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Barriers to progress in distributed hydrological modelling

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Why model? We model because we cannot measure everything everywhere; because we want to plan ahead for how catchment responses might change in the future; because we want to infer what might happen under more extreme conditions; and because, in the words of Max Kohler, 'we want to show that we understand our science and its complicated interacting phenomena'. Indeed, it has been suggested that the name of our species, 'Homo sapiens', could be replaced by another, perhaps even more appropriate, 'Homo simulatis' (Vinogradov and Vinogradova, 2010), because, in practice, we all model continuously without even thinking about it. Modelling is our usual, normal and almost unconscious state. We perceive the world around as a system of images, idealized representations and mental models. This also applies to people, events and natural phenomena. And everyone's models and images are quite individual, incomplete and sometimes erroneous. It can be argued that we live in a virtual world (e.g. Baudrillard, 1981).

In science, mathematical modelling gives us the opportunity to test the reliability of our comprehension of the nature of the processes and phenomena. Modelling in this sense aims to generalize, put in order and extract all relevant information available to the current theoretical and experimental science. In theory, at least, we understand that our virtual worlds have their limitations. Some compromise is necessary as a result of how well we understand and can represent the complexity of the real world. Compromise, of course, allows for many different solutions, but it does seem that in distributed hydrological modelling, there has not been a great deal of thought about an appropriate compromise. Rather, the recent modelling strategy has been technologically driven with the aim of converting distributed data of different types into useful information for decision-making in planning, development and management of hydraulic structures and water resources systems at the scales that current computing power will allow, whilst looking towards the 'hyperresolution' scales of the future (Wood *et al*., 2011). As a result of the practical demands of water resource management, older distributed models that do not adequately reflect our understanding of flow processes and connectivity on hillslopes still continue to be used. Our curiosity for the study of nature appears to have been dominated by a fascination with computational technology and implementation.

But to make progress in distributed modelling, there are significant barriers that need to be overcome. We suggest that insufficient thought is being given as to how to overcome them and that for real progress to be made, those barriers need to be addressed more explicitly. After all, the physics of runoff formation anywhere should be the same even if the conditions under which runoff is generated are extremely diverse. Geology, relief, air temperature and precipitation are the main factors determining the existence of natural runoff zones, biomes, unit source areas or hydrological response units. Various particular processes manifest them-

selves in different ways depending on the season and conditions of excess or deficit of catchment storage, with some processes more or less important in different conditions. However, this should not preclude a general mathematical formulation that is both consistent with current understanding and applicable everywhere.

This, of course, was the original idea behind the development of 'physically based' distributed modelling, as set out by Freeze and Harlan (1969) and the SHE model of Abbott *et al.* (1986). Models that are essentially based on this blueprint continue to this day in HYDRUS3D, Hydrogeosphere, InHM, PIHM, TOUGH3D, PARFLOW, CATHY and other codes. These models are being applied to finer and finer element scales for larger and larger catchment areas or whole countries. Their physics has also been incorporated, in reduced dimensional form, into a variety of land surface parameterizations used in global earth system science models. They could be the basis for the hyperresolution models of the future, except that they are based on quite the wrong physics (see Vinogradov, 1988; Vinogradov and Vinogradova, 2008, 2010; Beven, 1989, 2012; Beven and Germann, 2013).

The computational demands, parameter requirements and disappointments in using such models in real hydrological applications have led to the development of a plethora of simpler modelling frameworks, model structures and process representations. In nearly all cases, such models depend on model calibration against observed discharges (and sometimes other forms of observations) as a means of declaring success. But, as has been discussed elsewhere, model calibration is not an entirely satisfactory exercise as the basis for testing models as hypotheses about how catchments function (Klemeš, 1986; Morton, 1993; Oreskes *et al.*, 1994; Oreskes and Belitz, 2001). Accordingly, in the ungauged catchment case, we do not expect models to perform so well. We may already be seeing hydrological models of everywhere, but we do not have models that work everywhere (Beven and Alcock, 2012; Beven *et al.*, 2015). We argue here that a test of a mature hydrological science should be to define mathematical structures, and *a priori* parameterizations within those structures that are consistent with a wide range of hydrological conditions. The idea is not new; it was the idea that underlay the Freeze and Harlan blueprint, as well as the approach of unit source areas (Amerman, 1965) or hydrological response units (Flügel, 1995). So why has more progress not been made?

The main barrier to progress would seem to be using the wrong physics. Of course, any process representation necessarily means some simplified idealization of some natural phenomena and processes but, as it has been argued before, the scheme of slope runoff transformation presented in physically based models by a continuous water layer on smooth flat slopes, or the equilibrium gradient assumptions of the Darcy–Richards equations, should not be considered adequate. Such descriptions have become so ingrained into hydrological models that the attempts to draw attention to their inadequacy do not seem to have had much effect on the modelling community (Beven and Germann, 2013; Vinogradov and Vinogradova, 2008, 2010; Beven, 1989, 2006b; Vinogradov, 1988). Current hydrological community initiatives are still essentially based on these concepts, but it can be argued that the equilibrium experiments of L. A. Richards in 1931 on which such models are based have been a major barrier to real progress in hydrological modelling. In addition, little or no attention has been paid to modelling both the hydrograph response and the travel time distributions of water in catchments (McDonnell and Beven, 2014).

It just seems strange that there does not seem to be more dissatisfaction with the current modelling concepts. Is this because there is always a good justification for model calibration, allowing some demonstration of success in matching the available data? As Vit Klemeš (1986) pointed out many years ago, this is not a very strong test of a model and provides absolutely no guarantee of successful predictions at other (ungauged) sites or under changed or future conditions at the same site. We need stronger methods for testing models whilst allowing for the uncertainties of hydrological data (Refsgaard, 1997; Beven, 2010, 2012; see also the discussion of Clark *et al.*, 2011 and Beven *et al.*, 2012).

These problems and the issues of inadequate data sets for rigorous model testing across a wide range of hydrological conditions, the lack of any framework for identifying model and data deficiencies (rather than compensating by calibration and uncertainty estimation), the failure to translate process studies for both flow and travel times in catchment systems into improved model structures and the lack of any adequate theory for scale dependent process parameterizations in heterogeneous domains have been discussed as part of a Royal Society and Russian Fund for Basic Research international exchange project between Lancaster University, UK and the State Hydrological Institute, Russia, with the participation of researchers from several other institutions. Interestingly, whilst we mostly agree on the barriers to progress, after several workshops, there has been no real accord about how to overcome them. We record the debate briefly here, because the discussion is of much wider interest.

The St. Petersburg way to progress is to aim to build a model of everywhere, with local conditions classified in terms of different runoff formation complexes on the basis that similar hydrological conditions should generate similar surface and subsurface hydrological response (Semenova *et al*., 2013a, Vinogradov *et al*., 2011). The model parameters are determined at the scale of an elementary catchment in any distinct geographical zone determined by landscape and climate conditions. It should then only be necessary to determine parameters for a runoff formation complex at 'representative sites', for those parameters to be applicable elsewhere in similar geographical conditions (e.g. Lebedeva *et al*., 2014; Semenova and Vinogradova, 2009; Semenova, 2010). The core of the St. Petersburg Hydrograph model is the concept of runoff elements, which is briefly described by Vinogradov *et al*. (2011) and in detail by Vinogradov (1988). It is a simple natural process description and alternative to 'physically based' conceptual representation of slope flow transformation that is applicable from the scale of the elementary catchment to basins of any size. Complex sequential processes of water accumulation in surface and subsurface runoff elements and its outflow through rills and microstreams into the channel network are characterized by the typical volume and outflow times expected for different processes. Runoff redistribution in time is determined by regulating impact of runoff elements and is described by conceptual parameters that are assessed at the elementary catchment scale and linked with the properties of landscapes. The longer residence times and greater volumes of deeper groundwater bodies are added with the increase of basin size as necessitated by the basin geological structures. The parameters describing functioning of runoff elements of surface and different subsurface levels have dimensions per unit area and so can be upscaled for areas of similar characteristics. Because at least 25% of land on Earth is covered with permafrost and much more territory is the subject to seasonal soil freeze/thaw processes, an important part of the model is the energy balance that controls the heat dynamics in the soil (Semenova *et al*., 2014) and snowpack development.

Testing of the credibility of the model predictions in this framework is by expert opinion, taking proper account of the limitations of the available data, in a way that assesses whether the right results are being obtained for the right reasons (Kirchner, 2006). It is a matter of expert judgement as to whether performance of a model across a wide range of catchment conditions can be considered adequate, whether the data need to be questioned, whether the algorithm should be improved or modified, or whether new runoff complexes need to be introduced to allow for new sets of conditions. Automatic model calibration should not be used as a means to justify a poorly formulated model, and the danger to camouflage it by uncertainty estimation should be recognized. Instead, we should demand that our models be applicable over a wide range of hydrological conditions and different scales. Model evaluation should make use of all sources of data, including novel types of information and historical observations, which are often still not analysed, systematized and even digitized (e.g. Semenova *et al*., 2013b).

The Lancaster way to progress also envisages a model of everywhere but with a somewhat different intention. The expectation is that once a model of everywhere is available and its results are made available to the wider hydrological community, then it will be critically scrutinized and places identified where the predictions (and the concepts or data on which they are based) are wrong, and some improvement needs to be made (Beven, 2007; Beven and Alcock, 2012). Uncertainty, estimation is important in this view, because it will reflect the limitations of representing local responses (Beven, 2000; 2006a; 2012). Where inadequate predictions are identified, however, then the response might, of course, simply be a local recalibration and constraint of uncertainty making use of the new information. It might, however, result in reformulation of the model or recognition of data inadequacies and the need for new observation programmes.

These concepts are rather different, but there are some elements that are common to both approaches. We both see modelling as a learning process: the first by the expert researchers who learn about general principles from local responses by testing their model over a wider and wider range of hydrological conditions and the second in learning about local responses from local knowledge. We both suggest that more rigorous model testing is necessary, and that in doing so, the data available for model testing should be critically examined. Whilst objective tests might be possible in some circumstances (e.g. Beven and Smith, 2014), very often it will only be possible to make subjective judgments about the quality of data. We both agree that better process representations are required (e.g. Beven, 2006b; Vinogradov and Vinogradova, 2010; Beven and Germann, 2013; McDonnell and Beven, 2014).

We understand that breaking out of the barriers of the normal modelling paradigm is difficult and that providing more realistic modelling approaches is a non-trivial task. First of all, success will depend on our ability to be self-critical and to broaden the focus of routine research to a more general view of hydrological phenomena, rather than a concentration on specific catchment responses. The availability of good quality data sets, with minimal uncertainty, would allow examination of fitness-for-purpose model across a wide range of conditions (e.g. Kuczera *et al*., 2010). Ideally, such data sets should provide information about changes in storage and variations in travel time distributions as a catchment wets and dries so that models can be tested against flow, internal state and transport data in catchments with different properties (e.g. Tetzlaff *et al*., 2014).

There will still be the barrier of formulating new model concepts. What should they look like? Well, we know that they should take into consideration the differences between velocities and celerities. We know that they should be hysteric with respect to total storage in a landscape element (however defined). We know they will need to reflect the heterogeneities, and particularly the extremes of the distribution of heterogeneities that might be important in rapid runoff generation under wet conditions or retention under dry conditions. We know they should not be based on some averaged gradients between elements because of the nonlinearity of the process controls such that fluxes across the boundary of any heterogeneous element may average linearly, but gradient terms in general will not. We would wish that these features can be incorporated into a model formulation in a way that allows for parameters to be constrained with low uncertainty in similar hydrological circumstances. These are requirements for any distributed modelling scheme in hydrology that is going to be intellectually satisfying in reproducing both flow and travel times of water. That is the challenge that must be met to make a breakthrough in distributed hydrological modelling.

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