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Discussions

Interactive comment on "Advancing catchment hydrology to deal with predictions under change" *by* U. Ehret et al.

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It is difficult to know how to begin to review such a paper since one cannot disagree with any of the general principles that are expressed but where none (or hardly any) of the practical difficulties of applying those principles are discussed. The paper is then much more of a fashion statement about what a future hydrological science might hold than a practical guide to the way ahead.

Clearly the authors can then simply respond by suggesting that the details are in the papers cited. But there are examples of articles being cited positively even though in many of those papers there are real difficulties of how far the principles applied are properly justified (I am thinking of Clark et al., 2011, Gong et al., 2012; Zehe et al.,

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2012, ... there are certainly others).

I think the thing I object to most is the suggestion that hydrologists have been stuck in an assumption of stationarity for so long. This is a rhetorical device as a way of contrasting a misguided past with a brighter theoretical future but is simply not true (you can go back to Davis's geomorphological theory, Horton's landscape development theory and seasonal patterns of infiltration, impacts of urbanisation etc etc etc see also examples below). Assumption of stationarity for the purposes of identifying the parameters and structures of predictive models has been a response the practical difficulties of underdeterminism (and even then there have been studies looking at how inferred parameters from both measurements and model calibration change over time).

The big question, that the authors do not really answer here is whether any of this brighter future will really constrain the underdeterminism in predicting a changing (but unknown) future that will change the boundary conditions for their constraints.

They also only mention improved measurement techniques in passing. That cannot help in assessing future change, of course, but it is arguable that this might have a much more dramatic impact on the representation of current catchment responses than any of the principles invoked in this paper.

Some specific comments

P14 Advances in measurement techniques, in particular about spatial patterns (e.g. Grayson et al., 2002), helped address the dispute as they provided new information to identify at least some of the model parameters.

Is this statement actually true (in the sense it is being used here rather than the simplistic sense that more information might constrain some parameterization)? If so how?

P16 Hydrological systems, as systems of "organized complexity", exhibit a mixture of both dimensions, being roughly predictable under some conditions and at certain scales but unpredictable at others. Waldrop (1992) termed such systems as being at *the “edge of chaos”.*

Just how is this more useful than a recognition of avulsions, stream capture, meander cut-offs, changes in braided river patterns, landslides and debris flows etc

P21. Despite advances in geomorphology, pedology and ecology in unraveling the pathways and mechanisms that have determined those facets of the landscape, in conventional reductionist approaches the hydrology of a catchment is still largely understood as a system without a history, as brute fact.

See earlier comments. This is simply not true of any hydrologist that I know.

P22 a catchment classification system based on a shared developmental pathway “genotypes”) may be more fruitful than one based on similar current hydrologic behaviour alone “phenotypes”) (see Sect. 4.2.3).

But surely one of the implications of complex dynamic systems is just that we cannot properly know the history of development (e.g. Culling, Phillips in geomorphology), so how would you then classify genotypes in some non-trivial way? And genotype are not mentioned in Section 4.2.3.

P22 Darwinian hydrology could similarly search for ways to unify the variability of catchments' hydro-logic behavior, but will have to search for its own mechanisms, since clearly “natural selection” does not apply.

But natural selection surely does apply in your framework. The impact of extended droughts on vegetation patterns for example. The modification of ecosystems by anthropogenic impacts in both short and long terms could also be viewed as a form of natural selection.

P23 It should be noted, however, that even if such optimal states exist, the Darwinian approach admits that the contingencies of history of a system can strongly constrain its degrees of freedom to evolve, creating lasting sub-optimal forms that dominate current structure

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But does this not apply EVERYWHERE? What we see now is only partly self-organised – it is often much more shaped by external boundary conditions (the last glaciation, anthropogenic deforestation over millennia, impacts of large scale irrigation etc). If nearly everywhere is sub-optimal and optimality can only be a tendency to be disrupted everytime there is an external forcing, how does it help?

P24 For example, catchments satisfying the Budyko curve (Budyko, 1974) manifest the similarity of long-term hydrologic functions (partitioning of precipitation into rainfall, 25 runoff and evapotranspiration) under stationary climatic controls; while the deviation from the Budyko curve likely manifests the remaining degrees of freedom such as vegetation and landscape variations (Troch et al., 2013).

But Budyko was derived from data – with errors – and for long term averages that reflected a particular period of climate variability. Such a statement does more to undermine your case than support it.

P25 Searching for hydrologic similarities and their organizing principles can help to estimate the future of existing catchments under changed boundary conditions by trading space for time (Singh et al., 2011). So far, little thorough investigation has been done 25 on this topic and its use in hydrology, which means that the validity of its basic assumptions and its broad value still have to be assessed. For example, under what conditions is spatial variability a proxy for temporal variability?

Actually never - except perhaps in some vague sense of helping to constrain some prior distribution of parameters or expectations. But do we not do this already when trying to estimate the impact of changes in a catchment on the basis of regionalized information from catchments with different catchments elsewhere. How does the Darwinian approach help here when the changes are primarily anthropogenic?

P27. Close to TE (here expressed by a small hydraulic gradient), diffusive water flow is linearly dependent on the hydraulic gradient. Beyond a critical hydraulic gradient, subsurface backward erosion 25 can lead to the formation of (dissipative) preferential

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flow structures which accelerate the flow and add an entirely new quality to it.

But this is a very special case. What about all the much more common preferential flow structures that are induced by dessication cracking, or root channels, or worm channels or relic glacial/periglacial features that are NOT a product of the flow process?

P28. For prediction of catchments under change, an explicit representation of such feedbacks is vital, as for systems far from TE, it is the balance of positive and negative feedbacks that keeps them in or pushes them out of stable quasi-steady states. Further, expressing dynamics via a concept of cascading energy conversions along hierarchical thermodynamic gradients (see Fig. 2) leads to a natural hierarchy of processes which is useful, not only to establish hierarchical modeling concepts, but also to allow formulation of upper thermodynamic limits to the magnitude of each conversion process (see e.g. Kleidon and Renner, 2013). It is, however, important to understand that in this hierarchical view of earth system processes, the boundary conditions of each conversion process are not fixed and there may be strong interactions between dynamics and boundary conditions. For hydrologic fluxes in catchments this is e.g. reflected through evaporative fluxes, which (jointly with the sensible heat flux) deplete the vertical heating gradient between the heated surface and the cooled atmosphere (Kleidon and Renner, 2013).

Please read this again - it is not very meaningful. Just where is the value added here compared to current understanding of these systems and their closures? and what is the practical import of such an insight?

P32. In this more general case, it is important to recognize that information can also be wrong (i.e., bad) in the sense that it can result in an increase in uncertainty about the true outcome.

You should properly distinguish between two types of disinformation here. There is data that appear to be self-consistent but leads to an increase in uncertainty (perhaps because of past overconditioning to errors) and there are data that may be inconsistent (e.g. runoff coefficients > 1 in the catchment case) and that should not be used in the inference at all.

P32. that facilitates a robust evaluation and improvement of model performance (Gong et al., 2013),

Not really – Gong et al. compare a one step ahead information theory prediction with a simulation model. It is no wonder that there is an improvement in performance that has absolutely nothing to do with the application of informational principles.

P32. L25 Clarke should be Clark (but what about the later critical discussion of this paper?)

P33 Alternatively, the joint entropy can be approximated by the conditional entropy of Qt, given Qt−L. . .Qt−1. This last quantity involves a model for predicting Q from its previous L time steps, with the model structure assumed to be known (e.g. Gong et al., 2013).

See comment above. And entropy deals only with distributions. It is throwing away information about the structured dynamic response (except in so far as it can be crudely represented by the (stationary??) conditional entropy approach mentioned).

P33. Finally, in the hypothetical case that meteorological conditions and all hydrological processes are already perfectly known, the information content of the discharge series becomes zero, because the perfectly predictable values no longer contain any surprise.

But this has no relevance at all to the real world (and the Gong et al approach referred to earlier only works because it makes use of Qt-1 etc.!!!)

P36 While the approaches we presented to deal with catchments under change are not new, and are in fact based on existing and well- established theories, we suggest that their synergistic combination can serve as a paradigm to direct the further development of catchment hydrology to address questions of change.

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I do not disagree with the authors here – but I am interested in how it might work. What is needed is an example or couple of examples. Let us take the continuing deforestation of the Amazon basin for example – or perhaps you could go back to a post-hoc analysis of the Aral Sea. We know that such large scale changes to a catchment system can have feedback effects to recharge, evapotranspiration and runoff and consequently on precipitation etc over large areas (some of which were not subject to the original change. The drying of the Aral Sea is itself a consequence of irrigation practice that could have been predicted without invoking any of the principles advocated in this paper. Past (theoretical) work on feedbacks in Amazon rainfalls by Rodgriguez-Iturbe and others, however, also suggests that there might be potentially chaotic responses to change – which could have feedbacks to catchments that have not been directly affected by the change (the impacts are seen only as a change in boundary conditions). In concept we are already aware of these potential sensitivities. How would the principles expressed in this paper lead to a better analysis of such problems? This is not really clear but, for example, the effects of drying of the Aral Sea on rainfall might also have been predictable, but the sensitivities might have been more evident by invoking the concepts of this paper. Doers it amount to more than that? The key would appear to be in the constraints of Figure 3 but it is rather difficult to see how they might be applied in such a case study. I think that the addition of such an example of how it might work in practice would be extremely valuable in making the message much more than a fashion statement. Hence my suggestion of a revision.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 8581, 2013.