuous, and are certainly not always pipes. Previous comments have implied heterogeneity in the soil condition but here we have only a field capacity. There is again an inconsistency in previously suggesting that you would not use a differential equation approach and here suggesting a diffusion wave for surface flow. This does, of course, have analytical solutions for some boundary conditions but not for the depth variable infiltration condition. And how is the Cauchy boundary condition going to work on a heterogeneous soil for which the patterns are unknown?

3287 A key issue related to this, which is not discussed in this manuscript, is how to find the appropriate metrics for assessing similarity from geostatistics, graph-theory, mathematical morphology and multivariate statistics.

But is this not absolutely critical having criticised the HRU identification metrics in the past? You suggest earlier that this has to be related to function – ie. dynamics – but then all of the suggestions here are static. This surely has to be addressed in a revision of this paper.

The other key benchmark for the proposed concept is transferability of acceptable model parameter sets among members of the same EFU or lead topologies.

This was the obvious reason I thought of in relation to Hypothesis H1 above but transferability has not been mentioned before.

Finally what about the inverse problem? The model structure as outline crudely here involves a wide variety of parameters without any indication about how these will be identified (in a way that avoids equifinality of course) from the suggested measurements or organizational principles in practice. I do not see any real novelty here except in the very contentious issue of applying one or other of the optimality principles in constraining responses at the catchment scale.

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