Proceedings of the 2015 Winter Simulation Conference
L. Yilmaz, W. K. V. Chan, I. Moon, T. M. K. Roeder, C. Macal, and M. D. Rossetti, eds.

DELAY TIMES IN AN M/M/1 QUEUE: ESTIMATING THE SAMPLING DISTRIBUTIONS FOR THE STEADY-STATE MEAN AND MSER TRUNCATION POINT

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ABSTRACT

MSER is a method for determining the length of the warm-up period needed to mitigate systematic error in the estimate of the steady-state mean of an output resulting from the arbitrary initialization of a simulation. While a considerable corpus of empirical and theoretical research supports the effectiveness of MSER on a range of test problems, it has been suggested recently that MSER may fail to delete a significant amount of highly biased data for some simulation models (Law, 2015). One example given in support of this suggestion addresses the delay time in an M/M/1 queue for different initial conditions. We expand this example, applying replication/deletion to develop point estimates, confidence bounds, and approximations to the sampling distributions for both MSER-truncated mean and the MSER truncation point. We illustrate that the suggestion is not supported by this example.

1 INTRODUCTION

Conway (Conway et al., 1959; Conway, 1963) first recognized that the arbitrary selection of initial conditions introduces bias in the estimation of simulation output statistics such as the steady-state mean. Alternate solutions to the start-up problem have been advanced for over a half-century. Recently, however, a consensus has emerged that truncation of the initial output sequence using MSER is effective and efficient at mitigating bias, robust across alternate forms of biasing functions, easily understood and easily computed, and does not require specification of unknown parameters.

MSER was developed initially by McClarnon (1990), White and Minnox (1994), and White (1997) and was applied and extended by Rossetti et al. (1995), Spratt (1998), Cobb (2000), White et al. (2000), and Franklin (2009). Mahajan and Ingalls (2004) determined three truncation criteria adequate, with MSER-5 recommended for its efficiency and robustness. Oh and Park (2006) compared their EVR method "with the method MSER-*m* known as the most sensitive rule in detecting bias and most consistent rule in mitigating its effects." MSER was shown to outperform EVR in almost all experiments. Sandikci and Sabuncuogy (2006) automated MSER-5 as their means for studying transients. Bertoli, Casale, and Serazzi (2007, 2009) selected MSER-5 as the initialization approach for their Java Modeling Tools package and included a usage wizard. The criterion gained additional traction with an exhaustive empirical evaluation by Hoad et al. (2008, 2011), who chose MSER-5 as the most suitable method for automation over a wide range of published approaches to the transient problem, including heuristics, graphical procedures, initialization bias tests, statistical methods, and hybrid approaches.

White and Franklin (2010) confirmed the empirical findings of White and Robinson (2010) regarding the relationship between the MSER truncation point and the degree of mean bias and autocorrelation in an output sequence. They applied a parametric approach to analyze the expected behavior of MSER on an output model with geometrically decaying bias and constant-parameter AR(1) white noise. Franklin et al. (2009) explored the intuition that MSER minimizes the mean squared error (MSE) of the mean estimator. This empirical result was confirmed in theory (under mild assumptions) by Pasupathy and Schmeiser (2010, 2014), who showed that the MSER statistic is asymptotically proportional to the MSE, reasoned that MSE is the most appropriate criterion for evaluating alternate truncation criteria, and concluded that the MSER statistic is a solid foundation for initial-transient algorithms. Pasupathy and Schmeiser also suggested two new algorithms using the MSER statistic and compared these to the original MSER algorithm using empirical results for M/M/1 and AR(1) processes. Mokashi et al. (2010) compared their N-Skart method with MSER-5 and achieved only modest improvements with considerably greater computational effort. Taylor (2010) explored effectiveness of MSER using the commercial transportation simulation model VISSIM and Hoad and Robinson (2011) considered the practical implementation of MSER-5. Sánchez and White (2011) developed a weighting scheme for replication means to correct for unequal sample sizes when MSER is applied within a replication/deletion framework. Nelson (2013) and Law (2015) describe MSER in their simulation texts.

In this paper, our motivation, in part, is to illustrate the common-sense observation repeated by Snell and Schruben (1985), White and Robinson (2010a, 2010b), Pasupathy and Schmeiser (2010), and Franklin et al. (2011), among others. The purpose of an initialization procedure is to yield the best estimate achievable with the output data at hand, where "best" is defined in terms of both the accuracy and precision of the estimate. Assessing the value of a data-driven initialization procedure by the comparing an average truncation point to a theoretical expectation requires an appropriately large number of replications.

To this end, we examine the performance of MSER initialization by estimating the steady-state mean of the delay time in an M/M/1 queue from within a replication/deletion framework. Using a range of initial conditions, we estimate the sampling distributions of both the MSER-truncated mean and the MSER truncation point and develop confidence bounds on the corresponding estimates. We also demonstrate that, under the conditions simulated, the point estimates of the mean are statistically independent of both initial conditions and the corresponding MSER-5 truncation point. We show that while the mean estimates for this problem are quite good without truncation, applying MSER-5 truncation modestly improves the accuracy of these estimates.

2 MSER

Consider the stochastic process $\{Y_i,\ i=1,2,\ldots\}$. Our goal is to estimate the steady-state mean $E[Y]$ from the output of r simulation replications $\{Y_{i,j}\!: i=1,2,\ldots,n;\ j=1,2,\ldots,r\}$, where (without loss of generality) the length of each replication is n and the number in system at index $i=0$ is s. When Y_i is a tally variable, we compensate for arbitrary initialization using the weighted grand-mean of the truncated means (Sanchez and White, 2011, 2013)

$$E[Y] \approx \overline{\overline{Y}}\left(n, d_1, d_2, \cdots, d_r \middle| s\right) = \frac{1}{r}\sum_{j=1}^{r} \overline{Y}_j\left(n, d_j \mid s\right) = \frac{1}{N}\sum_{j=1}^{r}\sum_{i=d_j+1}^{n}\left(y_{i,j} \mid s\right)$$

as the estimator, where $N = \sum_{j=1}^{r}(n-d_r)$ is the total number of observations reserved after truncation across all runs. MSER initialization determines the optimal truncation point for each replication as

$$d_j^* = \underset{n>>d_j\geq 0}{\arg\min}\left\lfloor MSER(n, d_j \mid s)\right\rfloor$$

where the MSER statistic is the square of the estimated standard error of the mean $MSER(n,d_j \mid s) = \tilde{S}E^2_{\bar{Y}}(n,d_j \mid s) = S^2_{\bar{Y}}(n,d_j \mid s)/n$ obtained using the large-sample variance

$$S^2_{\bar{Y}_j}\left(n,d_r \middle| s\right) = \frac{1}{\left(n-d_r\right)^2}\sum_{i=d+1}^{n}\left(Y_{i,j}-\bar{Y}_j\left(n,d_r \middle| s\right)\right).$$

MSER-*k* applies this same scheme to the batch averages $Z_{i,j} = \frac{1}{k}\sum_{l=(i-1)k+1}^{ik} Y_{l,j}$ instead of the original observations $Y_{i,j}$.

3 THE EXAMPLE

Now consider the stochastic process $\{Y_i,\ i=1,2,\ldots\}$, where Y_i is the delay time of the i^{th} customer in an M/M/1 queue with a traffic intensity of $\rho=0.9$. The objective is to estimate the mean of the steady-state delay time $E[Y]$ from a set of simulation experiments under alternative assumptions for initial conditions, run length, and MSER-*k* batch size. This is the problem explored by Law (2015) in Examples 9.2 and 9.28, where the true steady-state mean is given as $E[Y]=8.1$.

Law's first experiment tests the sensitivity of the MSER-optimal truncation point, d^*, to the number in system at index $i=0$, for initial conditions $s=0, 5, 10, 12, 15, 18, 20$. MSER-5 is applied to the averaged sequence $\left\{\bar{Y}_i, i=1,2,\ldots,m\right\}$, where $\bar{Y}_i = \sum_{j=1}^{n} Y_j$, for $n=5$ independent replications, each with run length $m=65 \times 10^3$. The choice of these parameters was based on the recommendations made by Hoad (2009). The second experiment tests the sensitivity of d^* to m using initial subsequences of the previous sequence of lengths 1×10^3, 5×10^3, 10×10^3, 20×10^3, and 40×10^3 observations. The final experiment tests the sensitivity of d^* to the MSER batch size k by repeating the first experiment using MSER-1 instead of MSER-5.

Kelton and Law (1985) previously developed theory and computational algorithms for this problem. Following Gafarian et al. (1978), they considered the observations yielding 1% and 5% settling times as potentially suitable candidates for defining an optimal truncation point. For a 1% setting time, $E[Y]$ was shown to settle after something less than 800 observations for $s=0$. This falls to a minimum of something less than 200 observations for $s=15$ and then rises rapidly as s continues to increase. For a 5% setting time, $E[Y]$ settles after something less than 400 observations for $s=0$. This falls to a minimum of something less than 100 observations for $s=13$ and then rises rapidly as s continues to increase.

To assess the effectiveness of MSER, Law compares computed truncation points from his experiments to the theoretical transients for the process $E[Y_i]$ described by Law and Kelton. For every initial condition in every experiment, the *total* number of initial observations deleted is 15 or 16. This is far short of the several hundred observations predicted by reference to theory. Based on these (and other) experiments, Law concludes that, "it appears that MSER may fail to delete a significant amount of highly biased data for some simulation models." It is important to note that values of $\bar{Y}(m,0)$ (the naïve estimate without truncation) and $\bar{Y}(m,d^*)$ (the MSER-optimal estimate) are not reported and there is no assessment of the quality of the MSER estimate in terms of accuracy, precision, or potential improvement in the estimate of $E[Y]$ achieved without truncation.

To extend the first experiment, we make $r=100$ replications for each of the eight initial conditions $s=0, 5, 10, 12, 15, 18, 20$, and 40. We derive d_j^* for *each* replication (rather than the average across runs), as well as the means of the original and truncated series, $\bar{Y}(n,0)$ and $\bar{Y}(n,d_j^*)$, respectively. This allows us to (1) develop confidence bounds on the estimates in the context a replication/deletion approach, as would be used in practice; (2) compare approximations to the sampling distribution for each value of s; and (3) explore the relationships among d_j^*, $\bar{Y}(n,0)$, and $\bar{Y}(n,d_j^*)$.

Note that we intentionally do not apply common random numbers in this experiment—all 800 runs are independent. We seek to provide results on as many different sample paths as possible. Further, given the demonstrated homogeneity of the results across initial conditions, this choice allows us to combine all the data sets into one grand experiment, as discussed in Section 4.4.

4 RESULTS

4.1 Truncated Means: Sampling Distributions and Confidence Bounds

The box-and-whisker plots in Figure 1 show the variation in mean estimates across replications for each initial condition. There is modest skew to the right, which appears to be the result of a comparatively small number of large outliers. This modest skew also seems intuitive, given that observations of the state are bounded from below and unbounded from above. *Note that while some of the initial conditions are associated with negative bias and some with positive bias, all of the outliers are greater than the true mean of 8.1 min, irrespective of the sign of the initial bias.*

In contrast, there does not appear to be significant variation in the estimates across initial conditions. The mean data for each initial condition were fit to standard distributions, as shown in Figure 2. As one might anticipate for the sampling distribution of a mean, these fits were all approximately normal, with the best fits (using the AIC and BIC) being shifted lognormal or loglogistic to account for the skew. Again, these appear to be essentially independent of the initial conditions, as would be expected if MSER were working as intended. Approximately 80% of truncated means for every replication were within ±10% of the true mean for all s.

As shown in Table 1, the grand means for the corresponding replication/deletion experiments are strongly consistent, all within ±0.75% of the true mean. The corresponding 95% confidence intervals all cover the true mean. The error in the mean is negative for the two negatively biased initial conditions; the error in the mean is both positive and negative for the five positively biased initial conditions. These results appear to support the claim that the error is the result of sampling and not initial bias.

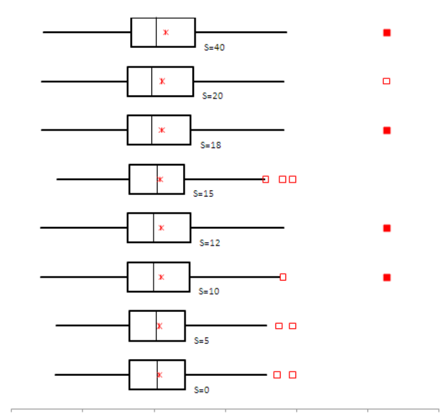

Figure 1: Comparison of box-whisker plots of the truncated means for eight initial conditions.

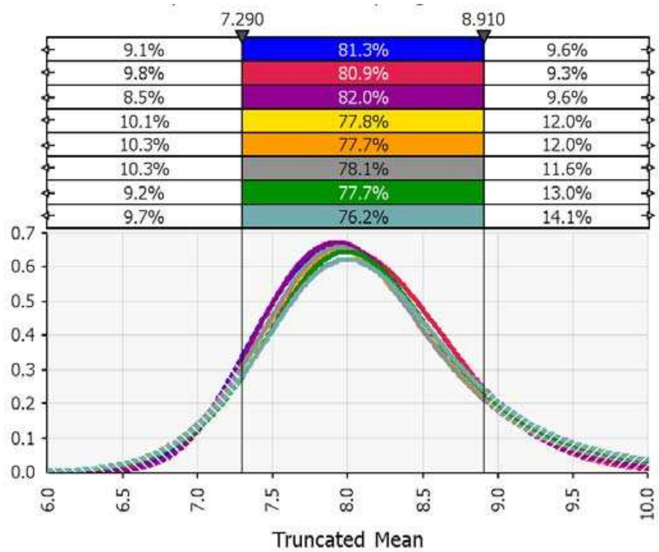

Figure 2: Comparison of sampling-distribution fits for eight initial conditions.

Table 1: Comparison of 95% confidence intervals for the truncated mean for eight initial conditions.

	s=0	*s*=5	*s*=10	*s*=12	*s*=15	*s*=18	*s*=20	*s*=40
Sample Mean	8.0716	8.0746	8.0997	8.1008	8.0798	8.1073	8.1116	8.1611
Lower Limit	7.9474	7.9505	7.9557	7.9565	7.9567	7.9626	7.9665	8.0152
Upper Limit	8.1957	8.1987	8.2437	8.2450	8.2030	8.2520	8.2567	8.3070
Sample Std Dev	0.6259	0.6253	0.7257	0.7269	0.6207	0.7292	0.7313	0.7353
Lower Limit	0.5496	0.5490	0.6372	0.6383	0.5449	0.6402	0.6421	0.6456
Upper Limit	0.7271	0.7264	0.8431	0.8445	0.7210	0.8471	0.8495	0.8542

4.2 Truncation Points: Sampling Distributions and Confidence Bounds

The box-and-whisker plots in Figure 3 show the extreme variation in the mean estimates across replications for each initial condition. The skew to the right also is extreme. Figure 4 shows the distribution of the MSER-optimal truncation points for each of the eight initial conditions. These distributions appear to be hyperexponential, although we did not attempt formal fits. Little or no truncation is indicated for the vast majority of replications. Figure 5 shows that, for the first seven initial conditions, only 30-40% of the replications required any truncation at all. This changes for the most extreme initial condition, s=40, for which all but one replication was truncated. In contrast, for two replications (one with s=5 and one with s=15), truncation was in excess of d=23,500.

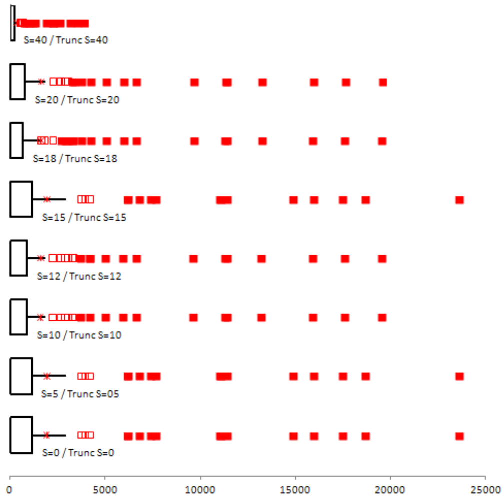

Figure 3: Comparison of box-whisker plots of the truncated points for eight initial conditions.

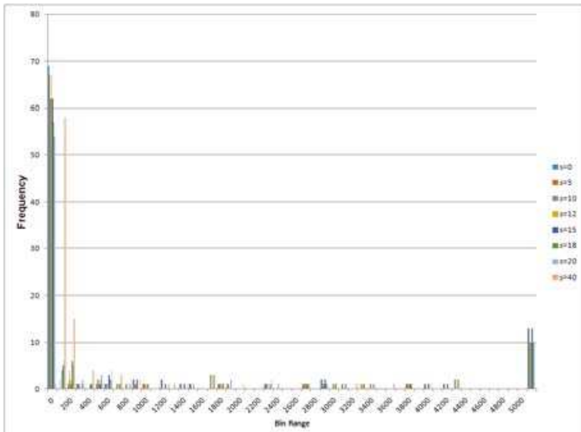

Figure 4: Comparison of distributions of the truncation points for eight initial conditions.

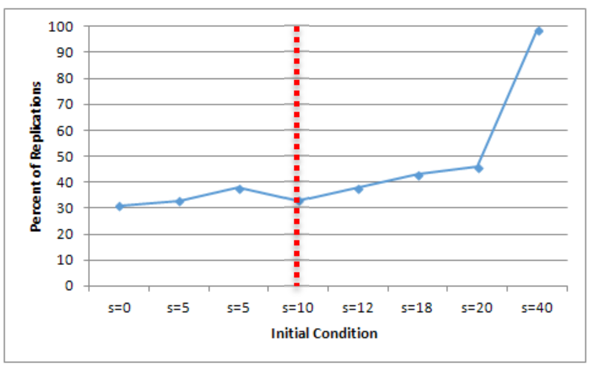

Figure 5: Percentage of replications requiring truncation (d_j*>0) as a function of the initial condition.

Table 2: Comparison of 95% confidence intervals for the truncated point for eight initial conditions.

	s=0	*s*=5	*s*=10	*s*=12	*s*=15	*s*=18	*s*=20	*s*=40
Sample Mean	1928.65	1934.90	1601.30	1610.85	1941.15	1612.35	1625.90	360.90
Lower Limit	1034.67	1041.34	844.03	854.19	1047.78	855.84	870.19	212.01
Upper Limit	2822.63	2828.46	2358.57	2367.51	2834.52	2368.86	2381.61	509.79
Sample Std Dev	4505.44	4503.33	3816.48	3813.37	4502.41	3812.62	3808.62	750.38
Lower Limit	3955.80	3953.96	3350.89	3348.17	3953.14	3347.51	3343.99	658.84
Upper Limit	5233.85	5231.41	4433.51	4429.90	5230.33	4429.03	4424.38	871.70

4.3 Truncation Points vs. Mean Estimates

Figure 6 shows the scatterplot of the truncated mean estimates vs. truncation points for eight initial conditions. Figure 7 shows the same scatterplot with the zero truncation points removed the truncation points plotted using a log scale and. These scatterplots illustrate important point. The linear correlation coefficients between the magnitude of the truncation points and corresponding error in the mean estimates for the different initial conditions are on range [0.289 > ρ > -0.058]. *That is, knowing nothing but the truncation point, one cannot say anything about the quality of the corresponding estimate on any replication.*

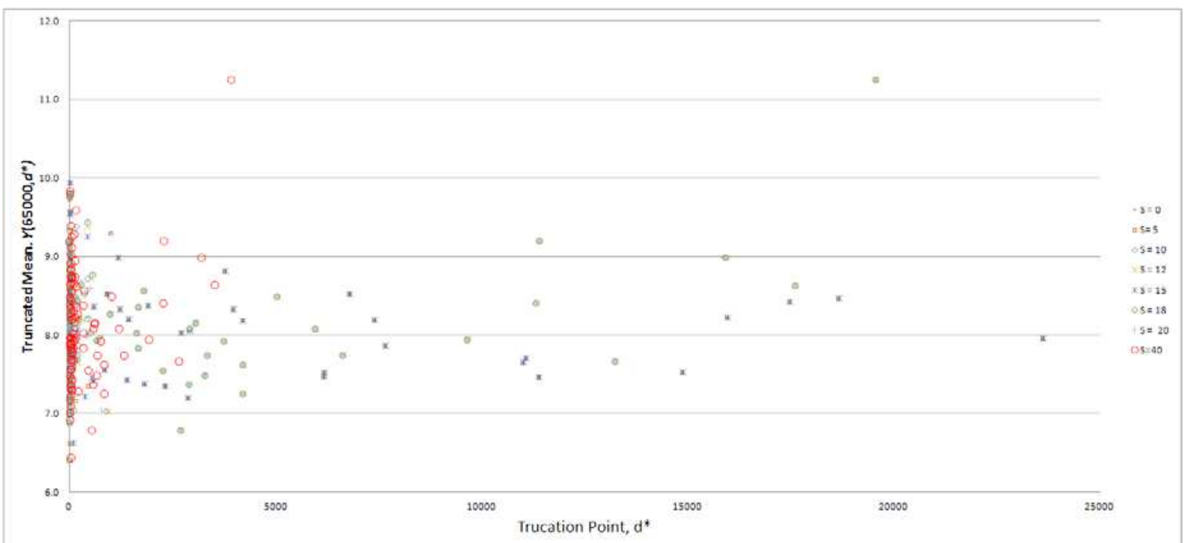

Figure 6: Scatterplot of the truncated mean estimates vs. truncation points for eight initial conditions.

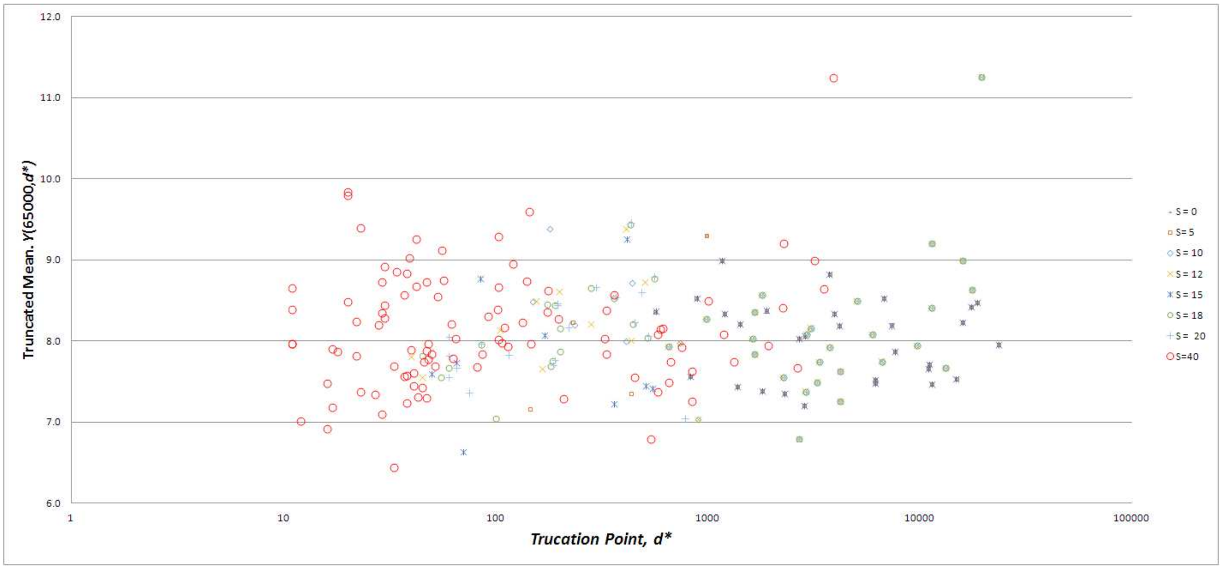

Figure 7: Scatterplot of the truncated mean estimates vs. truncation points for eight initial conditions plotted on a log scale with zeros removed.

4.4 Original vs. Truncated Estimates

Figure 8 compares the mean estimates for the truncated series (in black) against those for the original series without truncation (in grey). Clearly, the mean estimates are quite good without truncation, which presents a challenge to any truncation procedure. Indeed, one might question the choice of this particular example as the best test for an initialization procedure. Nevertheless, the mean estimates after MSER truncation are at least as good as those for the original series and in most cases modestly more accurate given these data.

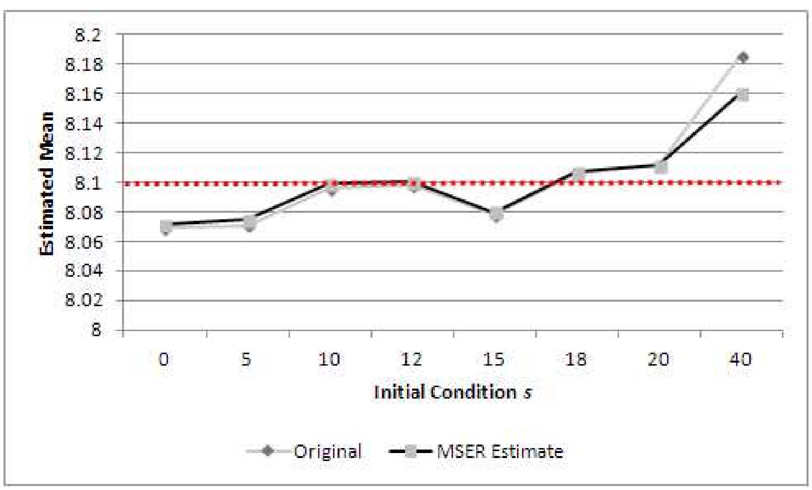

Figure 8: Comparison of the mean estimates for the original and truncated series.

Since the truncated data for all values of *s* are homogeneous and the amount of truncation typically is modest, we can combine all 800 observations of both the original and truncated means. Figures 9 and 10 and Table 3 provide results which compare the combined data sets for both the original and truncated series. Again we see the mean estimates for this problem are quite good without truncation. While the differences are neither practically or statistically significant, applying MSER-5 truncation improves the accuracy of these estimates by over an order or magnitude (reducing the percent error in the estimate from 0.0284% to 0.0098%) given these data with a negligible loss in precision.

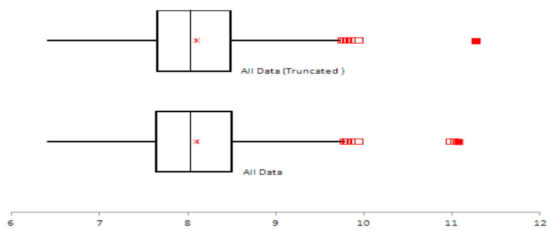

Figure 9: Comparison of the box-whisker plots for fits for the original and truncated means.

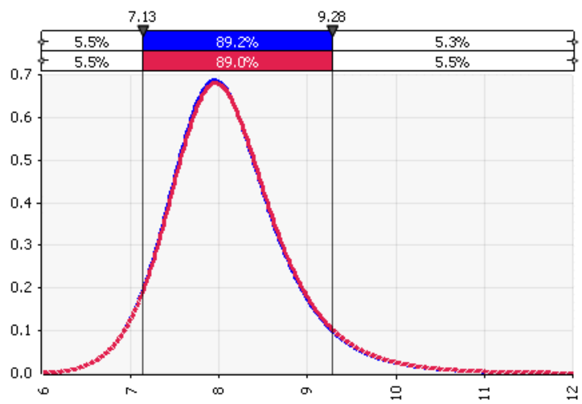

Figure 10: Comparison of the sampling-distribution fits for the original and truncated means. (The original has a very modestly fatter tail.)

Table 3: Comparison of confidence intervals for original and truncated means.

	All Data	All Data Truncated
Sample Mean	8.1023	8.1008
Lower Limit	8.0549	8.0530
Upper Limit	8.1498	8.1486
Sample Std Dev	0.6835	0.6894
Lower Limit	0.6516	0.6572
Upper Limit	0.7188	0.7250

5 CONCLUSIONS

In this paper we examined the performance of MSER initialization by estimating the steady-state mean of the delay time in an M/M/1 queue from within a replication/deletion framework. Using a range of initial conditions, we estimated the sampling distributions of both the MSER-truncated mean and the MSER truncation point and developed confidence bounds on the corresponding estimates. We showed that, while the mean estimates for this problem are quite good without truncation, applying MSER-5 truncation modestly improves the accuracy of these estimates. We also demonstrated that, under the conditions simulated, the point estimates of the mean are statistically independent of both initial conditions and the corresponding MSER-5 truncation point. This underscores our belief that MSER is best applied to individual rather than combined output sequences. That is, each output sequence should be truncated prior to being combined for estimation, rather than averaging the sequences and then truncating the averaged sequence for estimation.

In contrast to the suggestion that MSER may not truncate an appropriate large number of observations, we show that on average the MSER actually truncates a *greater* number of observations than contemplated by Kelton and Law (1985). This is easily explained in terms of the differing convergence criteria, the differing procedure by which MSER is applied, and the fact that MSER attempts to minimize the mean-squared error in the estimate, not the bias alone. Indeed, there is a (smaller and more exacting) value of p for which the p% settling time adopted by Kelton and Law (1985) will yield exact correspondence with the MSER truncation point for the data used here.

We note that in experiments in which the *sampling* distribution is slow to converge across replications, as here, theoretical averages may be very misleading if not applied to an appropriately large sample of replications. Moreover, while initial sequences may appear to be "biased" or "unbiased" on average in a temporal sense, these may in fact be wholly representative when viewed in terms of frequency, depending on the specific character of the corresponding sequences reserved and used for estimation. Finally, repeat the common-sense observation that the performance of any initialization scheme ultimately depends on quality of the estimates achieved, irrespective of the truncation points selected.

ACKNOWLEDGMENTS

The organization and presentation of this paper has benefited greatly from the unusually careful reading and comments by the anonymous referees.

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