Communication: Reply to Fox and Schruben

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When the standard deviation is relatively larger than the mean demand, as in Table 3, the decisions and costs are somewhat more sensitive to the form of the distribution. This is mostly notable in comparing distributions U and W, when the desired availability percent (and hence the corresponding unit cost of shortage) is relatively very high. In this extreme case, for zero leadtime, when the true distribution is W, but one assumes that it is U and makes the decisions $s = 5$ and $S = 13$, then the actual cost is \$16.20, as compared with the minimum cost of \$11.20. Conversely, if the true distribution is U and one assumes that it is W and makes the decision $s = 7$ and $S = 8$, the actual cost is then \$20.60, as compared with the minimum cost of \$15.79. But these seem to be very extreme cases that one would rarely encounter in practice.

A more realistic comparison would be to look at systems U and V, when leadtime is 3 periods and the availability percent 99. If the true distribution is V and one assumes it is U and uses the decisions $s = 21$, $S = 31$, then the actual cost is \$26.37, compared with the minimum cost of \$25.63 per period. Similarly, if U is the true distribution and one assumes that it is V and makes the decision $s = 19$ and $S = 27$, then the actual cost is \$29.99, as compared with the minimum cost of \$29.14 per period.

The insensitivity to the form of the demand distribution is even more dramatic when one also considers maximum demand in addition to the mean and standard deviation of demand. That is, if two different distributions have identical means, standard deviations, and maximum demands, then the optimal decisions in these systems are usually identical. And even when they are different, the effect on costs is negligible.

The author invites readers to send to him inventory systems for sensitivity analysis. In each case the parameters and the discrete distribution of demand should be given.

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In the recent issue of *Management Science* Fox (1978) expressed doubts about the relevance of *steady-state* behavior. In a reply Schruben (1978) disagreed on this point. With Fox he was in favor of publishing *failures* of variance reduction techniques (VRT's). I would like to add the following remarks.

1. Steady-State Behavior

Interest in steady-state behavior is mainly academic, i.e., in academic research simulation is often used to develop and validate analytical models; for case studies I refer to Ignall et al. (1978). These analytic models concentrate on steady-state behavior. In my opinion this emphasis is based solely on mathematical convenience. Limiting asymptotic distribution theory can be used for steady-state analysis;

transient analysis is much more difficult; see Kotiah (1978). Consequently many simulation studies have been performed with the very practical (!) aim of assisting theoretical studies. Note that in steady-state simulation studies we are confronted with questions such as: how long to continue a run; when is the transient phase over; how to initialize a run?

In practical simulation studies there is usually no interest in steady-state behavior; start-up and end effects do form part of the relevant output. Let me illustrate this statement with a few examples:

a. A bank or hospital clinique considered as a queuing system, opens in the empty state and closes its doors at, say 5 P.M. Interest may be in total throughput (number of clients).
b. Other queuing systems never close down, e.g., highway crossings and telephone exchanges. Inputs may be modeled as a Poisson process with varying traffic intensity parameter, say, λ_t. The simulation may study the handling of peak traffic: queues build up before and during rush hours, and decrease thereafter.
c. In practice simulation is used very often in a very simple way: Planning models for business and national-economic systems are simulated over, say, the next five years for different policies (what if approach). The simulation starts from the most recently known system state. Relevant outputs may be total profit over the next five years (including start-up effects), minimum employment over that planning period, etc.

Note that in these practical examples there are no difficult theoretical problems of initialization, runlength, and transient versus steady-state. The irrelevance of steady-state behavior may be emphasized by a quote from the famous economist, Keynes: "In the long run we all are dead."

2. Variance Reduction Techniques

Though I devoted my doctoral dissertation to the problem of VRT's, I have become very pessimistic about the practicality of these techniques. More relevant for saving computer time is the efficiency of the software: fast random number generators, fast sampling procedures for, say, exponential variates (slow logarithmic transformation), efficient user programs, etc. VRT's can be of practical relevance, if they are very simple. Examples are:

a. Common random numbers: Comparing different system configurations using the same random number seeds is straightforward, and often applied by practitioners. However, practitioners tend to neglect the analysis of their (expensive) simulation output. Statistical analysis becomes much simpler if the observations are independent instead of (positively) correlated.
b. Antithetic variates: Repeated runs may be initialized with antithetic seeds, say, $(1 - r_0)$. The analysis remains simple if the average of two antithetic runs is taken as "the" observation.
c. Control variates: The simulation output may be adjusted for deviations of the sampled input from its theoretical expectation. This adjustment can be based on familiar regression analysis.

There is a category of simulations that would benefit very much from variance reduction, namely, systems where the output depends on "rare" event such as excessive waiting lines or inventory stockouts. In such simulations nothing of interest happens during most of the simulated time. Importance sampling and virtual measures have been developed to improve the efficiency of such simulations. Unfortunately, my own experience with these techniques, applied to a complicated practical situation (a telephone exchange) turned out to be very disappointing: much mental energy was spent and the variance did not decrease at all; see Hopmans and Kleijnen (1978).

Readers interested in more details about steady-state simulation, variance reduction, etc., are referred to Kleijnen (1978).

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Notes[*]
VII

ON "FURTHER RESULTS ON PLANNING HORIZONS IN THE PRODUCTION SMOOTHING PROBLEM"[†]

ROBERT E. JOHNSON[‡] AND JOHN O. McCLAIN[§]

(INVENTORY/PRODUCTION—PLANNING HORIZONS: INVENTORY/PRODUCTION—PRODUCTION SMOOTHING)

In a recent paper [1], Lee and Orr present some results based on a study of the Modigliani-Hohn (M-H) production/inventory model. There are some ambiguities and errors in the paper which, when corrected, diminish the value of its principal results.

1. In §1 of [1] a planning horizon is defined, but the definition given is that of a *strong* horizon; i.e., one that is unchanging regardless of future conditions. As discussed in Modigliani and Hohn [2, p. 64] and Lieber [3, p. 325] the planning horizon in the M-H model is a *weak* horizon, which is dependent on knowing bounds on the demand beyond the horizon period. The Wagner-Whitin algorithm, on the other hand, provides a strong horizon. The authors fail to draw this distinction in footnote 1 of [1, p. 490], where they associate the M-H and Wagner-Whitin models. §5 of [1, p. 498], also lacks the clear distinction between weak and strong horizons.

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