RUNOFF CURVE NUMBER: HAS IT REACHED MATURITY?[a]

Discussion by Bernard L. Golding,[3] Life Member, ASCE

The discusser fully concurs with the author's contention that the runoff curve number (CN) method has reached maturity if maturity can be measured by acceptability and worldwide usage.

However, the application of the CN number to urban basins must be done with a great deal of care, considerably more than now used by most public and private engineering organizations. Two areas where the discusser has observed problems in using the CN method by engineering organizations are subsequently discussed.

USING SINGLE COMPOSITE CN NUMBER

Based on the simulation of many rainfall/runoff events from urban basins, it was apparent to the discusser that to achieve good simulation the directly connected impervious area (DCIA) of a basin must be considered separately rather than assigning a single composite CN number to the entire basin, as is suggested in "Urban" (1986). The DCIA is that impervious area from which runoff enters the drainage (conveyance) system without passing over an area where infiltration can occur. Several models (*Drainage* 1989; *Advanced* 1990; *HEC-1* 1981) now allow the engineer to assign 100% runoff from these areas. However, it has been the discusser's observation that this is not generally done, which can result in considerable error.

CARELESSNESS IN DETERMINING CN NUMBER

As stated in the author's paper, the National Resource Conservation Service (SCS) has classified over 4,000 soils according to their hydrologic soils group. This information is available in the soil surveys published by the SCS for almost every county in the United States. The various soils and their assigned group are also listed in "Urban" (1986). However, it should be realized that these classifications were based on a fairly limited number of soil samples and should be used with care. Simply using the values in these publications without further investigation can lead to significant error. In a previous discussion of a paper by Prof. Hawkins, the discusser related a situation where substantial runoff occurred from a high, steep, sandy hill on a golf course when the use of a CN number from "Urban" (1986) indicated no runoff at all. Upon investigation, it was found that the golf course was covered with a thick compressed thatch of old grass cuttings that made the surface fairly impervious. In this discussion the discusser also advocated a change in the initial abstraction coefficient.

The discusser has also noticed that many mistakes have been made where the SCS has classified soils as being A/D and B/D, which are supposed to reflect a high ground-water table. However, urbanization of an area can change the height of the ground-water table considerably. Also, there is a tendency to use the A or B classification rather than the D classification to reduce cost or, as in a recent lawsuit case in which the discusser was an expert witness, make a point.

To ensure that the correct CN is used, Orange County, Fla., requires that soil classifications be done by a geotechnical engineer. Although this is an improvement, it is the discusser's experience that many of the geotechs have little or no knowledge of hydrology.

However, based on his experience, the discusser is an advocate of the CN method. Over the years, the discusser has tried many other methods of computing rainfall excess and found them all wanting. It is also his opinion that the CN method should be used in storm sewer design. Two models that do so are "Storm Sewer Analysis and Design Using Hydrographs," available from McTrans at the University of Florida, and *XP-SWMM* (1990). Certainly the CN number of a basin can be determined more intelligently than the rational method runoff coefficient for the same area.

APPENDIX. REFERENCES

Advanced interconnected channel and pond routing model. (1990). Streamline Technologies, Inc., Winter Park, Fla.

Drainage basin runoff model. (1989). Hilbern Engineering, Orlando, Fla.

HEC-1, flood hydrograph package, user's manual. (1981). U.S. Army Corps of Engineers, Washington, D.C.

"Urban hydrology for small watersheds." (1986). *Tech. Release No. 55,* Soil Conservation Service, USDA, Washington, D.C.

XP-SWMM. (1990). XP-Software, Tampa, Fla.

Discussion by Roger E. Smith[4]

The subject paper constitutes a rather thorough, albeit subjective, treatise on the widely used curve number (CN) runoff method, offering conceptual interpretations of some of its features and various arguments in support of its utility. The discusser feels it is necessary and useful to challenge or correct several of the claims made by the authors, with hopes of preventing more misuse or misinterpretation of this useful estimation procedure.

CONCEPTUAL BASIS OF CURVE NUMBER EQUATION

The authors raise more confusion than light, it would seem, in their attempt to legitimize the CN procedure from a hydrologic perspective. The section on "Modes of Surface Runoff Generation" is an excellent 500-word summary of hydrologic processes, but it is quite unclear which of those processes the authors believe that the CN method is supposed to represent, or if they believe it to represent all of the listed mechanisms. This is quite relevant to their criticism of other runoff methods as "unbounded," i.e., without limit on the amount of rain the soil can infiltrate. Conceptually, hillslope processes, where runoff is controlled by a shallow, relatively impervious layer above which a perched water table may form, could have a physically related limitation on the value of S.

The CN method, however, as pointed out by the authors, "is an infiltration loss model," and had its origin in the Midwest, based on small watershed and plot data related to surface infiltration-controlled processes, not hillslope subsurface-controlled processes. For infiltration-controlled hydrology the argument against "bottomless" equations seems rather academic on the one hand, since storm depth is never unlimited, and physically meaningless on the other hand, since a soil profile of some 10-m depth would normally have a CN S value not larger than about 20 cm.

Another argument raised by the authors against "other

---

[a] January 1996, Vol. 1, No. 1, by Victor M. Ponce and Richard H. Hawkins (Paper 11094).

[3] Consulting Water Resour. Engr., 8982 Istesworth Ct., Orlando, FL 32819.

[4] Res. Hydr. Engr., ARS-USDA, AERC Colorado State Univ., Fort Collins, CO 80523.

methods'' is that they do not ''account(s) for the physical, chemical, and biological aspects . . . and include(s) all relevant hydrologic processes.'' There is no evidence that the CN method satisfies these criteria. The claim for the one-parameter CN as ''responsive'' and the proposed advantage of only one parameter seem to redefine the idea of responsiveness. The discusser asserts that a ''responsive'' equation would have identified distinct, independent causative factors as sources of runoff variation, and would have a measurable parameter for each factor. The CN or any other one-parameter equation merely requires that all physical variation be reflected in the value of that one parameter, which to me does not mean it is responsive.

After such subjective treatment of the CN methodology, near the end the authors do make a fundamentally important statement about the hydrology of the CN equation: ''The method works in the mean, which precludes it from being perfectly predictive.'' It is important to recognize that it ''works'' only in the mean. Any plot of actual data on a *P-Q* diagram will show an enormous amount of scatter in the sector where $Q < P$. CNs found from real *P-Q* data using (17) will vary enormously—from 45 to 95 is not atypical (Smith 1992; Montgomery 1980). Figs. 1 and 2 exemplify the variability in $Q(P)$ relations from two hydrologically different small watersheds (W-1, Moorefield, W. Va., 3.3 ha, 880 events; and R-5, Chickasha, Okla., 9.6 ha, 539 events). (In Fig. 2 the shaded area is bordered by the AMC I and AMC III CN relations.) On each figure are data for all runoff events as well as for the annual maximum storms, to illustrate the fact that scatter around the CN relation (or any simple relation) is not significantly reduced by such selectivity. Note that, despite the hydrologic differences between the Moorefield and Chickashaw catchments, the CN parameter values are similar.

It should be emphasized that the CN method is a nonlinear regression estimate for runoff using only the total storm depth value, and may represent a quite reasonable mean relationship for $Q(P)$. In fact, it benefits in this regard from the law of large numbers, insofar as the fit of real data to the relationship improves with aggregation, e.g., estimating monthly runoff from monthly rainfall has less scatter than for daily values.

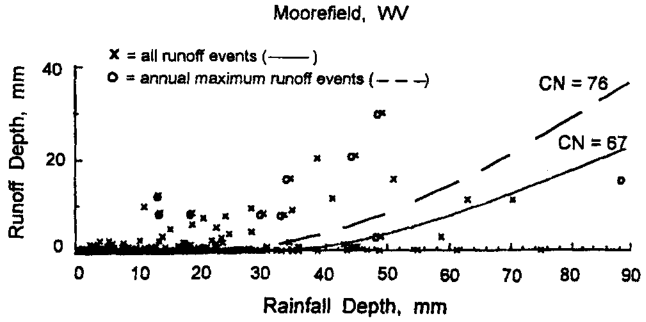

FIG. 1. Storm Rainfall/Runoff Data and Overall CN Relations for Watershed W-1 at Moorefield, W. Va.

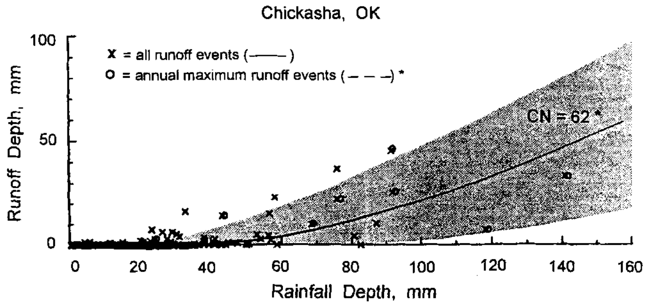

FIG. 2. Storm Rainfall/Runoff Data and Overall CN Relations for Watershed R-5 at Chickasha, Okla.

The cause of storm CN variability is an important misconception repeated by the authors and deserves comment. Although in one place this variability is given four sources by the authors, they go on to state that antecedent moisture content (AMC) is the major source of variability, and discuss no others. (The authors later conclude that a major disadvantage of the CN method is ''the absence of clear guidance on how to vary antecedent moisture.'') While antecedent soil moisture can be important, it drops quite rapidly near the surface after storms in most cases, and thus the antecedent moisture for most storms has a far lower variability than does the storm CN. However, AMC is not the major source of variability. In a detailed study of the AMC on 14 watersheds, Clopper (1980) demonstrated that adjusting the CN by use of the AMC (as recommended by the SCS) actually increased the runoff error of estimate by about 1 or 2 mm compared to using a fixed AMC II. While the range included between the AMC I and III (Fig. 2) is broad, this result indicates that, at least for these watersheds, the AMC itself (statistically) does not explain the scatter in runoff data.

In many climates a more important source of variability is the rainfall intensity and its pattern within the storm, and this cannot be accounted for by the CN methodology. The authors essentially ignore this. Although not as significant for a shallow-soil hillslope runoff mechanism, the intensity of the storm is critical to the amount of Hortonian infiltration and runoff, such that a 15-mm storm can produce about any amount of runoff from 0 to perhaps 13 mm, depending only on the rate at which the rain falls. This is clearly established by watershed data as well as infiltration theory, and cannot be accounted for by the CN method. A CN derived for a watershed will inherently reflect the particular storms for which it was fitted (annual series, etc.). In addition, if it does represent an expected value relation, it will reflect the rainfall intensity statistics for that region as well as the catchment conditions.

SURVIVAL BASIS OF CURVE NUMBER METHODOLOGY

None of the preceding is to argue that, given only rainfall depth and requiring an estimate of the runoff, there is a significantly better method for practical use. However, it serves neither science nor engineering to ignore the fact that we know far more about the physics of runoff production than is enshrined in the CN. As Figs. 1 and 2 indicate, actual data seldom justify any superiority of a CN relation compared to a straight-line relation (Montgomery 1980). What is needed is a recognition that rainfall rates and soil hydraulic property data are required if improved watershed runoff estimates are to be made, or if hydrographs are to be accurately estimated (Goodrich et al. 1995; Faurès et al. 1995). Also needed is a general recognition that the CN at best represents a long-term expected value relationship.

Most of the complicating factors listed by the authors (crusting, shrinkage, entrapped air, and root structure) can in fact be reflected (none perfectly) by physically related infiltration expressions, contrary to the authors' statements. Certainly, the CN method offers nothing in response to those complications by lumping al types of effects into one parameter and attributing all temporal variability to changes in AMC.

There is an over-riding reason for the acceptance of the CN method, however, that should be admitted by all engineers, i.e., that it is a method in a publication of, and supported by, a U.S. government agency that gives its users basic protection in case of litigation. Anyone dubious of the significance of this rationale has not worked in consulting engineering. Thus, until a better method is produced with equal government agency support, the CN will continue to be used due to the backward-looking nature of our legal system. Beyond that, there is habit and tradition; it is used because suggested values for CNs have been tabulated in government publications, thus making it easy to use as well as providing legal shelter. Whether those table values and the CN assumptions are relevant to a given engineering objective is a question not often enough asked. Those of us in government are responsible for supporting the advancement of hydrologic engineering beyond this method. As the opinions of the authors illustrate, it is not an easy task.

It is doubtful, finally, that the question posed in the title is answered in the paper, or even if the question of "maturity" is the relevant question to ask. The authors have provided a subjective review rather than an objective analysis. Part of what is at stake here is belief rather than fact. It remains the comfortable belief of many that the CN is a measurable watershed property and that S is related to real soil-water storage. There is also a beauty in the simplicity of the CN that has an appeal much like that of mythology.

APPENDIX. REFERENCES

Clopper, P. E. (1980). "Antecedent moisture considerations in the SCS curve number rainfall-runoff method," MS thesis, Colorado State Univ., Fort Collins, Colo.

Faurès, J. M., Goodrich, D. C., Woolhiser, D. A., and Sorooshian, S. (1995). "Measurement and analysis of small-scale convective storm rainfall variability." *J. Hydro.*, 173, 283–308.

Goodrich, D. C., Faurès, J. M., Woolhiser, D. A., Lane, L. J., and Sorooshian, S. (1995). "Impact of small-scale spatial rainfall variability on runoff simulation." *J. Hydro.*, 173, 309–326.

Montgomery, R. J. (1980). "A data-based evaluation of the SCS curve number method for runoff prediction," MS thesis, Colorado State Univ., Fort Collins, Colo.

Smith, R. E. (1992). "Opus, an integrated simulation model for transport of nonpoint-source pollutants at the field scale. Vol. 1, Documentation." *Rep.*, USDA-ARS, ARS-98.

Discussion by Gene E. Willeke,[5] P.E., Member, ASCE

The authors have rightly stated that the runoff curve number method is well-established. The reasons for its popularity, however, perhaps need restatement to some degree. It is convenient, simple, and seems to consider the important catchment properties of soil type, land use/treatment, surface condition, and antecedent conditions. Its authoritative origins, however, and the authenticity of how these catchment properties are considered deserve some discussion.

The method is authoritative only in the sense that a number of federal, state, and local agencies use it; and have endorsed it for use in certain procedures. This makes it sufficiently authoritative to qualify as a defense in legal proceedings that a practitioner has performed a hydrologic analysis in accordance with generally accepted standards.

In a more important sense, however, there is serious question about whether it should be considered authoritative. The normal standard we apply in determining whether a procedure is authoritative is whether it has withstood peer review in its treatment of the internal logic of the model, of the data used to calibrate or validate the model, and the results it yields when applied to cases within its purported scope. This has, in fact, never been done.

In the 1950s, when it was issued in the *National Engineering Handbook*, there was strong resistance to allowing outside review of the procedure. The method purports taking into account a wide variation in soil types and catchment conditions. Only a small number of people have ever seen that data, and it does not appear either in the literature or in files available to researchers. The authors allude to this lack of review and ask whether the procedure is obsolete, a remnant of outdated technology, and in need of overhaul or outright replacement.

The logic of the method depends on the factors included and the way they are incorporated into the model. The procedure has in its favor a certain intuitive appeal and a general belief that the catchment properties included seem to be going in the right direction. Soil conditions for which the surrogate is soil type affect infiltration. Antecedent conditions certainly have an effect on moisture absorption. Land use/treatment and surface condition are more debatable, except at the extremes, but are plausible factors affecting surface runoff. The magnitude of the effect, as displayed by changes in curve number, is almost certainly overstated. The ways in which the factors have been included have often been criticized (Pilgrim and Cordery 1993).

Apart from the logic of the model, the more serious concern is whether it works well enough to justify trust in its conclusions. The studies that have been done do not give either a practitioner or a researcher much confidence. The method has not performed well on individual storms. Among the many examples, Wood and Blackburn (1984), quoted in Pilgrim and Cordery (1993), report "differences between observed and computed flood peaks of greater than ±50% in 67% of the cases in Nevada, Texas, and New Mexico." After citing a number of such studies, Pilgrim and Cordery (1993) conclude that "all of the above results cast some doubt on the accuracy and validity of the SCS method."

One can only conjecture why the original data and analyses were never subjected to external scrutiny and review. It is based on obsolete views of the hydrologic cycle, accords more prominence to land treatment than seems to be merited, and was based on a very limited data set. The profession has suffered from this for more than 40 years, in the sense that designs of important structures have been based on analyses performed by the runoff curve number method that have little chance of being very accurate. The structure of the method is such that it probably could not survive rigorous analysis, and a new attempt should be made to devise a method that more closely replicates what happens in the real world. Our database is so much better than it was, as are our understanding of hydrology and our computational methods, that it seems inexcusable to continue usage of the runoff curve method as it is.

The evidence presented by the authors seems to point clearly toward positive answers to the questions they raised early in their paper. Yes, the method is obsolete; yes, it is a remnant of outdated technology; and it is in desperate need of overhaul or outright replacement. The authors ask whether the runoff curve number method has reached maturity. Sadly, the answer seems to be that it has grown old, but has not reached maturity.

APPENDIX. REFERENCES

Pilgrim, D. H., and Cordery, I. (1993). "Flood runoff." *Handbook of hydrology*, D. R. Maidment, ed., McGraw-Hill Inc., New York, N.Y.

Wood, M. K., and Blackburn, W. H. (1984). "An evaluation of the hydrologic soil groups as used in the SCS runoff method on rangelands." *Water Resour. Bull.*, 20, 379–389.

---

[5] Dir. and Prof., Inst. of Envir. Sci., Miami Univ., Oxford, OH 45056.

Closure by Victor M. Ponce[6] and Richard H. Hawkins,[7] Members, ASCE

The writers thank the discussers for their interest and thoughtful comments. Golding's concern regarding the applicability of the curve number (CN) method to sites with unusual land use/treatment is shared by the writers, as is his belief that the CN method is a better predictor of hydrologic abstraction than the rational method. His disappointing experience with alternative methods of hydrologic abstraction reinforces the writers' conviction that no method currently available is sufficiently holistic and able to account effectively for spatial and temporal variability.

Smith's call for more sophisticated and responsive methods is noble, but it sidesteps a basic quandary: whether increased sophistication is likely to lead to increased accuracy or utility, at all scales. Smith's assertions to the contrary, the question of the usefulness of "bottomless" equations, is neither academic nor physically meaningless. Soil profiles have finite depths, and these are subject to filling, especially for the extreme rainfalls that characterize design storms. Notwithstanding its origins in agricultural hydrology, the CN method is commonly applied to a wide variety of settings, including urban lands and natural hillslopes with shallow soils.

Smith argues that the CN method suffers from the same lack of holism as its more sophisticated counterparts. Our intention was not to compare the holism of alternative methods, but rather to stress the limitations of all methods with respect to this issue. Smith is correct in reiterating the essence of the CN method: to reduce the variability in estimating runoff response based on fitting Mockus' nonlinear equation to annual flood data. The method's developers saw fit to vary the only parameter of the method (the CN with its related "S" value) as a function of four readily identifiable descriptors: soil type, land use/treatment, hydrologic condition, and antecedent moisture. All other possible sources of variability, some of them intractable, were lumped into the fourth descriptor ("antecedent moisture"). As pointed out by Smith, this practice limited the method in certain cases; however, simplicity was retained.

Willeke questions whether the CN method has withstood adequate peer review. While his point may have some merit, it is now moot. The method has been in use for more than 40 years, and peer review at this juncture would seem rather extemporaneous. Willeke's assertion that the method has not performed well with individual storms is essentially correct. The counterargument is that the method was never intended to match individual storms.

The discussions serve to underscore a seldom-appreciated virtue of the CN method: that it gives substance to several larger yet unresolved issues of rainfall-runoff hydrology. Some of these are the following: (1) Does "S" exist in all cases, i.e., are nonlinear asymptotic forms of the P-Q equation applicable in every setting; (2) can land-use effects on runoff be effectively quantified; (3) what is the relative importance of rainfall intensity factors as compared to antecedent moisture; (4) what is acceptable as open/peer review of technical methods to be used in the profession; and (5) does complex necessarily mean better? While the CN method's simplicity makes it quite transparent, it also makes it easy to critique.

These questions would exist with valid content in the absence of the CN method. The latter merely gives form and example to them, serving as a lightning rod for general rainfall-runoff hydrology. Perhaps this is its greatest asset.

To close, the writers would like to reiterate that the CN method works in the mean, in a statistical sense, by reducing the possible variability in runoff estimates within curved bands (in P-Q space) described in terms of CNs, themselves surrogates for a conceptual parameter referred to as potential retention. The method sits at the base of a hierarchy of hydrologic abstraction models, all of which suffer from lack of holism and the practical inability to effectively account for spatial variability. As Smith has adroitly pointed out, given only rainfall depth, and requiring an estimate of runoff, there is no significantly better method for practical use than the CN method.

---

[6] Prof., Civ. and Envir. Engrg. Dept., San Diego State Univ., San Diego, CA 92182-1324.

[7] Prof., Watershed Resour. Program, School of Renewable Natural Resour., Univ. of Arizona, Tucson, AZ 85721.