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[a]Discussion period closed for this paper. Any other discussion received during this discussion period will be published in subsequent Journals.

Trickle systems operating at low pressures of the order of 5 ft (1.5 m) are generally considered not practical since a relatively small change in pressure due to friction loss or topography creates a relatively large change to correct operating pressure of the emitter. Consequently, the emitter discharge is significantly affected. For example, a 250-ft (75-m) long pipeline on relatively level ground could quite possibly have a rise of 1 ft (0.3 m), and therefore, the operating pressure of the emitter is reduced by 20% which will significantly affect the uniformity of the emitter discharge.

The saving in operating costs using low-pressure trickle systems operating at 5 ft (1.5 m) pressure as compared with high-pressure trickle systems operating at 35 ft (11.0 m) is of the order of $1 (Australian) per acre (0.4 ha) per yr, which is considered a relatively small cost to pay for a more uniform and therefore a more efficient irrigation system.

The design methods for trickle irrigation systems must incorporate the practical aspect of changes in ground elevation. Presumably this fact will become apparent when the authors continue their valued work and perform field design and analysis. In addition, discharge variations obtained under Australian conditions are relatively small and most of the inherent problems with this type of irrigation have been solved. High-pressure systems are more practical and more efficient than low-pressure systems. There also exists valid economical and practical reasons why various diameter pipes should be used so that the pipeline friction loss balances the gain in pressure due to a fall in ground level. The result is a constant pressure at all emitters which in turn permits the use of only one type of emitter throughout the whole scheme.

Appendix—Reference

8. Read, A. L., "Australia's Largest Trickle Irrigation Development," Experts Panel on Irrigation, Herzliya Pituach, Israel, Sept., 1971.

Subsurface Hydrograph Analysis by Convolution[a]

Discussion by Roger E. Smith,[3] A. M. ASCE

The authors have presented a two stage convolution method to reproduce subsurface hydrographs, which allows introduction of a specific form of nonlinearity into the unit-hydrograph method. The "analytical" method employed by the authors, in which one constructs a mathematical model for a system (as elegantly as possible) that reproduces the measured output of the system according

[a]September, 1972 by Willard M. Snyder and Loris E. Asmussen (Proc. Paper 9213).

[3]Hydr. Engr., Southwest Watershed Research Center, ARS-USDA, Western Region, Tucson, Ariz.

to some criteria of the analyst, is one which has been useful for many engineering problems. This is an empirical approach in that one only has a model of what has happened, and yet one can with some confidence, extrapolate to unmeasured analagous situations.

One should not fail to recognize, however, the quantum step taken when an attempt is made to assign physical (or "quasiphysical") meaning to the mathematical parameters of the model. The authors point to their intent to find such meaning for their model parameters; apparently, however, the technique as applied to surface hydrology (8,9), has yet to find physical relevance. It seems illusory, in fact, to seek "physical meaning" for parameters of a mathematical model developed by abstract analysis of (estimated) input and output traces, in an attempt to justify its use. Such justification should come through demonstrated parameter predictability for a range of system states in a more comprehensive test than is indicated in this paper.

The unit hydrograph (response function) as a model of a watershed has a well-defined physical analogy to a linear system such as a series of linear storage elements. Even this does not help in the determination of parameters for ungaged watersheds. It has been used for watersheds as an approximation—both surface and subsurface hydraulic phenomena are generally nonlinear. The authors treat nonlinearity as input and output regressive variations in a convolution construction of the unit response function. The writer submits that there is little reason to think that this model has physical meaning. Consider the following:

1. Eleven parameters are determined for each of the storms presented, based only on: (a) The measured hydrograph; and (b) an estimate of the input. The authors rightly emphasize the crudeness of the estimation procedure used, but do not stress the sensitivity of the parameters to the estimated input distribution. There is reason to expect serious bias in the procedure from use of a "maximum rate" of infiltration, which inverts well substantiated knowledge that infiltration capacity generally decays throughout a (continuous) storm, apparently asymptotic to a minimum. The "effective" rainfall determined in Fig. 6 (for September 21, 1969) is a good example of the illogic of the procedure—*no* rain is estimated to have entered the soil in the first *14 hr* of the storm. One might speculate at this point that the authors' concept of "effective rainfall" is related to vertical flux at the water table. In this case they have eliminated the most complex part of the subsurface region, but have defined a model whose input is beyond practical measurement.

2. The response function is obtained by convoluting an exponential function with a gamma-like function (defined by discrete points, parameters 4 to 11). Readers familiar with the mathematics of probability will recognize that the same response function could be obtained by convoluting a simple gamma function with a similar but different discretely defined "characteristic" function. The gamma function parameters could still be obtained as in Eq. 3. From this one could argue that many equally valid ways could be devised to vary the response function with input and output state. Given that we wish to allow an input, or output-based variation, or both, in the response function, is there indeed any physical reason why this should be done by a convolution of two functions?

3. The authors consider the parameter values (Table 3) as "reasonably consistent," although most vary over a factor of 3 or more. The easiest test

of "reasonableness," consistent with the objective of this procedure, is a trial prediction of one of the measured hydrographs with the average values of parameters 4 thru 11. Such a prediction test is conspicuously missing. Also missing is a hydrograph analysis for a double peaked event, and more seriously, a sensitivity test on assumed pattern of input.

At the present state of development of this approach, one would not consider incorporating water quality modeling. The procedure presented might have application in a context in which the functional nature of the system was not understood. It seems scientifically indefensible, however, to dismiss the knowledge available concerning flow in porous media systems in construction of a model for the authors' purpose. Simple comparison of computer times (perhaps now out-of-date) misses the point entirely—one should compare, among other things, what is learned in each case. An example of a perhaps opposite approach is a recent paper by Freeze (10). Construction of more workable models by simplification from such numerically complex models as Freeze has used would surely better serve the commendable objectives of the authors. It is hoped that this discussion will also help the authors improve their approach toward those objectives.

Appendix—Reference

10. Freeze, R. A., "Role of Subsurface Flow in Generating Surface Runoff. 2. Upland Source Areas," *Water Resources Research*, Vol. 8, No. 5, Oct., 1972, pp. 1272-1283.

Potential United States Water-Supply Development[a]

Discussion by Vít Klemeš[2]

The author makes a good case for the statistically based assessment of the low-flow augmentation potential of storage reservoirs and shows how it can be meaningfully used in water resources planning and development. The main value of the material presented is, as the author himself recognizes, in facilitating comparisons between regions and in showing the limits of the development of surface water resources rather than in establishing their locally optimal development level. Although implicit in the paper, the preceding point should probably have been emphasized more for it is likely that the results will be interpreted in the latter sense by some readers.

The writer has the following comments concerning the methodology of the storage-draft-risk of failure analysis.

[a]September, 1972, by Clayton H. Hardison (Proc. Paper 9214).

[2]Research Hydro., Inland Waters Directorate, Environment Canada, Ottawa, Ontario, Canada.