

# SEARCH FOR PHYSICALLY BASED RUNOFF MODEL—A HYDROLOGIC EL DORADO?<sup>a</sup>

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**ABSTRACT:** The search for physically based distributed runoff models has been underway for more than half a century. A great deal of progress has been made and we have a much better understanding of hydrological processes today than engineers and scientists did in the 1930s. Physically based models are being used more frequently in engineering practice, but questions are being raised regarding their "physical basis" and accuracy, and it has been suggested that simpler models are superior. Selected papers presenting tests of physically based models or comparisons with simpler models are reviewed. It is shown that, on a relatively small scale, these papers are overly pessimistic due to problems of hydrologic measurement and interpretation. However, there are great difficulties involved in scaling up to larger watersheds and we must realize that significant uncertainties are involved in predicting surface runoff.

## INTRODUCTION

I am greatly honored to be named the Hunter Rouse Lecturer for 1994 and to join the highly distinguished group of previous recipients. Unlike some of them, I was never a student of Hunter Rouse, nor were our contacts of a professional nature. In the late 1970s, Hunter Rouse spent the summers in Fort Collins where he taught a graduate course at Colorado State University. At that time we had a small lunch room at the Engineering Research Center and I shared a table with him on many occasions. I recall several pleasant conversations on a variety of topics.

I am struck by the large element of chance involved in receiving an award such as this. Clearly, the factors that condition the probability relate to my good fortune in having outstanding teachers, students, and colleagues, and in being in the right place at the right time. I have been privileged in this regard throughout my career, and I am most grateful for the guidance, encouragement, and friendship I have received. I also wish to acknowledge the unwavering support of my wife, Kathryn.

Much of my career has been devoted to the search for a physically based, distributed surface runoff model and I think that quite a bit of progress has been made. Models of this sort are being used more and more in engineering practice. However, in the past 10 years, several papers have appeared, which seem to show that the predictive ability of these models leaves a great deal to be desired. Indeed, some question whether such a model is possible or desirable. One of the books that I read during my recent stay in Córdoba, Spain, was *The Loss of El Dorado* (Naipaul 1969), a most interesting book in which the author describes some 16th Century Spanish expeditions to search for El Dorado.

El Dorado, though, is essentially a Spanish delusion. There had been a golden man, *el dorado*, the gilded one, in what is now Columbia: a chief who once a year rolled in turpentine, was covered with gold dust and then dived into a lake. But the tribe of the golden man had been conquered a generation before Columbus came to the New

World. It was an Indian memory that the Spanish pursued; and the memory was confused with the legend, among jungle Indians, of the Peru the Spaniards had already conquered. (Naipaul 1969)

This is far from a perfect analogy because no one is expecting to receive great wealth by developing a physically based, distributed runoff model. However, one might raise the question: "Are we searching for something that is unattainable?" In attempting to answer this question, I have reread several review papers dealing with this topic. Included among them are three of my own (Woolhiser 1973, 1975; Woolhiser et al. 1990a). It has been most interesting to follow the evolution of my own attitudes toward these models over the past two decades.

My attitudes are clearly colored by my interests, past research assignments, and experience, so it is appropriate that I summarize them. I have been most interested in surface runoff phenomena on small, agricultural watersheds and have had experience in field measurements of precipitation and surface runoff in Arizona, Wisconsin, Missouri, Iowa, and Colorado, and in indoor and outdoor experiments at Colorado State University. I have done some mathematical modeling of surface runoff and of erosion and chemical transport in small watersheds and have had a limited amount of experience in obtaining data on erosion and water quality in the laboratory and the field.

## SOME DEFINITIONS

First, it seems appropriate to consider what we mean when we speak of a physically based distributed runoff model. I assume that it is generally accepted that: "Models, formal or intellectual on the one hand, or material on the other, are ... a central necessity of scientific procedure." [Rosenblueth and Wiener (1945), p. 316]. From this, it certainly follows that models are essential in engineering practice, where models of physical systems are combined with economic models in an attempt to maximize net social benefits. After all, a model is just our best description of "how things work." A mathematical model belongs to the formal or intellectual class of models and consists of a set  $L_1, L_2, \dots, L_n$  of general laws or theoretical principles and a set  $C_1, C_2, \dots, C_m$  of statements of empirical circumstances (Hempel 1963). The general laws in a runoff model include conservation of mass and possibly the conservation of momentum, while the empirical statements might include Manning's friction relationship for overland or open-channel flow, Darcy's law for flow in porous media, and the system geometry. The most general partial differential equations are written in three space dimensions and time, with the term "distributed" referring to the space dimensions. Al-

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though the equations are continuous, we usually resort to discrete (finite-difference or finite-element) methods to solve them. Thus, even if the physical entities represented by the parameters vary smoothly in space and are constant in time, the parameters are actually lumped to some extent in the model. This is not important when the computational scale is smaller than the scale of the variations, but when the computational scale becomes too large, important variations in the parameter fields will be missed or badly distorted.

We must keep in mind that all models are simplifications or abstractions of reality and all models are to some extent wrong. In fact, if they aren't simpler in some sense than the real-world object, they aren't useful! For this reason we neglect certain aspects of the problem because they are considered to be unimportant. These simplifications should be based on sound physical reasoning or strong empirical evidence obtained from field studies or appropriate material models. We have already made significant simplifications when we describe the surface topography by stream tubes, cascades of planes, rectangular grids, or triangular irregular networks; use Manning's equation to describe the dynamics of overland and open-channel flow; and use Darcy's law for unsaturated and saturated flow in the soil, vadose, and ground-water zones. Other types of simplification include linearizing nonlinear equations (Stanley et al. 1970), using the kinematic approximation for overland flow (Woolhiser and Liggett 1967), or in the transport of chemicals and hydrodynamic dispersion neglecting molecular diffusion and hydrodynamic dispersion. A very common simplification is to reduce the dimensionality by treating streamflow as a problem in one space dimension. One extremely important simplification is due to our inability to describe the system geometry and properties in sufficient detail as we deal with larger (and generally more complicated) watersheds. Because of difficulties and the cost of measurement, we simply cannot provide a detailed, three-dimensional description of the surface microtopography, the hydraulic characteristics of the soils, and the underlying geologic materials. For this reason, we make some sweeping assumptions, i.e., the soil consists of layers of known thickness and the vadose and ground-water zones are homogeneous or have some regular stratification.

In my opinion, the term physically based, distributed model is an improper term, which was originally used to describe models that are not physically and bucket compartment models or general linear or nonlinear system models. A better approach might be to recognize that, instead of specific classes of models, we have an array, with the simpler models being higher-order abstractions of the more detailed models (Woolhiser 1975). As such, they share some common properties, but all inevitably involve distortion. The simpler models are more general and involve fewer parameters, but this generality has been purchased at the price of a less detailed description. We would expect that when the computational scales of the physically based model and the scales of spatial variability of the real system are commensurate, the parameters would have some physical significance, provided that important mechanisms have not been ignored. However, as the system being modeled becomes too large and the computational scale becomes large relative to the scale of variability of the real system, the parameters will most certainly lose some of their significance, although there may be some regularities that can be used.

## BACKGROUND

The search for a physically based, distributed runoff model has been underway for a long time (more than 50 years). Hjelmfelt and Aronson (1980) discovered a paper by Merrill Bernard, written and presented in 1937, in which he used a

rectangular grid to represent the topography of a small watershed and used an essentially kinematic routing scheme to represent overland flow. Infiltration was described by an empirical equation. He observed the kinematic shock phenomenon [not so named until Lighthill and Whitham (1955)]. Of course, all the computations had to be done by hand, so his work had little impact and was forgotten for more than 40 years. With the development of the digital computer in the 1950s, interest was renewed in the computational aspects of watershed modeling. Several early papers dealing with the mathematical modeling of overland flow include those by: Behlke (1957), Liggett (1959), Morgali and Linsley (1965), Schaake (1965), and Brakensiek (1966).

I completed my undergraduate studies at the University of Wisconsin in 1955 and took a position at The University of Arizona where I was responsible for installing instruments and analyzing data from a 7.5 ha. (18.5 sq mi) experimental watershed near Tucson. My experience with this semiarid watershed convinced me that a distributed model was essential to describe the important hydrologic phenomena. After three years I joined the Agricultural Research Service (ARS) of the U.S. Department of Agriculture (USDA) and worked at the 153 km<sup>2</sup> (59 sq mi) Walnut Gulch Experimental Watershed at Tombstone, Arizona for nine months. My experiences there strengthened my belief that the current procedures such as the Soil Conservation Service (SCS) curve number and the unit hydrograph simply could not accommodate the high spatial variability of the rainfall and the interaction between runoff generated in the upland areas with the infiltration losses in the alluvial channels. In 1959, I was transferred to Madison, Wisconsin, where I worked for Minshall (1960), a very painstaking experimental hydrologist who was the first to provide strong experimental evidence of the nonlinear nature of runoff from small watersheds. I learned a great deal from him about the importance of accurate measurements, careful calibration and maintenance of equipment, and checking of computations. I first met the Hunter Rouse Lecturer for 1989, Jim Liggett, at The University of Wisconsin where it was my good fortune to take his course in advanced fluid mechanics. I was also introduced to the excellent book *Engineering Hydraulics* (1950), edited by Hunter Rouse. I was transferred to Columbia, Missouri in 1961 where I helped design and install the instrumentation for the Treynor, Iowa experimental watersheds, which were set up to determine the effects of level terraces on the hydrologic regime, including erosion losses and both water quantity and quality. One of my assignments at this time was to read through several annual reports of watershed studies that were begun in the Midwest in the 1930s. These reports were enlightening because they showed how the engineers had set up the studies to try to understand the physical processes involved in the transformation of rainfall to runoff. They recognized the many different mechanisms of surface runoff generation and made careful measurements of soil properties, even to the extent of digging trenches to sample and map soil-profile variability along hillslopes. The engineers who started these studies were highly talented and were well-supported with personnel and equipment. However, with the outbreak of World War II, most of the staff went to war and the funding was drastically cut. Many studies were discontinued and the objectives of the remaining studies were curtailed.

An interesting finding from some studies in Illinois was that the runoff from a watershed could not be adequately described merely by adding the products of the areas of soil mapping units and the runoff per unit area of small plots located within them. This finding was no doubt related not only to interflow and perched ground-water contributions to runoff and to complex runoff-runon interactions along hillslopes, but also to the small-scale spatial variability of soils. This observation was

one factor that led to the establishment of large experimental watersheds by the newly formed Agricultural Research Service in the 1950s and an interest in developing numerical models of watershed behavior.

Much of the research in the 1960s dealt with the mathematical description of overland and open-channel flow, and investigators usually started with the one-dimensional St. Venant equations or a simplification such as the kinematic wave. Actually, when Liggett and I started our joint research at Cornell University in 1964, we intended to show that the St. Venant equations without simplification were required for the overland flow problem, although we came to quite different conclusions (Liggett and Woolhiser 1967; Woolhiser and Liggett 1967).

I returned to the ARS in 1967 and was stationed in Fort Collins, Colorado. At that time, the soil physicists in our organization were critical of our use of empirical infiltration equations in calculating rainfall excess. The implication was that we would solve the problem if we used the physically based Richards equation. The logical step was to couple the Richards equation for unsaturated porous media flow with the overland flow equations. This was achieved by Smith and Woolhiser (1971) in an experimental and analytical study. The increasing concerns of society regarding environmental problems in the 1960s and 1970s led to greater research emphasis on erosion and transport of agricultural chemicals. Now the trend seems to be toward describing the hydrology of large basins to evaluate the effects of possible climate change on water supply, floods, erosion and sediment transport, etc.

Currently, we seem to have two opposite trends. Physically based, distributed models are gaining greater acceptance in the engineering community, not only for estimates of runoff volumes and peak rates, but also for estimating soil erosion and transport of nonpoint pollutants. However, some in the research community and some practitioners as well have raised warnings and have suggested that this type of modeling is transscientific, and that simpler models should be used. My objective is to attempt to reconcile these seemingly irreconcilable opinions.

## CRITICISMS OF PHYSICALLY BASED MODELS

There are two classes of criticisms of physically based models:

1. Current physically based models are not really based on physics. If the models have a true physical basis, we should be able to estimate the parameters a priori or measure them in the field, yet such estimates have a great deal of uncertainty. Further, it is more difficult to calibrate physically based models because they are overparameterized. "For now, it is sufficient to conclude that the current generation of distributed physically based models are lumped conceptual models." (Beven 1989).

If there is an example of a computational hydraulics problem that has been defeated by the wealth of detail necessary for a practical solution, it must be this one. We are nowhere near to describing the hydraulic properties of a watershed. There have been useful advances and programs, but many of the watershed programs require the tuning of so many knobs that they are no better than statistical black boxes. (Liggett 1990)

2. Physically based models are not as accurate and are more likely to be misused than simpler models.

To deal with these criticisms, we must first consider the size of the hydrologic systems we are dealing with and the purpose of the modeling effort. The size of the hydrologic system under consideration might range from a laboratory column or a

flume to the Mississippi River basin. There seems to be little argument that models describing the laboratory systems are physically based, although one can argue this point. For example, if the equations describing free surface flow with raindrop impact do not include the surface tension forces or transport by splash, they may greatly distort reality near the upstream boundary where water depths are smaller than the diameter of the raindrops. Even in very simple laboratory systems where free surface flow is combined with porous media flow, we can find observations that are not consistent with current physically based model predictions (Snyder and Woolhiser 1985). I venture to say that few, if any, engineers would consider using a three-dimensional hydrologic model of the entire watershed to describe the behavior of the Mississippi River at New Orleans. So it seems that the scale we are concerned with is somewhere between these two extremes and, I would venture to guess, closer to the laboratory scale.

Models are generally used to understand or predict some natural phenomenon. "The first is to explore the implications of making certain assumptions about the nature of the real world system; the second is to predict the behavior of the real world system under a set of naturally occurring circumstances" (Beven 1989). In the early days of the ARS (1955-65), we were attempting to develop better technology to predict the effects of changes in land-use practices on floods in order to better design PL-566 watersheds. In those days, I (and many others) believed that if we could create distributed models that could take into account variations in soil mapping units and land use, we would solve the problem of spatial variability. Now we know that spatial variability is very important on much smaller scales. Later our interests shifted to predicting erosion losses and chemical transport from small watersheds in order to reduce nonpoint pollution from agriculture. More recently, we have been concerned with the effects of climate change on these factors. There seems to be little disagreement regarding the usefulness of physically based models for understanding hydrologic systems, so why is there so much concern about their predictive capabilities? This concern seems to arise from three sources: (1) Scientific papers, which conclude that certain models perform poorly in predicting runoff from experimental catchments; (2) perceived difficulties in estimating model parameters; and (3) a fear among researchers and some practitioners that models are being "oversold" or are being applied in inappropriate circumstances by persons who do not understand the model.

## ACCURACY AND SCALE OF PHYSICALLY BASED MODELS

Although there were calls for comparative model testing earlier, Loague and Freeze (1985) were the first to respond with a thorough study that included a physically based model. Their objectives were commendable: to compare the predictive capabilities of a distributed, quasi-physically based (Q-P) model (Engman and Rogowski 1974) with a regression model and a unit hydrograph model with a Phi index loss function. They used data from three experimental watersheds: Watershed R-5, 0.1 km<sup>2</sup> in the Washita River Experimental Watershed in Oklahoma; Watershed WE-38, 7.2 km<sup>2</sup>, in Mahantango Creek Experimental Watershed in Pennsylvania; and catchment HB-6, 0.13 km<sup>2</sup> in the Hubbard Brock Ecosystem Study in New Hampshire. The data were divided into calibration and validation sets and the Nash-Sutcliffe efficiency criterion was used to compare model predictions of runoff volume, peak flow rate, and time to peak. They concluded that none of the models performed very well and that the quasi-physically based model was acceptable only on the R-5 watershed.

There have been many justified criticisms of this work. One deals with the choice of watersheds. The quasi-physically

based model is based on the kinematic routing of runoff generated by the Hortonian mechanism, while the saturated hydraulic conductivity of the forested Hubbard Brook catchment was much greater than the greatest rainfall intensity, so one would not expect the model to apply. The Mahantango Watershed is also marginal in this regard, because it is known that there are other mechanisms, saturated overland flow and interflow, that are significant. The two largest events (where the Hortonian runoff could be significant) were eliminated from the data because the durations were too long. A careful examination of the Nash-Sutcliffe statistics reveals that the quasi-physically based model actually performed better than the other two for peak and volume at R-5, in spite of the fact that parameters for the other two models were estimated from the calibration data set while the parameters for the Q-P model were estimated a priori. Further, the poor time-to-peak efficiency statistic for the Q-P model was strongly influenced by one long storm where runoff continued for many hours and the model predicted the peak at the early part of the storm, while the real peak (not much different in magnitude) occurred many hours later! This type of error has no practical or theoretical significance. Another factor that must be considered in this study is that a broadcrested weir is used to measure runoff from R-5. The weir pond is large enough to significantly affect the outflow hydrograph from the watershed, yet no "ponding corrections" (Brakensiek et al. 1979) were made. The regression model and the unit hydrograph model were automatically calibrated to the effects of the pond, while the Q-P model attempts to predict inflow to the pond. In addition, the storage in the weir pond below the notch elevation is greater than the volume of many of the events in the runoff record, and there is no record of the water level in the weir pond before each runoff event. Consequently there is a great deal of noise in the data for the small runoff events. Finally, only one rain gauge was used to provide input to the models, and the runoff per unit area of many of the runoff events was smaller than the resolution of the rain gauge. As we shall see, this is more important for the Q-P model than for the other two. We must conclude that the Q-P model with parameters estimated a priori is actually better than the two models for the watershed to which it applies, although the efficiency statistics are not impressive. The maximum forecasting efficiencies for the volume and peak rate achieved by Loague and Freeze (1985) for R-5 were 0.25 and 0.71 (after calibration), respectively.

Goodrich (1990) obtained much more promising results for a physically based model. He used the model KINEROSR, a research version of KINEROS (Woolhiser et al. 1990b). In this model, topography is represented by cascades of planes and channels; infiltration by the Smith and Parlange (1978) approximation to the Richards equation; and runoff and channel flow are modeled by a four-point implicit finite-difference approximation to the kinematic wave equation. KINEROSR includes a provision to model the small-scale spatial variability of saturated hydraulic conductivity by modeling runoff from plane elements as the sum of hydrographs from  $n$  parallel planes, each having a hydraulic conductivity equal to the mean of the  $i$ th increment of a lognormal cumulative distribution function,  $i = 1, \dots, n$ . Goodrich (1990) applied the model to four small watersheds in the Walnut Gulch Experimental Watershed operated by the ARS, USDA in southeastern Arizona. Rainfall and runoff data from watersheds LH-106, 0.36 ha; LH-102, 1.46 ha; LH-104, 4.40 ha; and WG-11, 631 ha were divided into calibration and verification sets. All parameters were identified based on published information and limited field studies. Initial soil water content was estimated at each rain gauge using a water balance model CREAMS (Knisel 1980). The relative values of saturated hydraulic conductivities

$K_v$ , coefficient of variation of the conductivities  $C_v$ , and Manning's  $n$  for overland flow planes were assumed to be correct, but three multipliers of these values were fitted in the calibration phase to approximately maximize the Nash-Sutcliffe forecasting efficiencies for runoff volume and peak. The forecasting verification efficiencies attained for runoff volume and peak flow rate, respectively, were: LH-106, 0.98 and 0.79; LH-102, 0.93 and 0.93; LH-104, 0.99 and 0.96; and WG-11, 0.49 and 0.16. Clearly these efficiencies are very good for the three smaller watersheds, and the efficiencies for WG-11 are much better than those attained earlier. Unfortunately, Goodrich (1990) did not compare these results with those of simpler models, so we do not have a direct comparison. However, Hughes and Beater (1989) used data from six Walnut Gulch Watersheds (43 large events) with a lumped and semidistributed conceptual model. Their best forecast efficiencies were -0.02 and 0.01 for the lumped and semidistributed version of their model where no parameter adjustment was allowed for verification events. We have no evidence here for the superiority of "simpler" models at this scale!

To put the results of Goodrich (1990) into perspective, however, we must consider the related work of Michaud and Sorooshian (1994a), who used the same distributed runoff model (KINEROSR) on WG-1, the entire 150 km<sup>2</sup> Walnut Gulch Experimental Watershed. Their objective was to determine whether distributed runoff models would be useful for real-time flash-flood prediction in semiarid regions. Accordingly they used rain gauge densities similar to those found at flash-flood warning sites (one gauge per 20 km<sup>2</sup>) instead of all appropriate gauges on the watershed. They compared the accuracy of the KINEROSR predictions with those of a simple distributed model based on the Soil Conservation Service (SCS) method and a simple lumped model (also based on the SCS method).

Michaud and Sorooshian (1994a) concluded that: "None of the models investigated here were able to accurately simulate peak flows or runoff volumes for individual events. Models showed somewhat more skill in predicting time to peak and the peak to volume ratio." In a related study, Michaud and Sorooshian (1994b) showed that the rainfall errors due to too few rain gauges were responsible for about half of the simulation errors. They also pointed out that KINEROSR, which has nonlinear routing algorithms, may be more sensitive to rainfall errors as demonstrated by Singh and Woolhiser (1976).

What factors are responsible for the deterioration of model accuracy as watershed size increases? It is not possible to examine all possibilities in detail. Certainly, the distortions involved as the scale increases are a major factor. Michaud and Sorooshian (1994a) reported that the length of the pervious planes used in their watershed discretization ranged from 76 to 1.701 m. It is clear that elements of this size include significant channel networks, which would have different hydraulic properties and infiltration properties than the hillslopes contributing runoff to them. One can no longer claim much (if any) physical significance given plane elements of this size. What we are really doing is hypothesizing that the runoff generation and routing dynamics in the complex real system are similar to the runoff generation and routing characteristics of flow from a plane element. At smaller scales, this may not be a bad assumption. Indeed, Goodrich (1990) developed an algorithm for combining plane and channel elements into single plane elements that have nearly the same hydrologic response. His procedure involved maintaining basin area, mean overland flow length, average slope and soil parameters, and a characteristic response time (equilibrium storage divided by rainfall rate) by adjusting Manning's  $n$ . However, his results were for a much smaller scale (<631 ha), so it cannot be assumed that they also hold at a larger scale.

Another factor that would change with scale is the coefficient of variation,  $C_v$ , of saturated hydraulic conductivity. The algorithm in KINEROSR and the values of  $C_v$  may be adequate for very small scales, but it is certain that there would be much greater variability over the elements used by Michaud and Sorooshian (1994a). It may be that a lognormal distribution is no longer appropriate. Possibly a mixture of two distributions, one for the hillslopes and one for the channels, would be an improvement. However the parallel plane algorithm most certainly would not be adequate because of the directional nature of the flow and possible runoff-runon interactions.

## ESTIMATION OF PARAMETERS

It is clear that distributed runoff models have more parameters than simpler lumped models. Indeed, if we assign different values for each parameter at each computational node there can be no question that the model is "wildly overparameterized in a systems simulation sense" (Beven 1989), unless it is possible to directly measure the parameter values or estimate them from physical considerations. If the computational scale is large compared with the scale of spatial variation, then the parameters become "effective values" and cannot be measured. If we attempt to adjust or optimize them by comparing observed and simulated runoff, we must reduce the dimensionality of the problem by making some regularity assumptions and by a priori estimation of parameters to which the model is insensitive. It is also known that there are significant interactions between some parameters. "Parameter interaction is inherent in the physics of hydrological systems and given the number of parameters available in physically based models, any optimization of parameters must be subject to far greater problems of interaction than simpler lumped models" (Beven 1989). Is the situation for physically based models hopeless and are simpler models so much better?

One cannot disagree with the fact that it is difficult or impossible to calibrate models with many interacting parameters. It also appears that many of the problems associated with parameter estimation may, in fact, be due to poor measurements of the spatial and temporal characteristics of rainfall and/or to differential sensitivity of the parameters for small and large events. However, as we gain experience in model testing with accurate hydrologic data from different climatic regions over a range of basin scales, we may well find some regularities that will help provide initial parameter estimates or will guide optimization strategies. The works of Goodrich (1990) and Michaud and Sorooshian (1994) give us valuable insight into problems of calibration for watersheds when Hortonian runoff is the primary runoff generation mechanism, and where the importance of infiltration into the channels becomes more important with an increasing basin scale. It is especially revealing that parameter sensitivities change dramatically with both basin scale and magnitude of the rainfall input. For example, Goodrich (1990) found that runoff volume, peak rate, and time to peak were insensitive to Manning's  $n$  on both planes and channels for watershed LH-106 (0.36 ha), while Michaud and Sorooshian (1994a) found that runoff characteristics were very sensitive to Manning's  $n$  in channels. Physical reasoning would lead us to this conclusion as well: because LH-106 has a very short response time, the basin is frequently driven to near equilibrium. Under this condition, the peak rate and time to peak are primarily dependent on the rainfall intensity pattern. However, for WG-1 (150,000 ha), infiltration into the channels is often as great (or greater) than the runoff volume, so Manning's  $n$  as well as the channel geometry is important. It would be worthwhile to carry out similar studies on watersheds in more humid regions where the Hortonian surface runoff is important yet channel losses are insignificant.

The importance of high spatial and temporal resolution of rainfall data cannot be overemphasized. To conclude that a physically based model is a poor representation of reality when it fails to predict runoff well when the rainfall data are insufficient is akin to condemning a beam flexure formula because it fails to estimate the proper displacement when the wrong distribution of load is applied. Most hydrologists recognize the importance of the spatial variability of rainfall on the large scale. However, it is surprisingly significant on the small scale as well. A detailed study of rainfall variability was made on one of the watersheds used by Goodrich (1990). Goodrich et al. (1995) installed nine recording rain gauges and standard gauges on a 30 m grid within WG-104 (4.4 ha). Faurès et al. (1995) found that the spatial variability of rainfall can have a significant impact on model predictions of runoff even on this small scale. When five simulations were conducted for eight storms using input from only one of five recording rain gauges, one at a time, the coefficient of variation for peak rate and runoff volume ranged from 9 to 76% and from 2 to 65%, respectively. They concluded: "The results of this study indicate that if distributed catchment modeling is to be conducted at the five hectare scale, knowledge of the rainfall variability on the same scale is required. A single rain gauge with the standard uniform rainfall assumption can lead to large uncertainties in runoff estimation." (Faurès et al. 1995). It is also true that difficulties in the optimization of parameters will result from the poor spatial resolution of rainfall data.

## OVERSELLING MODELS

There have been several references in the literature to the problem of overselling of physically based models and the dangers involved in using models without adequate understanding. For example: "I have considerable concern about the practical application of the current generation of physically-based models. Software packages will soon be available to consulting engineers to allow such models to be used in a wide range of applications. There is a great danger that the theoretical rigor that underlies these models will engender uncritical belief in their predictions" (Beven 1989).

"Hydraulic engineers working with fluvial systems must recognize and avoid pitfalls associated with the use of mathematical models and expert systems (artificial intelligence) without full knowledge of the strengths and limitations" (Simons 1992).

"In using hydrologic models, the context of the original purpose and development is often lost, so they are applied to situations beyond their capabilities" (Grayson et al. 1992).

I cannot disagree with any of these comments. Certainly, at each step in any analysis with any model, the user should ask the questions: "Does this make sense?"; "What is the level of uncertainty of my prediction?"; "Does this level of uncertainty render the analysis meaningless?"; etc. However, these concerns apply to the simpler models as well as the more complicated physically based models.

## PROBLEMS IN MODELING SMALL RUNOFF EVENTS

In a thorough scientific test of physically based runoff models, it has appeared desirable to include events with a range of initial conditions, time patterns of rainfall, and total rainfall (and runoff) amounts. This ensures that the ability of the model to handle small- and large-scale spatial variability will be tested. However, it is important to appreciate the very sensitive nature of surface runoff modeling for small events (where the runoff is a small proportion of the rainfall) so that we don't have unrealistic expectations. Let us consider the problem at the hillslope scale. We will consider a plane surface with spatially varied saturated hydraulic conductivity  $K_s$ . Sev-

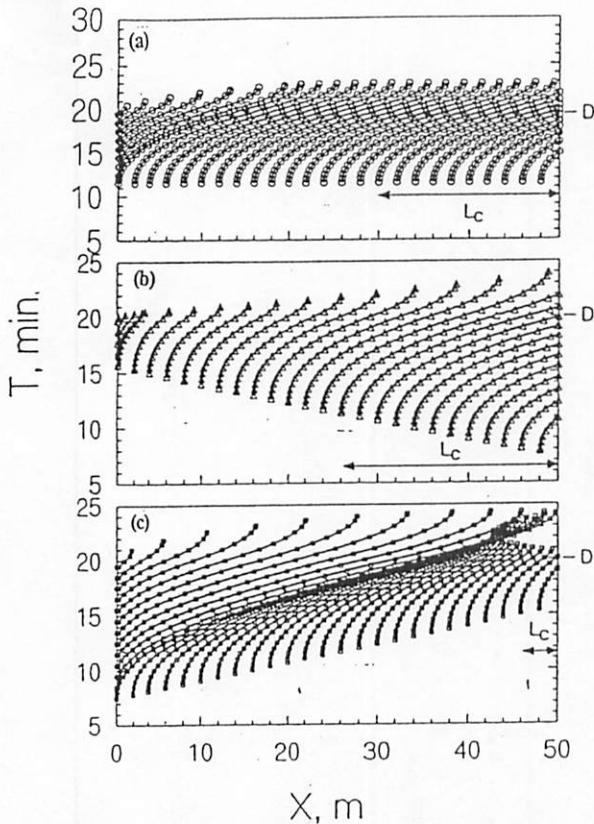


FIG. 1. Characteristic Nets for Hillslope Example of Smith and Hebbert (1979) with Rainfall Intensity of 88.8 mm/h for 20 min [from Woolhiser et al. (in press, 1996)]: (a) Case 4, Uniform  $K_s$ ; (b) Case 5, Decreasing  $K_s$ ; (c) Case 6, Increasing  $K_s$

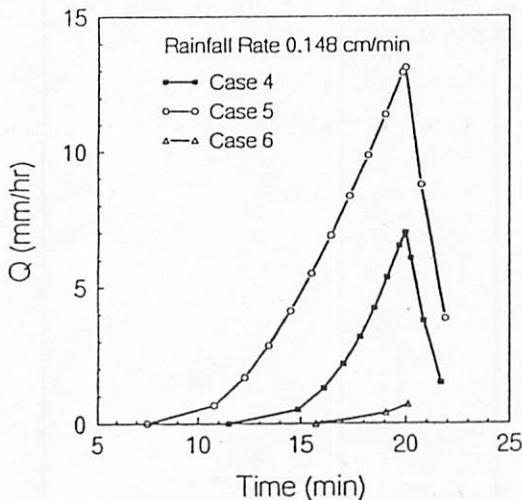


FIG. 2. Runoff Hydrographs for Hillslope Example of Smith and Hebbert (1979); Rainfall of 88.8 mm/h for 20 min [from Woolhiser et al. (in press, 1996)]

eral researchers have investigated this problem using somewhat different approaches. Freeze (1980) examined hillslope runoff response with spatially varied hydraulic conductivities. He analyzed a hillslope 100 m long and 200 m wide with saturated hydraulic conductivity varying on each 10 m  $\times$  10 m element. He used the Smith and Parlange (1978) infiltration equation for Hortonian runoff generation. However, there was no interaction between elements in the downstream direction (i.e., no runoff runoff), and Freeze (1980) used a simple translation of runoff from each element to obtain hydrographs. Smith and Hebbert (1979) used a distributed finite-difference model to study the effects of spatial variability on hillslope runoff. Both Freeze (1980) and Smith and Hebbert (1979)

found that spatial variability was very important for small storms, and that runoff response could not be closely approximated using average saturated conductivities.

To appreciate some of the subtleties involved, it is helpful to formulate the surface runoff problem in a kinematic characteristic framework (Woolhiser et al., in press, 1996). Characteristic nets are shown in Fig. 1 for the hillslope example of Smith and Hebbert (1979) with the low rainfall intensity (88.8 mm/h) and duration,  $D = 20$  min, for the cases of uniform  $K_s$  and  $K_s$  decreasing and increasing downslope. Hydrographs for these three cases are shown in Fig. 2 (case 4 has uniform saturated conductivity, in case 5 the saturated conductivity decreases downslope, and in case 6 the saturated conductivity increases downslope). The extreme variability of the hydrographs can be easily explained by examining the characteristic nets. We see that for all three cases, only a portion of the hillslope,  $L_c$ , contributes runoff to the downstream boundary because infiltration after the rainfall ends depletes the water en route. The case where  $K_s$  increases is the most extreme with only about 5 m of the hillslope contributing. Random variation of  $K_s$  around a mean value with the same standard deviation would lead to hydrographs with greater volume than the uniform case, but smaller than the decreasing  $K_s$  case. The response variability decreases as rainfall rates increase. Obviously, if there are trends in  $K_s$  in the field, a model that does not take them into account will provide a very poor prediction.

Another possibly important aspect of hydrology is the degree of interaction between runoff water and infiltration after rainfall has stopped. In the KINEROS model there is a parameter,  $h_c$  (RECS in FORTRAN), which is conceptually related to the effect of the microtopography on infiltration during recession. If  $h_c = 0$ , the entire wetted area is subject to infiltration loss when rain stops. If  $h_c > 0$ , the effective infiltrating area per unit wetted area is linearly related to the ratio of the local average depth,  $h$ , to  $h_c$  if  $h < h_c$ . If  $h_c > 0$  and  $h > h_c$ , the entire wetted area is subject to infiltration. This parameter was added when it was found that the model tended to underestimate recession hydrographs for plot data. On natural surfaces there is a concentration of flow into rills and we should expect this to affect recession infiltration rates. In testing on natural watersheds at a small scale, we haven't found the results to be very sensitive to  $h_c$ . However, it appears that there could be situations where  $h_c$  is very important. Fig. 3 shows the characteristic net obtained by setting  $h_c = 1$  cm for the case with  $K_s$  increasing downslope, and comparative hydrographs are shown in Fig. 4. Under this condition, more of the water in transient storage on the hillslope at the time rainfall stops can

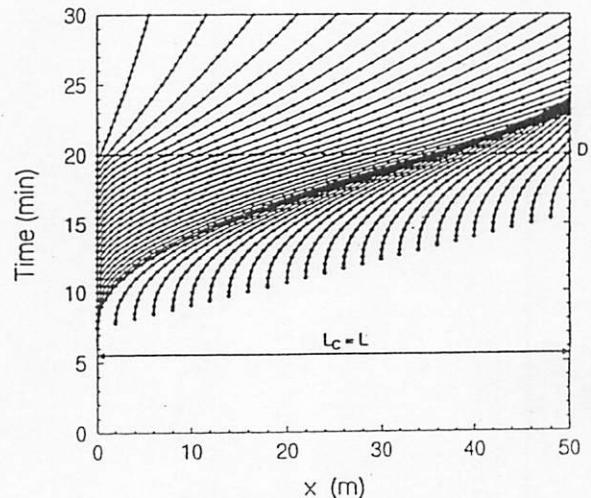


FIG. 3. Characteristic Net for Saturated Conductivity Increasing Downslope and  $h_c = 1$  cm [from Woolhiser et al. (in press, 1996)]

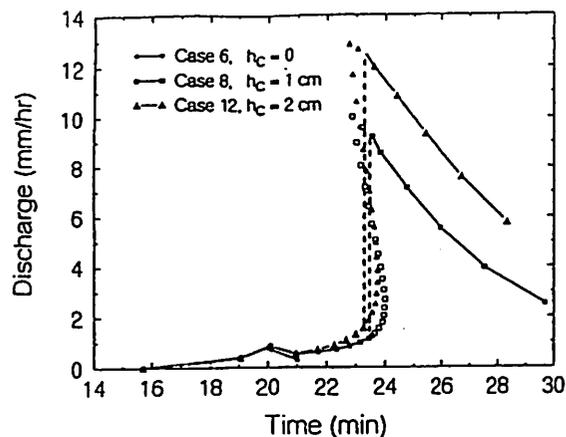


FIG. 4. Runoff Hydrographs for Saturated Conductivity Increasing Downslope [from Woolhiser et al. (in press, 1996)]

reach the lower boundary, resulting in dramatically different hydrographs.

The conclusion that can be drawn from these examples is that the runoff generation mechanism is exceedingly complicated for small runoff events under partial equilibrium conditions and we should expect poor agreement between model results and field flow measurements unless we have very detailed field measurements of the soil properties, rainfall gradients, and the microtopography. One can infer that the model structure and mechanisms for handling post-rainfall infiltration will become much more important as the scale of watershed elements (planes, stream tubes, pixels, etc.) increases, especially in arid or semiarid regions. This is an area that requires further research.

## ANALYSIS AND CONCLUSIONS

Can the search for a physically based distributed runoff model be described as a hydrologic El Dorado? Is a physically based hydrologic model a hydrologist's delusion? I believe the answer depends on the scale, the scope, and the purpose of the model. I agree with earlier commentators that physically based models are useful in understanding certain aspects of complex hydrologic systems. However, I also believe that they can be used in a predictive sense for small watersheds where the overland flow is generated by the Hortonian mechanism. Some studies, which purport to show that simpler models are superior to physically based models for prediction are severely flawed. At best we might say that for intermediate and large watersheds simpler models will give equally bad answers at a lower cost. For engineering purposes, we are usually interested in large events where the impact of spatial variability is not as important as for small events. For this reason we must be very careful in interpreting comparisons based on rather insignificant runoff events.

Carefully designed tests with carefully checked hydrologic data are extremely useful and should be encouraged. This is the only way that we can identify model shortcomings and try to improve them. The assumption that we can obtain accurate a priori estimates of all parameters of physically based runoff model using current techniques is incorrect. We know from field studies that there will be considerable uncertainty in some parameter estimates or measurements and that there are substantial small-scale variations in soil properties. However, we often have auxiliary data that can be used to check the reasonableness of parameter estimates and we may be able to obtain limited rainfall and runoff data to improve these estimates.

We must always retain a certain degree of skepticism regarding the predictions of physically based models (and those

of simpler models as well). We must remember that new labor-saving techniques to obtain watershed descriptions, such as geographic information systems (GIS), cannot improve model accuracy if the model structure is badly distorted or is being applied at an inappropriate scale.

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