1043

# Predicting Sediment Yield in Storm-Water Runoff from Urban Areas[a]

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The authors are to be commended for attempting to improve the methods of predicting sediment yield from urban watersheds. However, the discussers feel there are a number of problems with the approach taken.

In the section titled "Watershed Hydrology," the authors state that they developed simulated hydrographs using the unit hydrograph procedure. They state further that Soil Conservation Service (SCS) curve numbers (CNs) were used to estimate losses. They then go on to state that ". . . simulated hydrographs were separated into two hydrographs representing the contribution of storm-water runoff from the impervious area and the pervious area . . . Simulated hydrographs for the bare-soil areas were computed using the kinematic wave model." Further along they state, "The rainfall excess rate $I_e$ in (2) is computed by using the Green-Ampt [in]filtration equation." This is extremely confusing. Did the authors really use CNs with unit hydrographs? The kinematic routing discussions would lead the discussers to believe that the authors did not.

Justification should also be provided for the use of two different methods to treat infiltrating surfaces [curve numbers for pervious surfaces and Green-Ampt (Green and Ampt 1911) for bare-soil surface cover]. The Green-Ampt infiltration equations (Skaggs and Khaleel 1983) are capable of modeling both types of infiltrating surfaces and computing rainfall excess. Using both methods only confuses the presentation. But most importantly, because there are no interior runoff or sediment concentration data, one can only wonder what the justification is for the subdivision?

In the section titled "Sediment Yield Modeling of Impervious Areas," the authors assume that most (or all?) of the sediment is produced from the impervious area, a questionable assumption in our estimation although this may be true on small precipitation events. The assumption that the small plots (representing an area of 0.01 ha) can be used to determine parameter values for the four watersheds with drainage areas 1.2 ha $< A <$ 150 ha is certainly questionable. Are the authors implying that this could be done or was this procedure used? Although recognizing that this is done with some regularity, it is certainly not adequate for the largest drainage (i.e. for 150 ha). As the drainage area increases, the opportunity for additional water and sediment transport increases and the shear stresses increase in a nonlinear manner (Haan et al. 1982).

It would have helped if the authors had shown which of the plots were being simulated in Figs. 2–7 by cross referencing them to the simulation number in Table 1. Did the authors use an average of the two replications or does the result represent one replication? The discussers suspect that parameters were adjusted until hydrograph volumes matched those observed. Most of the hydrograph simulations look quite acceptable except for Fig. 5(b).

The results of the plot simulations might also be tainted because the authors do not state how the measured sediment yields were obtained in Figs. 2–7. One can only presume that they were total samples from the plot. With this in mind, how were the sediment concentration samples obtained in Figs. 8–11? Do they represent pumping sampler output (at a point in the flow depth) or are they somehow total load estimates from the entire cross section? Point samples from the side of a measuring device are notoriously low relative to the mean concentration for the entire station (unless only very fine particles are moving and the flow is well mixed) (Simanton et al. 1993). The agreement between the simulated and observed hydrographs look good. Are these best hydrographs, worst, or average results. The sediment concentration graphs (Figs. 8–11) are really not sediment yield graphs (as labeled). But more importantly, the few data points make it impossible to evaluate how good the sediment yields might be. Was any attempt made to compare average annual runoff and average annual sediment yield for simulated and observed? If so, what kind of agreement was obtained?

Figs. 12 and 13 imply that the sediment concentrations represent outwash from only the impervious areas. It was not clear that data was collected separately from pervious and impervious areas. Rather, we assume the sediment concentration was collected at the watershed outlet only. Thus, it is not clear how the impervious-area concentration was determined. The discussers think that the results shown in Fig. 13 are more serendipitous than real. The fact that sediment becomes more available with increasing time between storms may be conceptually adequate but not only because it comes from impervious areas. In other words, not all runoff comes from impervious areas nor does the assumption that all of the sediment comes from these areas make sense, especially because only between 39% and 86% of the watersheds were impervious. It is highly likely that the Hart Lane watershed had significant soil removed from pervious areas (unless you examined only small storm events).

Finally, the last sentence before "Conclusions" states that "For storm events with large antecedent conditions, only a small portion of the total runoff volume would need to be treated since most of the sediment washed off occurs during the first part of the storm event." The statement is partly true. Concentrations are almost always shaped like the exponential decay shown in Figs. 8–11. However, multiplying the concentration times the water discharge will in many cases show that a significant quantity of the total sediment yield also comes from other parts of the hydrograph-sediment discharge graph. Fig. 11 illustrates that 3 of the 4 samples of sediment concentration were collected prior to the hydrograph peak. Using the simulated concentration and water discharge in Fig. 11, we computed that 72% of the total event sediment discharge occurs during the period of the first three samples (0–30 min). This demonstrates that a substantial portion (28%) of the total sediment yield can occur after the initial portion of the storm event.

The discussers do not agree with the first section of the conclusions. Unless the discussers do not understand what the authors did, it has not been proved that ". . . separating an urban watershed into different land surfaces and representing each land surface individually, a better estimate can be obtained of the sediments in the storm-water runoff." This conclusion can only be justified with presentation of an actual urban watershed example in which runoff and sediment-yield results from each of the different land surfaces are examined. The verification examples appear to have been selected to isolate the particular hydrologic and sediment response for a given type of land surface and the discussers realize this is required to test the proposed methods for the individual sur-

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faces. For example, for the analyses related to sediment yield on impervious areas, four urban watersheds were selected with various percentages of impervious area. However, it appears that all were analyzed solely on the basis of impervious response. Was any attempt made to model the infiltrating portions of this watershed by the other methods for pervious surfaces proposed in the paper? As stated earlier, this was not apparent from the "Watershed Hydrology" section. This points out a major difficulty in application of the methods proposed. Because calibration was required in each of the methods, to apply these methods to a typical urban watershed, it would seem that both hydrologic and water-quality observations are available for each of the land-use areas in isolation. Having a single streamflow and water-quality observation point for a mixed-land-use watershed would not provide sufficient calibration information for the individual methods based on different land surfaces. If such data were available and calibration could be carried out, one must question the transferability of the calibrated model to other locations. Thus, the advantages expressed in the introduction of a "deterministic model" over "site-specific" regression type models are largely negated.

The discussers do agree: "It is very difficult to predict the initial amount of sediment that is present on a watershed at the time in which runoff occurs." Further, "the analysis showed that the buildup of sediments was dependent on antecedent time between storm events, . . ." Why would one expect to find that different land uses "affected the rate of sediment buildup?"

Given the aforementioned questions, it is difficult to evaluate the adequacy of the proposed model. Furthermore, it's difficult to assess whether the model postulated could be extrapolated to other watersheds (without additional runoff and sediment concentration data), or whether calibration data would be required prior to application to other urbanized watersheds. The limited sediment sample data certainly raises many questions regarding the sediment discharge graphs presented (the illustrations contain only a few samples for concentration data). The absence of information regarding the agreement (or lack thereof) between predicted and observed annual runoff and sediment yield do little more than raise uncertainty about the conceptual model framework.

## APPENDIX. REFERENCES


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