Proceedings of the 1989 Winter Simulation Conference
E.A. MacNair, K.J. Musselman, P. Heidelberger (eds.)

OPTIMIZATION USING COMMON RANDOM NUMBERS, CONTROL VARIATES AND MULTIPLE COMPARISONS WITH THE BEST

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ABSTRACT

This paper considers the problem of determining the best of a finite number of system designs by simulation experimentation when the criterion of interest is maximum or minimum expected performance. This is a special case of the general problem of optimization via simulation. The proposed method is based on multiple comparisons with the best (MCB), due to Hsu, which constructs simultaneous interval estimates for the difference between the expected performance of each system design and the best of the other designs. We propose a refinement of Hsu's procedure through the use of two variance reduction techniques, common random numbers and control variates, that are particularly useful in simulation experiments. We show that the proposed procedure is better than standard MCB in the sense that it is more sensitive to differences in expected performance.

1 INTRODUCTION

One of the primary uses of stochastic simulation is to compare alternative system designs, often in terms of expected performance since actual performance is subject to random variation. Frequently the goal is to find the system design having maximum or minimum expected performance. When the number of alternative system designs is finite and not too large, there are two standard approaches for solving this optimization problem: ranking and selection and multiple comparisons. Ranking and selection procedures yield a decision (e.g., which system design has maximum expected performance), while multiple-comparison procedures provide estimates (e.g., the difference between the expected performance of each system design and the best of the other system designs). This paper describes an approach based on multiple comparisons.

Multiple-comparison procedures provide confidence intervals for specified differences in expected performance, confidence intervals that are guaranteed to be simultaneously correct with a prespecified probability. There are a number of multiple-comparison procedures, and the appropriate one depends on the comparisons of interest. Hsu and Nelson (1988) describe multiple comparisons with the best (MCB), which is particularly useful when the goal is to find the system design having maximum or minimum expected performance. This paper presents a refinement of MCB.

Since multiple-comparison procedures provide interval estimates of differences in expected performance, an interval that contains zero when the true difference is not zero indicates there is insufficient evidence in the simulation data, relative to the variance of the estimators, to declare the performance of the two systems to be different. The refinement we present uses variance reduction techniques—common random numbers and control variates—to decrease estimator variance, and thus increase the sensitivity of MCB to small differences in expected performance. The primary contribution of this paper is to extend the use of common random numbers to simultaneous estimation of several differences, a longstanding problem in simulation output analysis.

The next section presents a simple example that illustrates the type of problem for which MCB is useful. Section 3 reviews MCB, and motivates the need for variance reduction. Section 4 introduces the refined MCB procedure; section 5 compares the performance of the new MCB procedure to standard MCB on the example. Section 6 offers some discussion.

2 EXAMPLE

The example is an (s, S) inventory model taken from Koenig and Law (1985). This section and the next is based on Hsu and Nelson (1988).

An (s, S) inventory system is one in which the level of inventory of some discrete item is reviewed periodically. If the inventory level is found to be below s units, then enough additional inventory is ordered to bring the inventory level up to S units. When the inventory position at a review period is found to be above s units, no additional items are ordered. Different (s, S) combinations correspond to different "system designs."

Let $\{I_t; t = 1, 2, \ldots\}$ be the inventory position just after a review at period t. Orders are filled immediately, so $I_t \in \{s, s+1, s+2, \ldots, S\}$. Let $\{D_t; t = 1, 2, \ldots\}$ be a stochastic process representing the demand for units of inventory in period t. The inventory position I_t changes in the following way: $I_{t+1} = S$ if

Table 1: Parameters and Expected Cost for Inventory Example

i	s	S	θ_i
1	20	40	114.18
2	20	80	112.74
3	40	60	130.55
4	40	100	130.70
5	60	100	147.38

$I_t - D_t < s$, or $I_{t+1} = I_t - D_t$ if $I_t - D_t \geq s$. We assume that $I_1 = S$ and $\{D_t; t = 1, 2, \ldots\}$ is a sequence of i.i.d. Poisson random variables with common mean 25. Under these assumptions $\{I_t; t = 1, 2, \ldots\}$ is a Markov chain.

In each period there are costs associated with the inventory position. If $I_t - D_t < s$, then in period $t + 1$ a cost of $32 + 3(S - (I_t - D_t))$ is incurred, which is a fixed cost plus a per unit cost of bringing the inventory position up to S. In period $t + 1$, if $I_{t+1} \geq D_{t+1}$, then a holding cost of $I_{t+1} - D_{t+1}$ dollars is incurred; otherwise a shortage cost of $5(D_{t+1} - I_{t+1})$ dollars is incurred.

Let C_t^i be the cost incurred in period t under policy i, where "policy" means an (s, S) combination. The quantity of interest is the expected average cost of the inventory system for 30 periods under several (s, S) policies, with a smaller expected total cost being preferred. The five policies considered by Koenig and Law are given in Table 1.

Let

$$\theta_i = \mathrm{E}\left[\frac{1}{30}\sum_{t=1}^{30} C_t^i\right]$$

be the expected average cost for policy i. The values of θ_i given in the table, which were taken from Koenig and Law (1985), can be obtained using standard Markov chain analysis. Of course, in a practical problem these values would not be known, but knowing them here facilitates evaluating MCB procedures.

3 MCB

Suppose that $r \geq 2$ system designs are to be compared in terms of their expected performance, and denote the expected performance of the ith system by $\theta_i, i = 1, 2, \ldots, r$. If finding the system with the largest mean performance is of interest, then $\theta_i - \max_{\ell \neq i} \theta_\ell, i = 1, 2, \ldots, r$ are the appropriate parameters to estimate, since if $\theta_i - \max_{\ell \neq i} \theta_\ell > 0$, then system i is the best system; otherwise, it is not the best system. In minimization problems, such as the (s, S) inventory example, the parameters of interest are $\theta_i - \min_{\ell \neq i} \theta_\ell$, $i = 1, 2, \ldots, r$, since if $\theta_i - \min_{\ell \neq i} \theta_\ell < 0$ then system i is the best system. Of course, minimization problems can be transformed into maximization problems by changing the sign of all terms. Hsu's (1984) method of MCB, which we describe next, provides simultaneous confidence intervals for $\theta_i - \max_{\ell \neq i} \theta_\ell$, for all i. By the nature of multiple comparisons, the fewer the number of statements that must be simultaneously correct, the sharper the inference. If maximum or minimum expected performance is most important, then MCB, which gives simultaneous confidence intervals for r parameters, is typically superior to procedures that provide confidence intervals for the $r(r-1)/2$ differences $\theta_i - \theta_\ell$ for all $i \neq \ell$.

Let Y_{ij} be the jth simulation output from the ith system design and suppose $\theta_i = \mathrm{E}[Y_{ij}]$ for all j. MCB is applicable if the balanced oneway model (1) pertains:

$$Y_{ij} = \theta_i + \varepsilon_{ij} \tag{1}$$

for $i = 1, 2, \ldots, r$ and $j = 1, 2, \ldots, n$, where $\varepsilon_{11}, \varepsilon_{12}, \ldots, \varepsilon_{rn}$ are independent $N(0, \sigma^2)$ random variables with σ^2 unknown. Model (1) implies that n independent replications are generated from each system, and the systems are simulated independently of each other. In the inventory example Y_{ij} would be the average cost for 30 periods of the ith (s, S) inventory policy on the jth replication.

Let $\theta_1, \theta_2, \ldots, \theta_r$ be estimated by the sample means

$$\bar{Y}_i = \frac{1}{n}\sum_{j=1}^{n} Y_{ij},$$

for $i = 1, 2, \ldots, r$, and let σ^2 be estimated by the pooled sample variance

$$\hat{\sigma}^2 = \frac{1}{r(n-1)}\sum_{i=1}^{r}\sum_{j=1}^{n}(Y_{ij} - \bar{Y}_i)^2.$$

The constants n and r, and the random variables $\bar{Y}_1, \ldots, \bar{Y}_r$ and $\hat{\sigma}^2$, are the inputs to MCB.

Let $d^{\alpha}_{r-1,r(n-1)}$ be the upper α quantile of a random variable that is the maximum of $r - 1$ equally correlated multivariate-t random variables with correlation 1/2 and $r(n-1)$ degrees of freedom, and let $+x^+ = \max\{0, x\}$ and $-x^- = \min\{0, x\}$. For model (1), Hsu (1984) showed that the closed intervals

$$\left[-\left(\bar{Y}_i - \max_{\ell \neq i} \bar{Y}_\ell - d^{\alpha}_{r-1,r(n-1)}\hat{\sigma}/\sqrt{n}\right)^-, \right.$$
$$\left. +\left(\bar{Y}_i - \max_{\ell \neq i} \bar{Y}_\ell + d^{\alpha}_{r-1,r(n-1)}\hat{\sigma}/\sqrt{n}\right)^+\right]$$

for $i = 1, 2, \ldots, r$ are $(1-\alpha)100\%$ simultaneous confidence intervals for $\theta_i - \max_{\ell \neq i} \theta_\ell$, for all i. A detailed proof is given in Hsu and Nelson (1988).

To apply MCB to the inventory example, let C_t^{ij} be the cost in period t of (s, S) policy i on the jth replication. Then

$$Y_{ij} = \frac{1}{30}\sum_{t=1}^{30} C_t^{ij}.$$

That is, Y_{ij} is the average cost of 30 periods of operation under policy i on replication j. For model (1) to be tenable, the experiment must be designed so that, for fixed policy i, Y_{ij}, $j = 1, 2, \ldots, 30$ are i.i.d., and Y_{ij} are independent for all i and j. In practice, this means that different random number streams are used to generate demand values for the simulation of each policy. Subroutine rnpoi from the IMSL Library was used to generate demands from the Poisson distribution in this simulation.

We are interested in simultaneous confidence intervals for $\theta_i - \min_{\ell \neq i} \theta_\ell$, $i = 1, 2, \ldots, 5$. That is, the difference between the expected average cost of each policy and the least expected average cost of the other policies. Figure 1, which is reproduced from Hsu and Nelson (1988), shows the confidence intervals from one experiment with $n = 30$ replications and $\alpha = 0.05$. The numerical values are given in Table 2.

With confidence level 0.95, policies 3, 4 and 5 are not the best since the lower endpoint of their intervals is 0, meaning that the difference between the expected cost of each of these policies and the other policy with the least expected cost is greater than or equal to 0. Although policy 2 appears to be the best, we cannot conclude that it is the best since the intervals for policies 1 and 2 contain 0. The 95% upper confidence bound for $\theta_2 - \min_{\ell \neq 2} \theta_\ell$ indicates that policy 2 may be worse than the true best policy by as much as 1.267. Stated differently, the random variation in $\bar{Y}_1, \ldots, \bar{Y}_5$ is too large relative

Table 2: Example MCB Result for Inventory Problem

i	$\bar{Y}_i$	$\bar{Y}_i - \min_{\ell \neq i} \bar{Y}_\ell$	interval
1	114.043	1.046	(-1.267, 3.359)
2	112.998	-1.046	(-3.359, 1.267)
3	131.055	18.057	(0.0, 20.370)
4	131.749	18.751	(0.0, 21.064)
5	146.715	33.717	(0.0, 36.030)

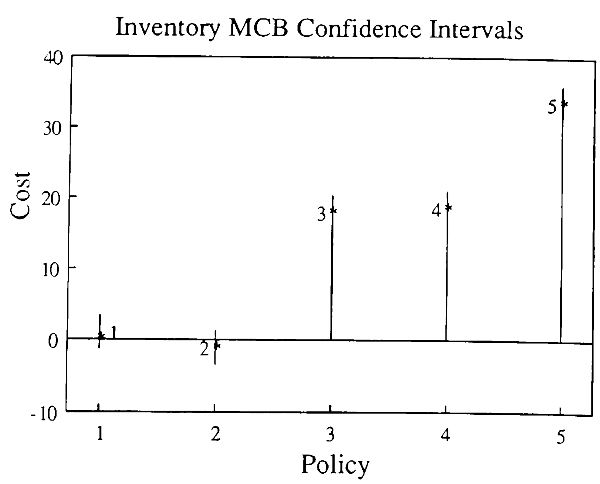

Figure 1: MCB Intervals for Inventory Problem

to the differences $\theta_i - \min_{\ell \neq i} \theta_\ell$ to determine the best (s, S) inventory policy. Increasing n would reduce the variability, but at additional computation cost. And, if the simulation experiment has already been performed, then the experiment must be repeated, or at least restarted, to increase n. In the next section we introduce a refinement of MCB that reduces variability without increasing n.

4 MCB AND COMMON RANDOM NUMBERS

In stochastic simulation experiments the response variable, Y_{ij}, often has a strong linear relationship with certain input random variables that drive the simulation experiment. Suppose the response variable can be described by the following model:

$$Y_{ij} = \theta_i + \boldsymbol{\beta}_i'(\mathbf{X}_j - \boldsymbol{\mu}) + \eta_{ij} \tag{2}$$

for $i = 1, 2, \ldots, r$ and $j = 1, 2, \ldots, n$, where $\eta_{11}, \eta_{12}, \ldots, \eta_{rn}$ are independent $N(0, \tau^2)$ random variables with τ^2 unknown; $\mathbf{X}_1, \mathbf{X}_2, \ldots, \mathbf{X}_n$ are i.i.d $q \times 1$ vectors of input random variables with known mean vector $\boldsymbol{\mu}$; $\boldsymbol{\beta}_i$ is a $q \times 1$ unknown constant vector; and $'$ indicates the transpose of a matrix.

Recall that in model (1), $\text{Var}[Y_{ij}] = \sigma^2$, for all i (different systems). In contrast, model (2) implies that $\text{Var}[Y_{ij}] = \tau^2 + \boldsymbol{\beta}_i'\boldsymbol{\Sigma}_{\mathbf{X}}\boldsymbol{\beta}_i$, where $\boldsymbol{\Sigma}_{\mathbf{X}} = \text{Var}[\mathbf{X}_j]$, and these terms are not necessarily equal for all i. However, model (2) assumes a linear relationship between Y_{ij} and $\mathbf{X}_j$, while (1) does not specify any such relationship between the simulation inputs and outputs.

Let $\theta_1, \theta_2, \ldots, \theta_r$ be estimated by the *control-variate estimators*

$$\hat{\theta}_i = \bar{Y}_i - \hat{\boldsymbol{\beta}}_i'(\bar{\mathbf{X}} - \boldsymbol{\mu}),$$

for $i = 1, 2, \ldots, r$, and let τ^2 be estimated by

$$\hat{\tau}^2 = \frac{1}{r(n-q-1)} \sum_{i=1}^{r} \sum_{j=1}^{n} [Y_{ij} - \hat{\theta}_i - \hat{\boldsymbol{\beta}}_i'(\mathbf{X}_j - \boldsymbol{\mu})]^2$$

where

$$\bar{\mathbf{X}} = \frac{1}{n} \sum_{j=1}^{n} \mathbf{X}_j$$

and

$$\begin{aligned} \hat{\boldsymbol{\beta}}_i &= \mathbf{S}_{\mathbf{XX}}^{-1} \mathbf{S}_{\mathbf{X}Y_i} \\ &= \left[\frac{1}{n-1} \sum_{j=1}^{n} (\mathbf{X}_j - \bar{\mathbf{X}})(\mathbf{X}_j - \bar{\mathbf{X}})' \right]^{-1} \\ &\quad \times \left[\frac{1}{n-1} \sum_{j=1}^{n} (\mathbf{X}_j - \bar{\mathbf{X}})(Y_{ij} - \bar{Y}_i) \right]. \end{aligned}$$

Notice that we have assumed that the $\mathbf{X}_j$, called the *control variates*, follow identical distributions across system designs; i.e., they are independent of i. Thus, if we use *common random numbers (CRN)* to generate these inputs then the control variates are *identical* across systems. For example, in the (s, S) inventory problem different inventory policies result in different system designs, but the demand on each system is independent of the inventory policy simulated. If the total demand during the planning horizon, $X = \sum_{t=1}^{30} D_t$, is the control variate then CRN results in the same total demand for each inventory policy. The η_{ij} term in model (2) represents sources of variation that are not explained by the linear relationship or cannot be made identical across systems through the use of CRN; they must be independent for model (2) to be tenable. In practice this means that different random number streams must be assigned to different systems for those input processes that cannot be made identical using common random numbers (see section 6 for further discussion of this point).

Let $\hat{\delta}^2 = n^{-1} + (n-1)^{-1}(\bar{\mathbf{X}} - \boldsymbol{\mu})'\mathbf{S}_{\mathbf{XX}}^{-1}(\bar{\mathbf{X}} - \boldsymbol{\mu})$. The constants n, q and r, and the random variables $\hat{\theta}_1, \ldots, \hat{\theta}_r$, $\hat{\tau}^2$ and $\hat{\delta}^2$, are the inputs to the new MCB procedure given in Theorem 1.

Theorem 1 *Assuming model (2) holds,*

$$\left[-\left(\hat{\theta}_i - \max_{\ell \neq i} \hat{\theta}_\ell - d^{\alpha}_{r-1, r(n-q-1)} \hat{\delta}\hat{\tau} \right)^-, \right.$$
$$\left. + \left(\hat{\theta}_i - \max_{\ell \neq i} \hat{\theta}_\ell + d^{\alpha}_{r-1, r(n-q-1)} \hat{\delta}\hat{\tau} \right)^+ \right]$$

for $i = 1, 2, \ldots, r$, *are simultaneous* $(1-\alpha)100\%$ *confidence intervals for* $\theta_i - \max_{\ell \neq i} \theta_\ell$ *for all* i.

The proof is given in Yang (1989). Critical to the proof is the fact that under model (2) the conditional variance of $\hat{\theta}_i$ given the control variates is $\delta^2\tau^2$, so that using common random numbers for the control variates causes $\hat{\delta}^2$ to be common for all i.

5 EVALUATION

The event that the MCB confidence intervals contain the true differences $\theta_i - \max_{\ell \neq i} \theta_\ell$ for all i, and, at the same time, do not contain 0 (except as an endpoint) when $\theta_i \neq \max_{\ell \neq i} \theta_\ell$ is important since it implies both identifying a difference and the direction of the difference. This could be called correct and useful inference. When model (2) holds and n is not too small, $\hat{\theta}_i$ is a better point estimator of θ_i than $\bar{Y}_i$ in terms of smaller variance (Lavenberg and Welch 1981). Yang (1989) shows that when n is not too small the expected length of the MCB intervals using CRN and control variates is shorter than the expected length of the standard MCB intervals. Thus, when both MCB procedures achieve the nominal coverage probability $1-\alpha$, we expect the new procedure to have a larger probability of correct and useful inference.

In this section we compare the new procedure to the standard MCB procedure, in terms of the probability of correct and useful inference, by simulation experiments on the inventory example. Of course, this is only an illustration on a single example. A more complete experimental evaluation is given in Yang (1989).

Table 3: Estimated Probability of Coverage (C) and Probability of Correct and Useful Inference $(C\&U)$ for $\alpha = 0.05$ (standard error of estimates ≤ 0.02)

n	$\Pr\{C\}$	$\Pr\{C\}$	$\Pr\{C\&U\}$	$\Pr\{C\&U\}$
	MCB	MCB+CRN	MCB	MCB+CRN
10	0.97	0.97	0.09	0.19
20	0.96	0.97	0.17	0.31
30	0.95	0.96	0.26	0.46
40	0.96	0.98	0.31	0.58
50	0.96	0.96	0.39	0.68
60	0.93	0.97	0.47	0.77
70	0.94	0.96	0.52	0.84
80	0.94	0.98	0.57	0.88
90	0.94	0.98	0.60	0.92
100	0.94	0.98	0.68	0.95

The coverage probability and the probability of correct and useful inference were estimated for the new and standard MCB procedures for $n = 10, 20, 30, \ldots, 100$ replications at the $\alpha = 0.05$ level by repeating the entire experiment 700 times. For the standard procedure each inventory policy was simulated independently (different random number streams were used to generate demands under each (s, S) policy), while for the new procedure CRN (a single random number stream) was used to make demands identical for each policy. IMSL subroutine drgivn was used to compute the control-variate estimators via least-squares regression.

Table 3 gives the results, which show that both procedures appear to have coverage at least 95%, but the new procedure dominates the standard MCB procedure in terms of the probability of correct and useful inference. Notice that in both cases the probability of correct and useful inference is significantly lower than the coverage probability unless n is large, which further emphasizes the value of variance reduction. As $n \longrightarrow \infty$ the probability of correct and useful inference converges to the coverage probability.

6 DISCUSSION

When the linear relationship (2) holds, we suspect that the equal residual variance assumption of model (2) is often less severely violated than the corresponding equal variance assumption for model (1), because model (1) is a special case of model (2) with $\boldsymbol{\beta}_i = \boldsymbol{\beta}_\ell$ for all i and ℓ. Thus, if the linear relationship holds, the assumption of model (1) is stronger than that of model (2) in the sense that it implies that the dependence between the response variable and the control variates is the same for all systems.

On the other hand, if the linear relationship (2) does not hold then the control-variate point estimators are biased (Nelson 1988). Since multiple-comparison procedures construct estimates of differences, however, the bias of the estimators may cancel out. Thus, multiple-comparison procedures based on control-variate estimators are expected to be robust to deviation from the linearity assumption.

CRN, as it is typically applied, reduces $\mathrm{Var}[\bar{Y}_i - \bar{Y}_\ell]$, provided it induces positive correlation between $\bar{Y}_i$ and $\bar{Y}_\ell$ for $i \neq \ell$. Thus, we would also expect CRN to reduce $\mathrm{Var}\left[\bar{Y}_i - \max_{\ell \neq i} \bar{Y}_\ell\right]$. This variance reduction is achieved without the necessity of assuming either models (1) or (2), and it could be that $\mathrm{Var}\left[\bar{Y}_i - \max_{\ell \neq i} \bar{Y}_\ell\right] < \mathrm{Var}\left[\hat{\theta}_i - \max_{\ell \neq i} \hat{\theta}_\ell\right]$, since CRN can be used on all input processes in the former case, but—in our formulation—may not be used on all input processes in the latter. However, it is difficult to construct interval estimators based on $\bar{Y}_i - \max_{\ell \neq i} \bar{Y}_\ell$ under CRN—except by using very conservative methods such as the Bonferroni inequality—because the correlation between $\bar{Y}_i$ and $\bar{Y}_\ell$ is unknown.

The limitation in our formulation is that the control variates $\mathbf{X}_j, j = 1, 2, \ldots, n$ must assume identical values across systems. This assumption may not be necessary. The assumption implies equal conditional variance of the control-variate estimators for different systems; therefore, the appropriate quantiles (e.g., $d^{\alpha}_{r-1, r(n-q-1)}$) are easily calculated. In general we only need to know the ratios of the variances of estimators for different systems; equality is not required. More precisely, we only need equal residual variances for different systems under model (2), and estimators with a diagonal correlation matrix, in order to compute appropriate quantiles (Hayter 1989, Edwards and Hsu 1983). Extensions in this direction are under investigation.

ACKNOWLEDGMENT

This research was partially supported by National Science Foundation Grant No. ECS-8707634.

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