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DISCUSSIONS AND CLOSURES

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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Note. Associate Editor:

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 $3\text{-year}\,15\text{-}$ min event. It was only for summer events from May–August for 15- min intensities of $< 10\text{-year}$ 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.