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Key Points:
- Reviews benchmark WRR on runoff generation
- Discusses the current lack of field work in hydrology
- Review is context for a vision for the future

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Whither field hydrology? The need for discovery science and outrageous hydrological hypotheses

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Abstract Field hydrology is on the decline. Meanwhile, the need for new field-derived insight into the age, origin and pathway of water in the headwaters, where most runoff is generated, is more needed than ever. *Water Resources Research* (WRR) has included some of the most influential papers in field-based runoff process understanding, particularly in the formative years when the knowledge base was developing rapidly. Here we take advantage of this 50th anniversary of the journal to highlight a few of these important field-based papers and show how field scientists have posed strong and sometimes outrageous hypotheses—approaches so needed in an era of largely model-only research. We chronicle the decline in field work and note that it is not only the quantity of field work that is diminishing but its character is changing too: from discovery science to data collection for model parameterization. While the latter is a necessary activity, the loss of the former is a major concern if we are to advance the science of watershed hydrology. We outline a vision for field research to seek new fundamental understanding, new mechanistic explanations of how watershed systems work, particularly outside the regions of traditional focus.

1. Introduction

"As computing power becomes cheaper and field work more expensive, there is a trend away from field work and towards more complete dependence on simulation" [*Kirkby*, 2004, p. 16]

Watershed modelers are being asked to answer more and more complex questions as computing power continues to expand and problems of water quantity and quality continue to escalate in a warming world. However, predictions across spatial and temporal scales are extending beyond our fundamental understanding of hydrological processes. This is happening at a time when field studies designed to further process understanding are in decline. Looking back on 50 years of Water Resources Research (WRR), it is clear that the fraction of field activities in watershed hydrology is much less than what it was when WRR started (Figure 1). In 1965, the first year of WRR, nearly half the papers contained an element of field studies with almost a quarter delving into fundamental processes of streamflow generation and questions of where water goes when it rains, its flow path to the stream and its residence time. Now field-based papers are 10% at best of what is published and this continues to decline (Figure 1b). While the scope of WRR and hydrological inquiry has expanded—and that is a good thing—there is no doubt that, as computing power has become less expensive and field work more expensive (and risky, compared to model approaches), there has been a movement away from field work and towards an almost complete dependence on modeling [*Kirkby*, 2004]. This is a problem on many levels. Our models are only as good as our understanding of how systems work—understanding fully beholden to discovery science. And, there are still so many fundamentals that we do not understand with regards to how water cycles in our catchments and gets to our streams [*Barthold and Woods*, 2015], how vegetation interacts with these runoff processes [*McDonnell*, 2014], and how catchment storage affects surface water responses to land use change and biogeochemical cycling [*Birkel et al*., 2015].

Some may argue that there are much bigger fish to fry in hydrological research, like the integration of socio-economic and stakeholder factors into integrated models of water resource systems [*Sivapalan et al*.,

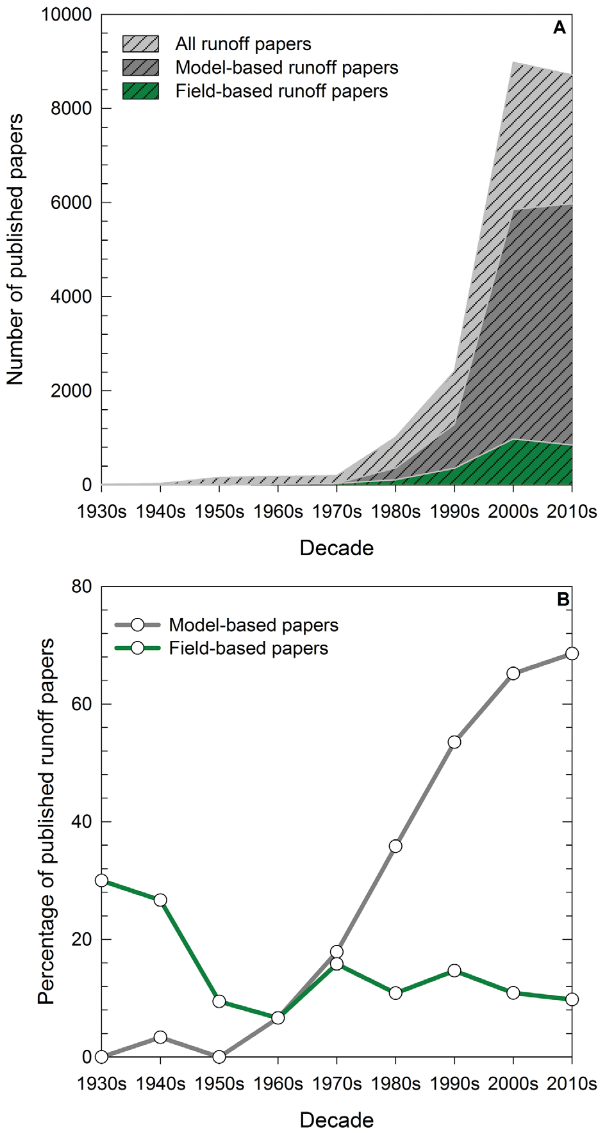

Figure 1. The decline of field studies in the streamflow generation literature, 1930-present. Data source: Web of Science.

2012; Lall, 2014]. Some may say that there are now remote sensing tools and other approaches for best examining large-scale system relations [*Famiglietti et al.*, 2011]. While others may say that enough field work has been done and that water delivery processes in the headwaters have been 'worked out' (!); that the task now for hydrology is to work out the implications of process studies at larger scales [*Bloschl*, 2013]. While we certainly support hydrological research pluralism, and continued and new work on many different fronts, it is a sad fact that when we bring our very best models up against field reality, calibration is still our main remedy [*Sivapalan et al.*, 2003]. Even physics-based simulation involves calibration where tuning van Genuchten parameters is a way of fitting a square parameter peg into a round process hole (because of the fact that flow in soils is highly preferential and that our models are highly nonpreferential). While again progress is being made here and there [e.g., *Laine-Kaulio et al.*, 2014], making models work for the right reasons is wholly dependent on our ability to characterize and understand mechanistically such processes in the field. *Kirchner* [2006] noted the consequence of this situation: "the key question is not whether models of hydrologic systems should be physically based; instead, the question is *how* [our italics] they should be based on physics." We have completed a hydrological decade focused on a movement away from calibration towards prediction in ungauged basins [*Hrachowicz et al.*, 2013], but have we really changed the culture? Have we stemmed the tide of declining exploration of field processes? Can we really advance watershed science when field studies are now so rare? Can we advance new physical understanding of catchment systems without field discovery?

We are not the first to highlight the decline of field hydrology. Many have noted the disappearance of stream gauging stations around the world [e.g., *Shiklomanov et al.*, 2002] and the major decline in field studies that seek to measure new things, and to uncover new understanding about how catchment systems work [*Wagener et al.*, 2010]. Others have made calls along the way for integration of field studies into modeling [*Dunne*, 1983] and improved dialog between experimentalist and modeler [Seibert *and McDonnell*, 2002]. The last "anniversary issue" of WRR in 1986 included several papers making effectively similar points and calling for a renewed search for hydrologic laws [*Dooge*, 1986] and an end to dilettantism in hydrology [Klemeš, 1986]—largely a result of the movement away from core discovery. With notable exceptions (largely funded and motivated via the new Critical Zone Observatory (CZO) network in the USA, and the Terrestrial Environmental Observatories (TERENO) network in Germany plus rogue groups of field hydrologists at some universities), field-based inquiry of the age, origin and pathway of runoff has been largely supplanted by modeling and model analysis as Figure 1 attests. Here we argue that, if we are to ever address fully the Dooge and Klemeš calls, the amount and nature of field studies in hydrology must change.

2. Field Work: A Dying Art?

One might say that we are at a point now in our science that is analogous to where Geology was in the early Twentieth Century when *Davis* [1926] saw a decline in lateral thinking and debate in field geology. *Davis* made the case then for geologists to go into the field and hypothesize outrageously. We would say that for hydrology today, we too need bold new field experimentation (and outrageous hypotheses) to challenge existing ideas and process complacency, and to confront old theories with new data. There are many examples where repeated measurement in the same catchment—by different groups or using different techniques—has overturned ideas. We need more of this. To go out and hypothesize outrageously, we have nothing to lose but our paradigms (to paraphrase *Baker and Twidale* [1991]! And paradigms we must challenge and risk losing, or else we will still be lamenting Dooge and Klemeš in the WRR 100th anniversary issue!).

While there is rather general agreement that we are theory-limited in catchment hydrology [*McDonnell et al*., 2007], it seems now as though our scientific imagination in hydrology is hampered by widely used and widely accepted watershed models. In the early days of WRR, we were not hampered by these *de facto* 'scientific principles' that are now encoded into our models. Our field work then 'roamed with little restraint', as *Davis* [1926] recalled in Geology. Ideas and paradigms were to be challenged with field data [*Kennedy*, 1983]. Now field data collection is motivated mostly by model parameterization (of, in many cases, bad models).

What outrageous hypotheses do we have today? What fractious debate do we have in the discipline? We have legions of PhD students around the world applying the same old modelin yet another catchment. While there may be laudable goals and intentions of such studies, what new understanding is gained from this activity? We have had 50 years of the Curve Number—something known to "not work" [*Hawkins*, 2015]—and yet we continue to use it in practice uncritically, with little field-based inquiry stepping up and saying "wait just a minute—the world does not operate as a Curve Number suggests." We go further and further down the rabbit hole of model uncertainty estimation but we rarely stop to ask ourselves about our model structures and process data, and the process inference upon which they are based. We rarely question the paradox that, while all flow is preferential [*Uhlenbrook*, 2006], few if any models incorporate such effects? And none address *why* all flow is preferential. We acknowledge the ubiquity of flow networks [*Band et al*., 2014] but we are largely unable to predict *a priori* what networks are like and what these network effects will have on the permeability architecture of the porous media, be it soil, saprolite or bedrock.

It is not only research that needs a field hydrology wake-up call. With increasing costs, a field component of graduate education is also on the extreme decline [*Wagener et al*., 2012]. With climate change and nonstationarity to deal with, process-based inquiry and field-based graduate training is needed now more than ever to show our largely model-focused graduates the limits of prediction and predictability. *Famiglietti* [2012] summed up the situation best in his testimony to the US Congress: "How can we manage water for the benefit of mankind, in nonstationary times when we know so little about its various stores, flow pathways and residence times, even in a developed country like the United States?"

Note that we are not old field guys lamenting the days of yesteryear (although, admittedly, we are old and we do lament). We finished our PhDs in the last millennium at a time when universities were shifting to an expectation for publishing that was (and still is mostly), a few papers per year for baseline performance. So metrics are not driving this trend. Field equipment and data loggers are now infinitely easier to use and less expensive. In the 1980s, one read a British Geomorphological Research Group (BGRG) *Technical Bulletin* [e.g., *Burt*, 1978] to learn about how to make a sensor for field data collection. Campbell Scientific Inc. loggers were just appearing on the scene. Now there is a seemingly endless array of field loggers with many sensors complete with self-contained loggers and high-frequency water quality sampling opportunities. So price and availability of equipment are not driving this trend. So what is? Most likely, it is the apparent ease of computer simulation models and the seduction of prolific model output.

Admittedly, there risks associated with field work—many times things do not work out: weather does not cooperate; instruments fail. Field studies can be difficult to publish in international journals if they are seen as case studies. For the watershed hydrologist starting out, all this impacts her or his decision-making process on where and what to invest their time in. We argue though that such risks are worth it, given that lack of field experience misleads one into thinking that the model actually captures process realism. Standing in a catchment during a rainstorm, observing the nature of infiltration, streamflow response, the subtleties of rainfall intensity variation (or melt intensity) and its impact on patterns of soil moisture, can all instill (even if no new discovery of process is made) an appreciation of the extreme heterogeneity of the system we are seeking to understand. Or, at the very least, that runoff is caused by rainfall or snowmelt and not an excess of random numbers!

3. Fieldwork History in WRR

WRR has included some of the most influential field-based papers in the area of streamflow generation, particularly in catchment hydrology's formative years during the First International Hydrological Decade; a time when the knowledge base was developing rapidly. Whilst WRR has sometimes been thought of as a journal dominated by mathematically and theoretically focused papers, a succession of editors has encouraged the publication of field-based, process research. It is interesting to note that Volume 1, Issue 1 included articles by *Sopper and Lull* [1965] and *Rothacher* [1965]—process-based forest hydrology papers using the paired watershed approach. This was a time when outrageous hypotheses were plentiful. Take Roger Betson's studies of storm runoff generation. Initially, he argued that infiltration-excess overland flow usually originated from a small, but relatively constant, part of the watershed (the "partial area"), where uniform conditions could be assumed [*Betson*, 1964]. The study was firmly rooted in the Hortonian paradigm with no indication that some of the storm runoff might have been from subsurface sources (despite citing John Hewlett's advice from the nearby Coweeta watershed that it is better to start with the assumption that all rain infiltrates unless there is evidence to the contrary) or from saturated soils where the infiltration capacity would effectively be zero.

The WRR paper that followed by *Betson and Marius* [1969] indicated a complete transformation in thinking and approach. They focused on the detection of surface soil saturation to define runoff production areas, using field investigation of shallow water tables to support their arguments. An array of piezometers was then used to explore saturation of the uppermost soil horizon across the watershed slopes. They showed the importance of an expanding saturated zone "riparian to the watershed outlet'.

If ever a pair of papers demonstrated a paradigm shift in thinking and approach, these two WRR papers by Roger Betson, just five years apart, do just that. In one sense, the partial area model of runoff generation was retained, but with a more complex set of possible runoff process mechanisms. These ideas were soon confirmed by *Dunne and Black* [1970a, 1970b] in two notable WRR papers.

Pinder and Jones [1969] sought to separate stormflow and baseflow in a new way, using a simple two-component mixing model (Table 2). Their conclusions were controversial because process hydrology was still dominated by the Hortonian paradigm; evidence of rapid contributions of subsurface stormflow and saturation-excess overland flow was only just beginning to appear (as reviewed by *Beven* [2006]). In this regard, *Martinec* [1975] presented what was an outrageous hypothesis: that rapid, diurnal stream discharge peaks observed during the snowmelt season could be composed of anything other than direct snowmelt inputs to the stream. *Martinec* [1975] noted that "By this concept, the quick reaction of outflow to a massive groundwater recharge is brought to agreement with the long residence time of the infiltrated water." As *Kirchner* [2003] put it somewhat more eloquently, "hillslopes store water for months to years but release it in minutes or hours to the stream." *Martinec* [1975] used tritium to trace subsurface flow and showed that infiltration of meltwater caused an immediate, corresponding increase of outflow from groundwater storage. Whilst the exact nature of the propagation mechanism was not known, he speculated that some sort of pressure wave was involved, accounting for the rapid outflow of long-residence time groundwater and the nonappearance of recently infiltrated water, in effect one of the earliest and clearest articulations (based on data) of the difference between celerity and velocity [*McDonnell and Beven*, 2014].

Paul Mosley's research on macropore flow at the Maimai watershed in New Zealand was bold and equally contentious because no one had previously imagined a connected series of preferential flow paths down a forested hillslope that might explain Horton-like flashy storm hydrographs with correspondingly high runoff ratios. *Mosley* [1979] showed that the saturated hydraulic conductivity of the soil matrix need not be a limiting factor on a rapid subsurface flow response—a radical challenge to theory [cf. *Freeze*, 1972]: flow of dye tracer through macropores was observed at rates up to three orders of magnitude greater than the soil matrix saturated hydraulic conductivity. Mosley's interpretation was later refuted by *Pearce et al.* [1986] and

Sklash et al. [1986] on the basis of natural stable isotope data where the isotope results suggested that the new water contribution to the storm hydrograph was less than 3% [*Pearce et al*., 1986]. *Sklash et al* [1986] ascribed the rapid and significant rise in both the stream and old water hydrographs to groundwater ridging in the lower slopes where the capillary fringe was at or near the ground surface. They envisioned that saturated wedges developed quickly on the lower slopes as infiltrating water converted the tension-saturated zone into phreatic water, steepening the hydraulic gradient towards the stream at the same time. There was no need to invoke either flow paths bringing water rapidly from upslope or pressure wave transmission downslope. Mosley was correct in the sense that macropores can and do produces enough subsurface flow to account for the storm hydrograph; he was just wrong in the age (old versus new) of the water in the macropores. Sklash and coworkers were right in that old water dominated the storm runoff, but the groundwater ridging hypothesis has proved less influential than they argued.

These studies evoke a time of sweeping shifts in thinking—even in the same research catchment. Where is that level of curiosity-based argument and debate today? We are timid in our process interpretations because it would appear that we are not making the kinds of measurements to overturn paradigms. Mention of the capillary fringe above reminds us of another rather outrageous assumption in process hydrology—*Abdul and Gillham*'s [1984] proposition that a capillary fringe could explain rapid groundwater ridging. *Ragan* [1968] was among the first to demonstrate that the rapid development of groundwater ridges adjacent to the stream in response to precipitation inputs could generate a rapid increase in groundwater discharge to the stream. *Sklash and Farvolden* [1979] embraced this as a potential mechanism to explain their findings of large preevent water presence in streams draining upland forested catchments in Southern Canada. The height of the capillary fringe above the water table is inversely related to pore size, and so is greater in fine-textured soil. Despite negative pore water pressures, the soil remains saturated (or very close to it) and only a very small amount of water is needed to raise the water table and create a downslope hydraulic gradient. *Abdul and Gillham* [1984] used a laboratory model to demonstrate the phenomenon, showing how the rapidly rising water table would steepen the local hydraulic gradient, driving "old" subsurface flow toward the channel. If the capillary-fringe or tension-saturated zone extends to the ground surface before the event, then only a small amount of rainfall is needed to go from negative to positive pore water pressures. Of course, work that followed rejected the capillary-fringe, groundwater ridging process, even for the Maimai catchment [*McDonnell*, 1990]. So a good argument for continued field studies related to streamflow generation is perhaps also to demonstrate where some of these outrageous hypotheses are supported, and to show where they cannot be accepted. The failure of the groundwater ridging hypothesis to explain runoff generation in all landscapes is a good case in point.

Of course, there have been striking examples of field-based inquiry and discovery since those early WRR benchmarks: identification of the importance of macropore flow [*Germann and Beven*, 1981, 1985]; spatial patterns of soil moisture, their relation to topography and the use of geostatistics to interrogate such data [*Western and Grayson*, 1998; *Western et al*., 1999]; from one of the best watershed studies of all time at Coos Bay, how landscapes store and release water and the role of (weathered) bedrock [*Montgomery et al*., 1997] and the understanding it provided in terms of flow path control on solute release [*Anderson et al*., 1997] and old water effusion [*Torres et al*., 1998]; the importance of hydrological connectivity between upslope area, riparian zone and stream channel [*Jensco et al*., 2009].

4. A Vision for the Future

What do these examples from the early years of WRR have in common? A unifying theme is that they all sought to elucidate yet-unexplained phenomena: what parts of the watershed were most important for the delivery of water to the channel; how flashy hydrographs could be explained when no overland flow was visible; how groundwater could get quickly into the stream to explain the presence of "old" water. They were about basic discovery. They fundamentally challenged the *status quo*. It did not matter if new ideas outraged our sense or our sensibilities. For *Davis* [1926], the crux was that "we must not allow our concepts of the earth, in so far as they transcend the reach of observation, to root themselves so deeply and so firmly in our minds that the process of uprooting them causes mental discomfort." We need to revert to the creation, testing and discarding of genuine hypotheses and to spend less time congratulating ourselves that some concept or other explains a significant proportion of the observed variation. Whilst we tend to feel

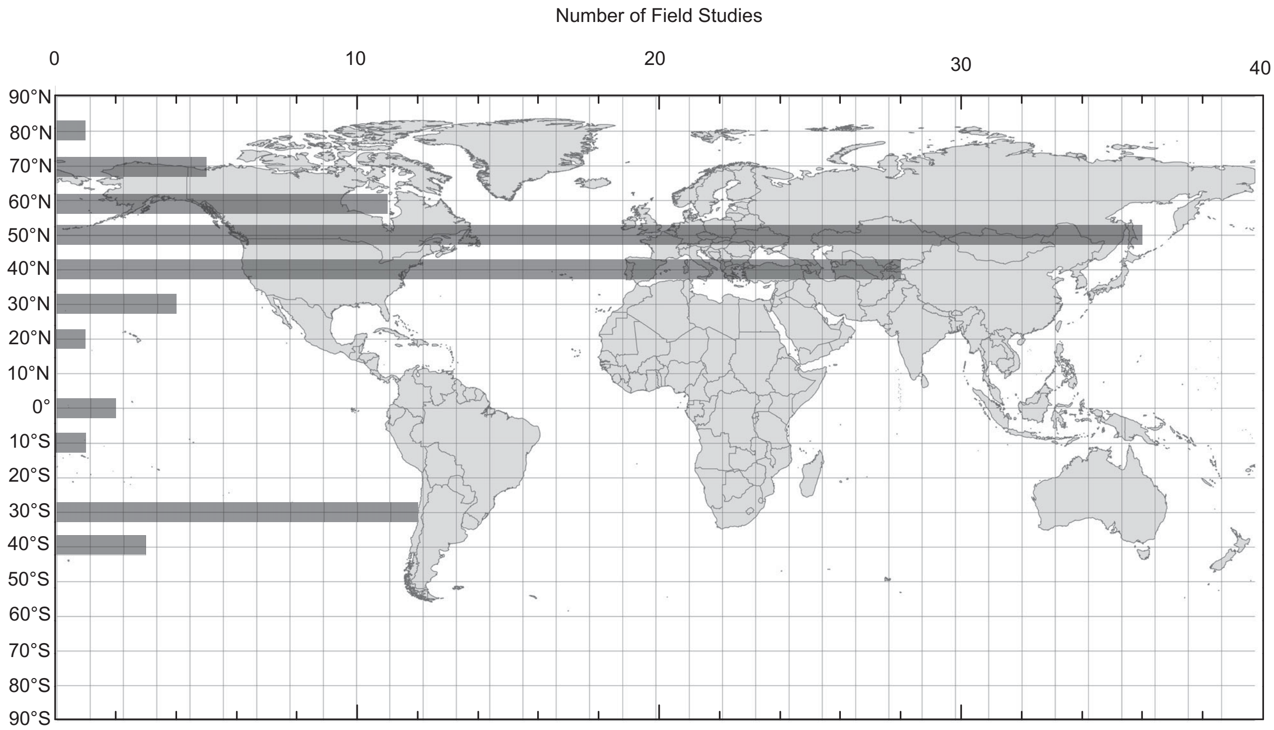

Figure 2. The extreme locational bias of published field studies in watershed hydrology published in WRR 1989–2015; 93 papers with 104 total field locations. Data source: Web of Science and study site sections extracted manually from each paper.

that most new hypotheses will most likely emerge inductively from curiosity in the field, we must acknowledge that models can be used as hypothesis generators too [e.g., *Burt and Butcher*, 1985, *Ali et al.*, 2013]. Whether we use models or field observation to generate hypotheses, deductive testing *must* involve field observation in our view; we might then, as *Kennedy* [1983] exhorts, be able to substitute imperfect answers with (more) clearly defined questions.

So what is the way forward? We need a renaissance of field hydrology. We need it at the individual level; we need it collectively. Whilst humid, upland, forested systems have received the bulk of our attention in the northern hemisphere midlatitudes (Figure 2), recent work, even there, has exposed huge holes in our understanding of subsurface mixing [*Brooks et al.*, 2010; *Goldsmith et al.*, 2011], the contribution of transpiration to evapotranspiration [*Jasechko et al.*, 2013; *Wang-Erlandsson et al.*, 2014; *Sutanto et al.*, 2014], and how similar catchment forms can mark radically different internal plumbing [*Hale and McDonnell*, 2015].

While we may be at the asymptote of the learning curve regarding the range of runoff mechanisms (and maybe all runoff processes are the same anyway [*McDonnell*, 2013], there is pressing need to understand such processes in rapidly changing environments (as Figure 2 attests). Cold environments are losing their cold and permafrost-dominated landscapes are changing fast. While pioneering research has been done in the 1990s in deglaciated landscapes [*Branfireun and Roulet*, 1998; *Peters et al.*, 1995], particularly in Canada, new behaviors outside of these cases largely linked to groundwater and wetland processes are in need of basic field study. The tropics are in a similarly precarious position with warming temperatures changing many montane cloud forests to simply rain-fed forest catchments. What are the consequences of these changes on groundwater recharge, runoff generation, low flows? We have large-scale analysis in the tropics showing rather different groundwater recharge processes linked to intense rainfall events [e.g., *Taylor et al.*, 2013] and some linked to more frequent smaller events [*Scholl and Murphy*, 2014]. The process basis of this is sorely lacking.

The built environment and managed landscapes in general remain a vast untapped wilderness of discovery [*Soulsby et al.*, 2015; *Wang and Pataki*, 2012]. Many other managed landscapes have pressing need for field-based inquiry. For instance the Loess Plateau in China is the world's largest eco-restoration project, and mechanistic insight is needed on how afforestation efforts will affect how these watersheds will store and release water through exceptionally deep unsaturated zones. Many other areas of the world are grappling with age-old questions of forest harvesting impacts on streamflow, that so dominated the early issues of WRR. In Chile, almost 2 million ha of forest are managed plantations of radiata pine and eucalypt. Field work is needed to understand how the management of these forests will affect summer low flows, especially when similar rainfall-runoff response can mask very different transit times [*Hale and McDonnell*, 2015].

Controlled experiments are also needed to push systems beyond the range of events one might encounter in a PhD timescale. The LEO slope at Biosphere-2 is a great example [*Gevaert et al*., 2014] as are the earlier covered roof experiment at Gjardson [*Rodhe et al*., 1996] and the Hydrohill experiment in China [*Kendall et al*., 2001]. We need more of these. What happens when a catchment is denuded of its soil - how does it respond? How does it respond when the soil is put back? We are doing this with cover systems at mine sites all around the world but are any of these sites being studied in detail? There are new opportunities to see how such systems evolve with one good example at Chicken Creek by *Gerwin et al*. [2009]. There are classic old experiments like *Hewlett and Hibbert* [1963] with a controlled repacked hillslopes (incidentally, used by *Sloan and Moore* [1984] later to test a suite of hillslope models) that could provide a perfect vehicle to chronicle soil structural development through 50 years of pedogenesis. It seems like field-based preferential flow studies have largely ground to a halt. We are seeing clear evidence of the importance of bedrock groundwater in the headwater hydrograph, but, aside from limited groups in Japan and a new CZO effort, who is drilling into the bedrock to interrogate that groundwater chemistry and water table dynamics? Who is mapping the permeability architecture of the subsurface and how might this be done across large areas of hillslope?

Finally, outrageous hypotheses are needed. Perhaps the phrase: "the Richards equation is based on the wrong experiment" (as advocated by *Beven and Germann*, 2013] is a good start? We might advocate that fill and spill theory [of *Tromp van Meerveld and McDonnell*, 2006] can largely explain all runoff phenomena at all scales. And, that there are effectively two water worlds—one tightly bound world that plants use for transpiration and one mobile world that we see expressed in groundwater recharge and streamflow [*Evaristo et al*., 2015]—and that this ecohydrological separation effectively undermines transit time estimates that do not account for this lack of mixing. And finally, perhaps going full circle to WRR issue number 1: that paired watershed studies have been based on the wrong the 'before and after experimental design' and that frequency-pairing is the only way to see true harvesting effects [*Alila et al*., 2009].

5. Summary

Like the famous Sydney Brenner quotation: "Progress in science depends on new techniques, new discoveries and new ideas, probably in that order" [*Robertson*, 1980], field work and new techniques are key. *Kirchner et al*. [2004] make the point that science is often driven forward by the emergence of new measurements; whenever one makes observations at a scale, precision, or frequency that was previously unattainable, one is almost guaranteed to learn something new and interesting. For example, stable isotopes have had a revolutionary effect on our tracing of the water molecule. New distributed fiber optic temperature sensing [*Selker et al*., 2006] is having a similar impact. New hydrogeophysics techniques are opening up novel opportunities [*Ward et al*., 2014. Unmanned aerial vehicles (UAVs) offer the prospect of low-level surveillance (e.g., LIDAR, thermal IR) that was unimaginable just a few years ago [*Spence and Mengistu*, 2015]. Field-deployable autoanalysers are now a reality, and ion-specific electrodes continue to improve, most especially at low concentrations and limits of detection. These technological developments promise to provide measurements of rainfall and streamflow chemistry at hourly or subhourly intervals (similar to the time scales at which hydrometric data have long been available) and to provide these measurements for long spans of time, not just for intensive field campaigns associated with individual storms [*Kirchner et al*., 2004].

But new discoveries can still happen without the use of cutting-edge technology. Sydney Brenner was a microbiologist and his emphasis on new (lab) technology is not unexpected. But hydrology is an outdoor science—where our data come from and our ideas are ultimately tested. Field work, by its very nature, involves observation, curiosity about the world around us. For those of us who are educators, we should be encouraging field work whenever we can, raising awareness of the value of field work and promoting its benefit in terms of personal development (observational skills, teamwork, etc.). But above and beyond these generic benefits, field work's primary purpose must be to teach our students to be curious, to look, to collect data, to test existing ideas, to develop new hypotheses, including outrageous ones!

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