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INVITED COMMENTARY

Scaling in hydrology

Günter Blöschl

Institut für Hydraulik, Gewässerkunde und Wasserwirtschaft, Technische Universität Wien.

Correspondence to:
G. Blöschl, Institut für Hydraulik,
Gewässerkunde und
Wasserwirtschaft, Technische
Universität Wien, Karrsplatz
13/223, A-1040 Wien, Austria.
E-mail:
bloeschl@hydro.tuwien.ac.at

The term ‘scaling’, to many, is veiled in a nimbus of exciting mystery. At a basic level, part of the mystery simply comes from confusion of two connotations of the word—meaning either scale invariance (i.e. processes behaving similarly at small and large scales) or upscaling/downscaling (i.e. aggregating/disaggregating data). However, once this hurdle is surmounted, from whence does this excitement come and in what direction does it lead? If we follow the scale invariance track of enquiry, some hidden signature of hydrologic systems that can be encapsulated in beautifully simple equations is promised. The idea of self-similarity, first conceived by L. F. Richardson and expertly marketed by B. Mandelbrot, is compelling given so much visual evidence of variability at small *and* large scales. And, indeed, if you believe there exists a single universal relationship underlying hydrologic processes at many scales it is hard not to fly off to cloud-cuckoo land with this idea. The upscaling/downscaling track of enquiry is more practical. In hydrology, much of the recent interest began in the 1970s with the early work of A. Freeze and L. Gelhar aggregating the groundwater flow equation based on a stochastic approach, and gained additional momentum in the 1980s when it was realized that spatial heterogeneity of the land surface matters for atmospheric models. Those subdisciplines of hydrology in which the basic equations are known with some degree of confidence (e.g. groundwater flow) have a head start, but for catchment hydrology and hillslope hydrology there is still a long way to go before the derivation of an aggregate large-scale equation from first principles will be possible. It is likely that *ad hoc* relationships with little theoretical justification will be with us for another few years.

Field hydrologists may wonder what role field observations and on-site experience have in all this, and I wonder too. Is it coincidence that most of the celebrated (and rightly so) pioneers of the scaling community never were personally involved in fieldwork, or is there a causal relationship? I believe it is the latter. Fieldwork and scaling theory, apparently, are too widely divergent for a single individual to excel in both. Or perhaps it is something else. I continue to be intrigued by the complexity of hydrological processes when in the field. The rich diversity in the spatial arrangements of flow paths and mechanisms that, to the observer, quite obviously change with scale make it difficult for me, when back in the office, to write down simple formulations that *neglect* most of what I know is out there. Many of the better-known scaling relationships do neglect the important bits. For example, stochastically averaged groundwater flow equations usually assume that

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hydraulic conductivities vary randomly, whereas we know that, more often than not, preferential flowpaths control the response. The alternative is to engage in ever more complex model building. But the price to pay is that these so-called physically based models are essentially 'unverifiable' at the catchment scale, and if they do work we know they most likely do so for the wrong reasons. Maybe, instead of trying to capture everything when upscaling we should be developing methods to identify dominant processes that control hydrological response in different environments and at different scales, and then develop models to focus on these dominant processes, a notion we might call the 'Dominant Processes Concept' (DPC). This may help with the generalization problems that have haunted hydrologists since the science began.

To be sure, we do need stochastic methods. Even very simple analyses such as the statistics of linear averaging can give important clues on, for example, how one may expect runoff coefficients to change with model element size, and more sophisticated analyses are on the verge of becoming useful for practical predictions. But all of these developments must firmly rest on field data and field experience. Remote sensing has a role here too, at larger scales, but there is a lot of value in 'down to earth' measurements, perhaps more than is generally acknowledged by the scaling community. Novel measurement techniques that allow rapid measurement of spatial patterns, such as sled-mounted FM-CW radar for mapping snow depths or all-terrain-vehicle-mounted TDR probes for mapping soil moisture, should be more widely used in developing scaling relationships, as should traditional data from the networks of the various hydrographic services.

What, then, are the exciting opportunities for scaling research in the years to come? I believe there is very important conceptualization work to be done that is likely to bring us much further. Perhaps most important is the notion of scale that, so far, we have not fully exploited in hydrology. Sister disciplines, such as fluid dynamics, have a strong track record of using characteristic lengths and times for parameterizing the order of magnitude of a phenomenon, but in hydrology there have been but a few attempts. And because in hydrology, often, the space–time arrangement of the sampling is such a key limitation of our process understanding, the characteristic scales of the sampling will also have to be considered and, in a similar vein, the scales at which the predictions are needed (what one might call the model scale). It is worthwhile to make a concerted effort to centre conceptual work on characteristic lengths and times (and maybe fluxes) of hydrologic processes, of samples, and models. Although (and maybe because) these characteristic scales only capture the order of magnitude, they are likely to provide a lot of 'big picture' insight into how hydrologic processes and our representations thereof change with scale.

Another potential stepping stone related to scale is the concept of similarity. Similarity concepts, generally, can be used profitably when the physics of the processes in question are not fully understood and, alas, this is the case in hydrology. Here, the challenge will be to reconcile statistical descriptions and observation-based process interpretations. One example is rainfall variability, which is statistically self-similar over many scales, although we know this to be the result of quite different processes operating at each scale. So, what is it that makes a hillslope and a catchment hydrologically similar and what makes two catchments similar? Addressing these questions will also provide an important opportunity for the two tracks of scale research mentioned above (i.e. scale invariance and aggregation) to feed into each other to the benefit of both, since the notion of similarity is central to both. The DPC mentioned above can be thought of as a similarity approach.

The data problem, when linking process descriptions across scale, is clearly with us to stay, but I think we can make much better use of qualitative observations or proxy data. This is already being done in an informal way in, say, building models of hillslope response based on a scientist's qualitative field experience, but I believe there are merits of formalizing the use of this type of information. Qualitative observations of the presence/absence of saturation areas and surface flow in a catchment may greatly assist in upscaling local-scale process representations, and formal methods are needed to exploit this information fully. Whenever possible, these methods should make use of observed *spatial patterns,* be it proxy data or hard data, as patterns can give us a much better handle on the space–time dynamics of flow systems than can point data.

It has been said, somewhat pretentiously, that scaling issues are at the heart of most, if not all, hydrologic problems. It is probably true that a better understanding of hydrologic space–time variability—be it through scale invariance or aggregation analyses, or, as suggested above, through a closer examination of characteristic scales, similarity concepts and proxy data—will help with many hydrologic problems. On second thought, perhaps there is an even more important role for scaling research within the hydrological sciences. Some observers have complained that the recent literature reflects a certain tendency toward fragmentation of the subdisciplines with papers 'digging the same hole deeper' prevailing over comprehensive studies. Maybe what we need is a change of paradigm. I believe strongly that breakthroughs are more likely to arise when scientists transcend their disciplinary isolation and collaborate in the unexplored territories between specialities. Given that scale issues are common to all of the hydrologic subdisciplines (and indeed far beyond them), scaling work may perhaps reveal its greatest potential as an umbrella under which a rich spectrum of concepts, tools, and measurement techniques covering a range of areas can be unified. Ideally, scaling work should materialize as a unifying theory of hydrology—a theory so urgently needed—for which I believe the scaling ideas must be the cornerstone. However, even at more modest levels there are still so many synergies to profit from and so many interactions that are the 'important bits'; scale concepts can also provide a *common framework* for them because the change of time or space scale is often the critical issue. Many years ago, Horton (1931, p. 201), in a lecture on the future of hydrology, stated 'the most immediate needs for the advance of the [hydrological] science are ... research to provide connective tissue between related problems.' Today, this connective tissue is more important than ever, as eloquently elaborated by Burges (1998, p. 131). Although a clean-cut resolution of what this connective tissue is, exactly, has yet to emerge, it is clear that scale concepts will be the catalyst.

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