

Interactive comment on “HESS Opinions “Hydrologists, bring out shovels and garden hoses and hit the dirt”” by M. G. Kleinhans et al.

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I like the tenor of the proposed manuscript much and agree that controlled independent experiments may offer an important peace to the puzzle of understanding the water cycle. It is well argued, well written and calls for discussion (which is the idea of an opinion paper). I guess, as this is an opinion paper, I cannot come up with “recommendations” but merely with a couple of thoughts/ comments that might help to clarify things, avoid misunderstandings and “discuss”. Thanks for throwing the ball in such a nice manner.

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- line 20: Maybe use reductionist physics based, because conceptual models are still based on physics (they assume mass conservation, don't they)

-I prefer model structural uncertainty as term compared to equifinality, because this addresses the key problem that several structures (in their sense spatial model structures) fit the observed response equally well. The dilemma is that these structures can be very, very different, equifinality wouldn't be a serious problem otherwise.

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- Third bullet: we cannot measure processes, we measure states or fluxes and infer backwards on processes

- Fourth bullet: What do you mean exactly here, I guess reductionist approaches? We apply physical laws all the time in conceptual models (see above)?

- Line 20: Topography and shape is not a good example for things that are difficult to observe.

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- Line 20: You cannot use a model to test whether observations conflict with physical laws! Both observations and the model are based on physical principles. You can use the model to test whether our perception about for instance a hillslope structure or interactions with groundwater etc. are sufficient to reproduce observations. If not, this does not compromise physical laws, but hints that our perception is wrong.

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- I think you cannot verify things, if they claim to have universal validity. Otherwise you can of course verify something that just aims at a unique case and place etc... This is the old crux of positivistic science theory.

- You can of course falsify things in hydrology, best example is the very controversial discussion about preferential flow: that was first neglected, then accepted as some-

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thing exceptional, than as the merely rule You can for instance falsify the hypothesis that matrix flow is sufficient for explaining pesticide breakthroughs at many places of the world (Flury, 1996 JEQ, Zehe and Fluehler, 2001 JoH).

- Line 25: I wouldn't call this a mess, this is beauty, at least for me ĩAŁ

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- Using models to conduct virtual experiments maybe indeed very helpful, as shown by Weiler & McDonnel 2004 JoH, Zehe & Bloeschl, 2004 WRR, Zehe et al. 2007 WRR, or Zehe et al. 2006 HESS. However, this cannot provide experiments that are independent from the theory that has to be tested. This is were independent experiments come in, at least in physics.

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- I disagree that you cannot call field experiments "experiments". There is certainly less control on boundary conditions. However, I would not underestimate what can be achieved by a proper stratified sampling design to test a well defined statistical hypothesis (maybe you check for instance Helmut Elsenbeers work). I agree however that it is very difficult to work this way.

- In this line, controlled experiments in systems that are not representative for natural systems (too small/too homogeneous) maybe to a large degree useless. Lysimeter studies conducted to assess mobility of pesticides in natural soils are a good example (there is a bunch of literature in the 80/90 on this issue for instance on uselessness of dispersion coefficients derived with undisturbed columns). - To stay within this line, there is currently the idea to build "king size Lysimeters" (Chicken creek) to learn about our capability to conduct independent predictions. The problem here, is that you cannot geometry of these things either. . .

In general I disagree that we cannot physical build that is valid in general, I think the REW approach is a good counterexample (though not perfect yet). It separates clearly

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what comes from first principles (balance equation) from things that become site specific (closure, there is a nice HESS special issue on this). I think this is the key to go. In meteorology nobody kicks the model into the dustbin, because closure relations for turbulence (that imbed geometry and all nice things with them) doesn't work. They go for better ones.

Best regards and merry Christmas,

Erwin Zehe

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