

## Discussion of “Simple Estimation of Prevalence of Hortonian Flow in New York City Watersheds” by M. Todd Walter, Vishal K. Mehta, Alexis M. Marrone, Jan Boll, Pierre Gerard-Marchant, Tammo S. Steenhuis, and Michael F. Walter

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### Introduction

Many of our models in engineering practice “work” but for perhaps the wrong process reasons. In many cases, this may be tolerable for specific problem solving and applications where flow alone is the key. Increasingly however, the things that we are challenged to model are strongly conditioned by internal processes. Such is true with water quality models—whose formulations often demand a link between input–output response and internal water flow pathways, chemical reactions along those flowpaths, and mixing of waters from different regions in the watershed across different timescales.

Thus, the paper by Walter et al. is a welcome contribution to the engineering literature, as it raises the issue of the process and pathway of delivery of water to stream. The paper examines critically the prevalence of Hortonian flow in the New York City watershed. Ironically, this is also the area where Horton did much of his pioneering work on infiltration theory. The authors find Hortonian overland flow occurrence to be highly restrictive in terms of rain intensities and durations, where it is unlikely to occur anywhere in the region (aside from paved and disturbed surfaces) for events smaller than the 3-year 15-min event. It was only for summer events from May–August for 15-min intensities of <10-year magnitude that Hortonian overland flow would be expected to occur.

These findings challenge the engineering community to think about the lingering legacy of Horton infiltration theory and its embeddedness in our operational models. The authors make a clear exposition of the limitations of these standard approaches and advocate new, saturation excess formulations for these regions. Indeed, showing “where things work” and, more importantly, “where they don’t work” is a key part of progress in the science of hydrology. The authors note, and I agree, that determining which process dominates has a profound effect on determining methods for watershed management.

### An Overland Flow Non Sequitur

My issue with the paper, and the motivation for this comment, is that while they show the restricted nature of Hortonian flow in

New York City watersheds, their primary conclusion (on page 217) is that “The Catskills area of New York State, in which the Catskills reside, appears to have a low frequency of Hortonian flow, which supports previous anecdotal evidence that saturation excess is the primary process involved in generating overland flow.” To me, this statement is a non sequitur—I fear that the unsuspecting engineer may be left to “read between the lines” that saturation overland flow is the only mechanism to generate rapid runoff in New York City watersheds.

The authors note that the Catskills are characterized by steep hilly topography and shallow permeable soils. In fact, Delaware County and the Delaware watershed are largely forested with significant upland areas that dominate the larger watershed area. While not demeaning the processes of runoff generation and flux of labile nutrients from agricultural fields in and around a surface-saturated area due to a rising water table (the acknowledged focus of the authors’ previous and ongoing research), it must be acknowledged that the large preponderance of data from the New York City watershed(s) would suggest that subsurface storm flow is a major runoff response mechanism in the Catskills—especially for the return periods examined by the authors in their paper. The lateral transfer of water from hillslopes to streams is, in many cases, the dominant streamflow generation mechanism in the New York City watershed. While difficult for the engineer to diagnose via a hydrograph or saturated area map, the hydrograph *composition* often shows this to be the case.

For instance, subsurface storm flow (sometimes termed “interflow”) is often a dominant mechanism to explain the runoff volume and time source composition of channel flood waters in the headwaters of the Catskills (Brown et al. 1999). Subsurface storm flow is a significant delivery agent for DOC flushing in the Neversink watershed (Welch et al. 2001) and largely controls the fate and transport of septic effluent entering surface waters in the West of Hudson area (Sherlock et al. 2000). In addition, work in the New York City watersheds has shown that subsurface storm flow has a large effect on surface water nitrate concentrations outside of the limited agricultural land uses (Murdoch and Stoddard (1992) and controls to a large extent the acid neutralizing capacity of stream waters in the undisturbed areas of the region (Lawrence 2002). Some recent work has shown also that subsurface storm flow during rainfall and snowmelt events is a dominant control on the age, origin, and pathway of watershed runoff in low density suburban areas in Dutchess County (T. Vitvar, manuscript under review).

### Conclusion

My worry is that an engineer reading the original paper may think that simply rejecting Horton enables one to then automatically accept Dunne! (widely acknowledged as the originator of the saturation overland flow conceptualization.) While I realize that the authors are not stating this per se, I am concerned that readers of the paper may take this the wrong way. Indeed, there are instances where saturation excess does dominate, but I would argue, that for managers of the New York City watershed, lateral subsurface storm flow may be a more vexing issue for water quality

and quantity. As an engineering community, we need to begin to consider models that work for the right process reasons. In New York City watersheds, this will involve integrated assessment across different spatial scales, landuses, and process domains. Moving away from Hortonian overland flow is simply the first step.

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## Introduction

The writers appreciate the thoughtful and cogent comment by the discussor on our paper in which he correctly asserts that rapid flow from the landscape to streams is not restricted to overland flow mechanisms but, rather, that rapid subsurface flow paths should also be explicitly acknowledged. As noted by the discussor, our paper and his comment emphasize that recognizing and understanding physical hydrological and pollutant transport processes is an essential precursor to "engineering" water quality protection strategies. The discussor's comment contributes to an

interesting discussion that perhaps warrants some more dialogue here.

## Historical Note

The discussor noted the irony that Robert Horton developed the infiltration excess concept at his lab in Voorheesville, N.Y., which is geographically between Sleepers River, Vt. (~300 km north), where Thomas Dunne and Richard Black carried out their landmark experiments on saturation excess hydrology (Dunne and Black 1969, 1970a,b, 1971) and the Catskill Mountains (~150 km south), where the writers (Frankenberger et al. 1999, Walter et al., 2000; Scott et al. 2001) and the discussor (e.g., citations in McDonnell's comment) have been investigating non-Hortonian hydrology and associated chemical transport processes. This suite of work together emphasizes the range of different hydrological processes that need to be more directly recognized by engineers to develop reliable water quality protection practices. It should probably be noted that much of the field data Horton reported to support his infiltration-capacity theory was from outside the northeastern United States, including arid areas like Arizona (e.g., Horton 1940, p 405) where we would anticipate Hortonian flow. Recently, Beven (2004) took an in-depth look at Horton's Voorheesville data on infiltration excess and proposed plausible explanations consistent with the region's highly permeable soils that we noted in our original paper. The writers echo the discussor's challenge to "... the engineering community to think [critically] about the lingering legacy of Horton infiltration theory..."

## Hydrological Flow Paths: Identification and Implications

The increased attention to hydrological flow paths has led to a remarkable evolution of terminologies and the inevitable blurring of their definitions. "Runoff" is perhaps the most dubious of terms, often used synonymously with "overland flow," despite the more formal definition as all the water that leaves a watershed above ground, i.e., the stream flow in many watersheds (e.g., Chow 1964). Copious prefixes to the term runoff, e.g., direct runoff, subsurface runoff, etc. and new terms like interflow, return flow, quick flow, old- and new-water have also entered the hydrological vernacular in an attempt to add precision to our scientific rhetoric but perhaps their copiousness better illustrates how difficult it is to capture the complexities of watershed hydrology in words. This difficulty is perhaps partially at the root of our discussion and, in fact, I think we share the discussor's vision of how the physical processes affect flowing water through the Catskills, especially with respect to storm runoff, which includes surface and subsurface contributions.

That storm flow often has a large subsurface component is well established in hydrological science, as evidenced by the citations in the discussor's comment. However, distinguishing in the field between subsurface and overland contributions to a storm hydrograph is difficult and, perhaps, not meaningful at hillslope and at larger scales. Indeed, the saturation excess overland flow concept is usually understood to include a rapid subsurface flow component (e.g., Dunne and Leopold 1978 pp. 256, 258). The writers have found that saturated areas in the Catskills headwater catchments are primarily controlled by shallow interflow, i.e., rapid subsurface lateral flows.

Exfiltration of interflow occurs in these saturated areas bringing subsurface water to the surface. With respect to stream water, then, it is unclear what the appropriate terminology is to describe the flow paths since water that flowed originally in the subsurface also moved overland (and, in other situations, visa versa). This is partially why “hydrograph [chemical] composition” is not a universally accepted indicator of flow paths (e.g., Waddington et al. 1993; Buttle 1994; Burns 2002). Also, the simple experiments by Ahuja and Lehman (1983) and Gao et al. (2004) show that overland flow should be expected to contain soil-water chemicals (i.e., the chemicals used to identify subsurface flow components), exemplifying the fact that the presence of chemicals associated with subsurface water does not necessarily indicate that the flow path transporting the chemicals is a subsurface route.

Despite the difficulty of discerning interflow from overland flow, we agree with the discussor that both flow paths are important. However, explicitly distinguishing between them is perhaps not as necessary as the discussor suggests, at least not with respect to nonpoint source pollution control. In the landscape, saturated areas coincide both with those areas that are likely to generate overland flow and areas that experience rapid subsurface flow. That these areas are often near streams is not a coincidence as streams are essentially perpetually “saturated areas.” We of course recognize that rapid lateral subsurface flow could be independent of upland surface saturation but, in the absence of artificial drainage, this type of flow is most commonly noted in forests, which are not generally considered major contributors of nonpoint source pollution.

Let us leave this rather conceptual discussion for some brief practical comments about effective management for protecting water quality in landscapes prone to saturation excess and associated interflow. Perhaps the simplest and most logical management practice is to simply avoid placing potential pollutants in areas that are prone to saturation, i.e., hydrological sensitive areas (HSA). As mentioned, these areas are often close to streams, thus, riparian buffer strips are probably effective, not because runoff flows through them and is “treated” (magically?) or “filtered” before it flows into the stream, but because there are no pollutants placed in the buffer strips/HSAs to enrich the storm runoff flowing from these source areas (either by surface or subsurface flow paths). We often hear people suggest removing HSAs with drainage-tile lines, but it is unlikely that this “solution” will work because it simply reroutes the rapid pollutant transport to the subsurface (e.g., Geohring et al. 2001). This is somewhat indicative of the writer’s comment that “... subsurface storm flow may be a more vexing issue for water quality...” than overland flow. Although sometimes management practices work for the wrong reasons, clearly, more attention to water quality protection strategies that recognize, more fully, the complete system of physical hydrological processes will provide more reliable solutions.

## Conclusion

It has been noted previously, e.g., Walter et al. (2000), that water quality modeling and protection strategies are lagging advances in hydrological science; this disconnect is hampering the development of new and potentially more reliable approaches to watershed management. We would like to again thank the discussor for initiating this discussion and we hope that engineers and water quality modelers will continue to be increasingly attentive to physical hydrologic processes and their implications for water resources protection.

## Acknowledgments

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