

Bootstrapping Efficiency Probabilities in Parametric Stochastic Frontier Models*

William C. Horrace

Syracuse University and NBER

Seth O. Richards

NBER

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Abstract

Parametric stochastic frontier models yield firm-level technical efficiency measures based on estimates of parameters from truncated normal distributions. Using bootstrapped estimates of these parameters and the parametric probability statements that they imply, this study is concerned with inference techniques on maximal efficiency for the stochastic frontier model. A non-standard multivariate distribution yields probabilities that each firm is efficient, while a bootstrap algorithm incorporates the sampling variability of the underlying parameters. The technique provides unique maximal efficiency measures that are relative (as opposed to absolute), that allow efficiency statements on subsets of firms based on secondary firm characteristics, and that account for all sources of variability in the analysis. Applications to a cost function of U.S. banks and to a production function for Texas electric utilities are considered.

Key Words: Truncated Normal, Stochastic Frontier, Bootstrap, Efficiency.

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* Center for Policy Research, 426 Eggers Hall, Syracuse University, Syracuse, NY, 13244-1020. Voice: 315-443-9061. Fax: 315-4431081: Email: whorrace@maxwell.syr.edu.

1 Introduction

A broad class of fully-parametric stochastic frontier models represent production or cost functions as composed-error regressions and imply that firm-level production or cost efficiency can be characterized as a truncated (at zero) normal distribution. Whether cross sectional data or panel data, cost frontier or production frontier, time-invariant efficiency or time-varying efficiency, parametric stochastic frontier models typically yield truncated normal distributions for efficiency. See, for example, Jondrow et al. (1982), Battese and Coelli (1988), Kumbhakar (1990), Battese and Coelli (1992), Cuesta (2000), and Greene (2005). After estimating the cost or production function for a sample of firms, analyses typically ignore the sampling variability inherent in the estimated function, and then, using parametric assumptions on the composed error, calculate the mean and variance of normal distributions, which (when truncated at zero) estimate the conditional distributions of efficiency for each firm. Often the conditional mean of the truncated distribution is used as an "estimate" of efficiency [e.g., Jondrow et al. (1982), and Battese and Coelli (1988)].

If these parametric models are a correct representation of the data generation mechanism, then their estimation suffers from two technical deficiencies. First (and most obviously), the sampling variability of the cost or production function is not taken into account. A few studies have considered this variability and attempt to incorporate it using the bootstrap [e.g., Jensen (2000), and Simar (1992)]. Second (and perhaps less obviously), all that these models

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truly identify is the *distribution* of efficiency and not an estimate of realizations of efficiency itself. Consequently, using the conditional mean of the truncated normal distribution as a point estimate of efficiency is potentially misleading, since a firm's realized efficiency may not equal this mean in any particular sample.¹ Even more to the point, comparing firms by ranking these conditional means compounds the opportunity for misinterpretation, because the true efficiency differences across firms may not equal the differences of the conditional means in any particular sample. This problem has been addressed by Horrace and Schmidt (1996), who calculate confidence intervals from the truncated distributions, and by Bera and Sharma (1999), who calculate the conditional variances of the distributions. Recently, Horrace (2005) calculates probabilities on relative maximal efficiency that allow statements to be made on which firm is relatively most (least) efficient. That is, the technique yields statements like, "firm j is most (least) efficient relative to the rest with probability 0.3," and it is argued that this is the only measure of relative efficiency that the parametric stochastic frontier model truly identifies. Additionally, these statements on maximal (minimal) efficiency neatly summarize the multiplicity inherent in a rank statistic into a single probability statement, which heretofore had not been done in the fully-parametric setting.² Unfortunately, Horrace and Schmidt (1996), Bera and Sharma (1999), and Horrace (2005) all ignore the sampling variability from the frontier function estimation.

This paper attempts to overcome these technical deficiencies by applying bootstrap tech-

¹ If there were sample realizations of technical efficiency for each firm, we would naturally estimate some conditional mean. Here, however, the conditional mean estimate is derived directly from moment conditions imposed on the estimation problem itself and is, therefore, an artifact of the specification, not a "result" of the empirical exercise.

² This has been accomplished in the semi-parametric, fixed-effect specification of the stochastic frontier, using the theory of multiple comparisons. See Horrace and Schmidt (2000).

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niques to the results of Horrace (2005). If statements like "firm j is most efficient with probability 0.3" are the only appropriate measure of a firm's relative performance, then incorporating the sampling variability through the bootstrap could meaningfully change the magnitude of the statements and the inferences that they imply. This is empirically relevant, particularly if the bootstrap confounds the statements to the point where the implied ranking (across j) of the probabilities is meaningfully changed. The paper also demonstrates that the relative efficiency probabilities can be made for any *subset* of the firms in the sample, where the subset might be selected based on some additional information criterion which does not enter into the frontier estimation. The next section reviews the stochastic frontier model, defines probability statements on maximal and minimal efficiency, and discusses the bootstrapping algorithm employed. Section 3 illustrates the results with two empirical examples: the efficiency of Texas electric utilities and the efficiency of a panel of U.S. Banks. Section 4 summarizes and concludes.

2 Probability Statements on Maximal Efficiency

The parametric stochastic frontier model was introduced simultaneously by Aigner, Lovell and Schmidt (1977) and Meeusen and van den Broeck (1977). Since then, there have been many re-formulations of the basic model. For example, consider the standard linear frontier specification for panel data with time-invariant efficiency:

$$y_{jt} = x'_{jt}\beta + v_{jt} \pm u_j, \quad j = 1, \dots, n, \quad t = 1, \dots, T, \quad (1)$$

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where y_{jt} is productive output or cost for firm j in period t ; x_{jt} is a vector of production or cost inputs and β is an unknown parameter vector. The $v_{jt} \in \mathbb{R}$ are random variables representing shocks to the frontier. Let v_{jt} have an *iid* zero-mean normal distribution with variance σ_v^2 . The $u_j \in \mathbb{R}_+$ are random variables representing productive or cost inefficiency, added to the cost function representation or subtracted from the production function representation. Let u_j have a distribution that is the absolute value of an *iid* zero-mean normal random variable with variance σ_u^2 . Additionally, let the x_{jt} , v_{jt} and u_j be independent across j and across t . There are more flexible parameterizations of the linear model. For example, Kumbhakar (1990), Battese and Coelli (1992), and Cuesta (2000) consider forms of time-varying efficiency, u_{jt} . Greene (2005) considers an extremely flexible model that incorporates firm level heterogeneity in addition to the usual error components. Our selection of the more simple model in equation 1 is merely to allow direct comparison to the results in Horrace (2005) and should not be construed as a limitation on the technical results that follow. In fact, the inference and bootstrapping procedures detailed herein apply in time-varying efficiency models, in Greene (2005), or in *any* frontier model where the conditional distribution of efficiency is truncated normal (including the case where the *unconditional* distribution of efficiency is exponential). Per Jondrow et al. (1982), the distribution of u_{jt} conditional on $\epsilon_{jt} = v_{jt} \pm u_{jt}$ is a $N(\mu_{*j}, \sigma_*^2)$ random variable truncated below zero. Per Battese and Coelli (1988), the μ_{*j} and σ_*^2 are:

$$\mu_{*j} = \pm \frac{\sigma_u^2 \bar{\epsilon}_j}{\sigma_u^2 + \frac{\sigma_v^2}{T}}, \quad j = 1, \dots, n; \quad (2)$$

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and

$$\sigma_*^2 = \frac{\sigma_u^2 \sigma_v^2}{T\sigma_u^2 + \sigma_v^2}, \quad (3)$$

where $\bar{\epsilon} = T^{-1} \sum_{t=1}^T \epsilon_{jt}$. (The right-hand side of equation 2 is "+" for the cost frontier or "-" for the production frontier) Parametric estimation usually proceeds by corrected GLS or MLE [e.g. Horrace and Schmidt (1996) for details], yielding estimates $\hat{\beta}$, $\hat{\sigma}_u^2$, and $\hat{\sigma}_v^2$. Then, defining $e_{jt} = y_{jt} - x'_{jt}\hat{\beta}$, "estimation" of μ_{*j} and σ_*^2 follows by substituting e_{jt} for ϵ_{jt} , $\hat{\sigma}_u^2$ for σ_u^2 , and $\hat{\sigma}_v^2$ for σ_v^2 in equations 2 and 3. Then, for a log-production function, the usual measure of technical efficiency based on a $N(\hat{\mu}_{*j}, \hat{\sigma}_*^2)$ assumption is:

$$\hat{\theta}_j = E(\exp\{-u_j\}|\epsilon_j) = \exp\{-\hat{\mu}_{*j} + \frac{1}{2}\hat{\sigma}_*^2\} \frac{1 - \Phi\left(\frac{\hat{\sigma}_* - \hat{\mu}_{*j}}{\hat{\sigma}_*}\right)}{1 - \Phi\left(\frac{-\hat{\mu}_{*j}}{\hat{\sigma}_*}\right)}, \quad j = 1, \dots, n. \quad (4)$$

Assuming that the specification in 1 is true, it is widely known that estimates from equations 2, 3, and 4 are problematic, and yet they are often employed in empirical analyses. First, all three results hinge critically on the distribution of e being the same as the distribution of ϵ , which is questionable. Essentially the calculations assume that $e_{jt} = \epsilon_{jt}$, and it is not clear that this will hold (even asymptotically). Even if it were the case that $e_{jt} = \epsilon_{jt}$ (the equivalent of knowing β), we would still have to estimate $\hat{\mu}_{*j}$ and $\hat{\sigma}_*^2$, and it is not clear what their distributions are, nor is it clear that the distribution of $u_j|e_{jt} = \epsilon_{jt}$ is still truncated normal when μ_{*j} and σ_*^2 are merely estimated. Therefore, the point estimate in 4 is suspect. Intuitively, the convolution of v_{jt} and u_j creates an identification problem for the "structural parameters" μ_{*j} and σ_*^2 (even under strong parametric assumptions on the shape and dependence of the error component distributions). In the words of Bill Greene (paraphrased, personal communication), "the problem is that u_j can never be observed."

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Despite this problem, the parametric model is still empirically relevant and estimates from equations 2, 3, and 4 are regularly reported [e.g., Jensen, 2000, Table 1]. Second, even if we ignore the first problem, the point estimate in 4 is misleading. Granted the shape of the conditional distribution may be "known," but it is unrealistic to think that the first moment of an asymmetric, truncated distribution can summarize its entire probabilistic nature. Illustration of this point is the essence of the contributions of Horrace and Schmidt (1996) and Bera and Sharma (1999): the first moment does not adequately summarize efficiency variates, so one should quantify the second moment too by constructing confidence intervals or calculating the variance of the truncated distribution. (Ideally, one might calculate higher moments as well, particularly odd moments, which affect the probability of extreme realizations of efficiency in clear ways.)

What may be potentially more misleading than reporting $\hat{\theta}_j$ is reporting the relative ranks of $\hat{\theta}_j$ to identify relatively maximally or minimally efficient firms. The problem here is that the ranks completely mask the fact that the true exercise should be a ranking of the conditional distributions (not the conditional means), because the *distributions* are all that the model identifies. The confidence intervals on the individual $\hat{\theta}_j$ of Horrace and Schmidt (1996) account for this, insofar as they may overlap across j , exposing the weakness of conclusions based on differences of the ranked $\hat{\theta}_j$. Horrace and Schmidt conclude that, "in many empirical analyses using stochastic frontier models, differences across firms in efficiency levels are statistically insignificant." Indeed, calculating the second moment of the distribution is informative, but the intervals or Horrace and Schmidt cannot be used

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to perform inference about the maximal (or minimal) relative efficiency, because they do not account for the multiplicity implied by a statement about the maximum. (That is, the intervals cannot say that $\widehat{\theta}_j > \widehat{\theta}_1$ and $\widehat{\theta}_j > \widehat{\theta}_2$ and $\widehat{\theta}_j > \widehat{\theta}_3$ and...) Horrace (2005) addresses these shortcomings in the estimate of $\widehat{\theta}_j$ and calculates multivariate probabilities conditional on ϵ , given that the distribution of u_j is a truncated (at zero) normal variate and β is known.

These probabilities are:

$$P_{\max}^j = \Pr\{u_j < u_i \forall i \neq j\} \quad j = 1, \dots, n, \quad (5)$$

$$P_{\min}^j = \Pr\{u_j > u_i \forall i \neq j\} \quad j = 1, \dots, n, \quad (6)$$

Notice that there is room for confusion in the notation. The "*max*" notation in P_{\max}^j is intended to represent the fact that j is "maximally efficient", which happens to coincide with u_j being *minimal* ($u_j < u_i \forall i \neq j$ in a probabilistic sense). The "*max*" notation should *not* be confused with "maximal u_j ", which is synonymous with "minimal efficiency". Similarly, the "*min*" notation in P_{\min}^j represents the fact that j is "minimally efficient" ($u_j > u_i \forall i \neq j$ in a probabilistic sense). Specifically, the probabilities are given by:

$$P_{\max}^j = \int_0^{\infty} f_{u_j}(u) \prod_{i \neq j}^n [1 - F_{u_i}(u)] du,$$

and

$$P_{\min}^j = \int_0^{\infty} f_{u_j}(u) \prod_{i \neq j}^n F_{u_i}(u) du,$$

where $f_{u_j}(u)$ and $F_{u_j}(u)$ are the probability function and the cumulative distribution function of a $N(\mu_{*j}, \sigma_*^2)$ distribution truncated at zero, respectively. That is,

$$f_{u_j}(u) = \frac{(2\pi\sigma_*^2)^{-1/2} \exp\left\{-\frac{(u-\mu_{*j})^2}{2\sigma_*^2}\right\}}{1 - \Phi(-\mu_{*j}/\sigma_*)},$$

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and

$$F_{u_j}(u) = \frac{\Phi(\{u - \mu_{*j}\}/\sigma_*) - \Phi(-\mu_{*j}/\sigma_*)}{1 - \Phi(-\mu_{*j}/\sigma_*)},$$

where Φ is the cumulative distribution function of the standard normal. The probabilities in equations 5 and 6 condense all the information on the relative differences of the distributions of efficiency into a single statement and also account for the multiplicity of the probability statement on maximal (minimal) efficiency, which the conditional mean and conditional variance cannot. These probabilities "may be of great use to researchers." (Dorfman and Koop, 2005, p. xxx).

A useful feature of these probabilities is that they are statements of *relative* efficiency (efficiency relative to a within sample standard), whereas the typical efficiency measure, $\hat{\theta}_j$, is a measure of *absolute* efficiency (efficiency relative to an unobserved population standard). Relative efficiency may be empirically relevant. For example, one may be interested in understanding the relative performance of a subset of the sample of firms $j = 1, \dots, n$, based on a certain information criteria or decision rule. In our banking application, we estimate a cost function for a sample of 500 banks, but then calculate probabilities of maximal efficiency for a subset of the banks with large assets; we are interested in how large banks perform relative to one another, conditional on a common cost function for all banks. The probabilities P_{\max}^j and P_{\min}^j will change as the cardinality of and the membership within this subset changes. That is, let set $N = \{1, \dots, n\}$ be the set of all firm indices in the sample, and let this subset be $J_\Omega \subset N$ be based on some external information or decision rule Ω . Then the probabilities

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in equations 5 and 6 become:

$$P_{\Omega \max}^j = \int_0^\infty f_{u_j}(u) \prod_{i \neq j, i \in J_\Omega} [1 - F_{u_i}(u)] du, \quad (7)$$

and

$$P_{\Omega \min}^j = \int_0^\infty f_{u_j}(u) \prod_{i \neq j, i \in J_\Omega} F_{u_i}(u) du, \quad (8)$$

for all $j \in J_\Omega$. These will be different, in general, than the probabilities P_{\max}^j and P_{\min}^j of Horrace (2005). In fact, the probabilities in equations 5 and 6 are a special case of equations 7 and 8 when $J_\Omega = N$. If Ω is empirically relevant, then probabilities like $P_{\Omega \max}^j$ ($j \in J_\Omega$) may be more useful than P_{\max}^j ($j \in N$). Also, experiments on the effects of different Ω and J_Ω on the probabilities in equations 7 and 8 may be of particular interest to empiricists. These types of experiments flow more naturally from relative efficiency measures like the probabilities in equations 7 and 8 than they do from absolute efficiency measures like $\hat{\theta}$ in equation 4.

2.1 Bootstrapping Probabilities on Maximal Efficiency

The single shortcoming of the probabilities in equations 5, 6, 7, and 8 is that they ignore the sampling variability in the original model (i.e., they assume β is known). The goals here are to bootstrap $\hat{\beta}$, $\hat{\sigma}_u^2$, $\hat{\sigma}_v^2$, e_{jt} , $\hat{\mu}_{*j}$, and $\hat{\sigma}_*^2$ and to incorporate this uncertainty into the probability statements on maximal efficiency, P_{\max}^j or $P_{\Omega \max}^j$, in equations 5 or 7. This section details the bootstrapping algorithm employed. We bootstrap the original data $\{y_{jt}, x_{jt}\}$ (the so-called "naive bootstrap"), as opposed to bootstrapping from the residuals e_{jt} in the style of Simar (1992), Hall et al. (1995), Simar and Wilson (2000), Jensen (2000), or Kim and

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Schmidt (2000). Bootstrapping of the residuals is necessary when producing the empirical distribution of a point estimate of efficiency: a distribution of bounded, unknown support. Per Simar and Wilson (2000), the naive bootstrap may be problematic in these instances. Basically, estimation of efficiency often requires application of a "max" operator which causes the non-parametric empirical distribution to have a mass point at an extreme tail. Here, we are not interested in point estimates of efficiency, *per se*; we are effectively interested in the empirical distribution of the $\hat{\mu}_{*j} \in \mathbb{R}$ which maps in the empirical distribution of the probability statements, so the naive bootstrap seems appropriate in this context.³

Let bootstrap replications be indexed by b , and let the bootstrap sample be $\{y_{jt}, x_{jt}\}_b$. Bootstrapping in this context is complicated by several factors: First, it is natural to randomize across j only, and not across j and t , because we do not want to mix performance data between firms. Therefore, when a firm j is selected in the randomization process, all y_{jt} and x_{jt} , $t = 1, \dots, T$ data points are drawn. (This is identical to the procedure for drawing the original sample of size n from the population on interest.) Therefore, we will randomly draw with replacement from the set $N = \{1, 2, \dots, n\}$, and the bootstrap sample is actually:

$$\{y_{jt}, x_{jt}\}_b = \{(y_{j1}, x_{j1}) \dots (y_{jT}, x_{jT})\}_b, \quad j = 1, \dots, n, \quad b = 1, \dots, B.$$

Second, occasionally a bootstrap replication will yield $\hat{\sigma}_u^2 < 0$, and in this case it is standard practice to discard the replication. Therefore, if the bootstrap algorithm is designed for B replications, it may be the case that the final bootstrap sample size is some $B^* < B$, and

³ Indeed, in the applications that follow, the empirical distributions from the bootstrap of the $\hat{\mu}_{*j}$ were all smooth with thin tails in. These mapped into P_{\max}^j and P_{\max}^j , producing similarly well-behaved empirical distributions of these probabilities.

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in these cases the bootstrap results may be biased.⁴ Finally, in a given resample, b , the number of *unique* firms j may be less than n due to the replacement of already sampled firms j . Consequently, using only the replicated data, the number of *unique* $\hat{\mu}_{*j}$ may be less than n , making it impossible to make meaningful comparisons across all n firms in the original sample using equation 5 or across some fixed subset of N using equation 7. Therefore, for the purposes of calculating $\hat{\beta}$, $\hat{\sigma}_u^2$, and $\hat{\sigma}_v^2$, the bootstrap data are used, but for the purposes of calculating e_{jt} , $\hat{\mu}_{*j}$, $\hat{\sigma}_*^2$, and the probabilities in equations 5 and 7, the original data, $\{y_{jt}, x_{jt}\}$, are used. Based on these considerations, the bootstrap algorithm is:

- 1 Resample with replacement a sample of size n across j from $\{y_{jt}, x_{jt}\}$, and use the resulting $\{y_{jt}, x_{jt}\}_b$ to estimate $\hat{\beta}_b$, $\hat{\sigma}_{ub}^2$, and $\hat{\sigma}_{vb}^2$, $b = 1, \dots, B$ by MLE or corrected GLS.
- 2 After discarding replications with $\hat{\sigma}_{ub}^2 \leq 0$ and using the *original* data, $\{y_{jt}, x_{jt}\}$, calculate $e_{jtb} = y_{jt} - x'_{jt}\hat{\beta}_b$, $b = 1, \dots, B^*$. Then use equations 2, 3 to calculate $\hat{\mu}_{*jb}$, and $\hat{\sigma}_{*b}^2$, $b = 1, \dots, B^*$, $j = 1, \dots, n$.
- 3 Use the $\hat{\mu}_{*jb}$, and $\hat{\sigma}_{*b}^2$ to calculate $P_{\max b}^j$ or $P_{\Omega_{\max b}}^j$; $b = 1, \dots, B^*$ in equations 5 or 7, respectively.

3 Applications

We begin with a reanalysis of the production frontier data in Horrace (2005) to see if incorporation of the sampling variability in $\hat{\beta}$ has meaningful effects on the probability of relative

⁴ All things being equal, negative values for $\hat{\sigma}_u^2$ will be more likely in populations where σ_u is small relative to σ_v . Therefore, discarding negative $\hat{\sigma}_u^2$ should upward bias σ_u and, perhaps, downward bias σ_v . It is, however, more difficult to speculate on the bias effects on other parameters in the model. Exploring these effects is left for future research.

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efficiency. These are panel data for Texas electric utility plants between 1966-1985, where inputs to the production of the logarithm of electricity are the logarithms of capital, labor and fuel. Even though $T = 18$ is large relative to $n = 10$, we find that ignoring the the sampling variability may be problematic. For comparative purpose, we analyze a second panel data set where n is large relative to T : the cost frontier of 500 U.S. banks for the 5 years between 1996 and 2000. Here the problems on the probability statements of ignoring the sampling variability seem less pronounced. It appears that the large value of $n = 500$ for the banks may be driving this result.

3.1 Texas Electric Utilities 1966-1985

Horrace and Schmidt (1996) and Horrace (2005) calculate the GLS technical efficiency of 10 Texas electric utility plants from a panel of data between 1966-1985, where inputs to the production of electricity are capital, labor and fuel. See Kumbhakar (1996) for a complete explanation of the data. The production frontier is based on the Cobb-Douglas specification,

$$\ln Y_{it} = \alpha + \beta_1 \ln K_{it} + \beta_2 \ln F_{it} + \beta_3 \ln L_{it} + v_{ji} - u_j,$$

where Y_{it} = output in kilowatts of electricity, K_{it} = capital, F_{it} = fuel, and L_{it} = labor.

Both the GLS sample and bootstrap results are in Table 1. The first column contains the parameters, the second column contains the sample estimates; the third and fourth columns contain the bootstrap mean and median estimates, respectively; and the last two columns contain a 90% bootstrap confidence interval. There are no real surprises for the estimated elasticities; the sample estimates of the elasticities are essentially in agreement

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with the bootstrap estimates. The sample estimates are also fairly well centered in the bootstrap confidence intervals. Similarly for the variance component estimate $\hat{\sigma}_v^2$. However, things are quite different for the estimates $\hat{\sigma}_u^2$, and $E(u)$, where ignoring sampling variability is apparently meaningful. Here, the sample estimates and the bootstrap estimates are markedly different, with the sample estimates being at the extreme upper bound of the bootstrap confidence interval. For example the estimate for σ_u^2 in the sample is 0.008 while its bootstrap median estimate is 0.002. This suggests that, while the sample elasticity estimates are accurate, the variance estimate of inefficiency may be biased.

This has implications for the estimates $\hat{\mu}_{*j}$ in equation 2, which are contained in rank order in Table 2. The sample estimates of μ_{*j} (column 2) are all larger than the bootstrap estimates (columns 3 and 4). For example, for firm 4 (which has largest $\hat{\mu}_{*j}$, and, hence, has high probability of being inefficient) the sample estimate of $\hat{\mu}_{*j}$ is about 50% larger than the median bootstrap estimate. Consequently, the sample estimates in Table 2 are all nearer to the upper bound of the confidence interval (column 6) than to the lower bound (column 5). Clearly, ignoring the variability in $\hat{\beta}$ has consequences for the magnitude of the mean (and variance) of the underlying normal distribution of efficiency for this particular data set. It also appears that the distribution of the bootstrap $\hat{\mu}_{*jb}$ is skewed for certain firms. Firm 5 has a mean below the median, and firm 4 has the mean above the median. Fortunately, the rank order of the firms is preserved, when based on any of the three estimates: $\hat{\mu}_{*j}$, median $\hat{\mu}_{*jb}$, or mean $\hat{\mu}_{*jb}$.

Turning to the probability results in Table 3, we see that ignoring variability is problem-

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atic for the maximal probability statements for this data⁵. That is, the probability statement based on the sample P_{\max}^j that "firm 5 is relatively efficient with probability 0.709," may be biased and imprecise. First, the mean (0.607) and median (0.650) estimates of the probability $P_{\max b}^j$ are considerably lower than the sample estimate (0.709) for firm 5. It seems reasonable to suspect that adding an additional source of variability to the analysis would decrease the probability (on average) at the extreme upper end of the rank statistic. Therefore, this sample probability may be biased when the additional variability is ignored. Second, the 90% confidence interval is extremely wide [0.162, 0.932]. That is, the probability that firm 5 is efficient may be as low as 0.162 or as high as 0.932, when the sampling variability of $\hat{\beta}$ is taken into account at the 90% level.⁶ In fact, according to the confidence intervals, firm 3 may have a relatively high probability of being the most efficient: 0.632 (column 6). This is a fairly pessimistic outcome for this particular rank statistic, which has gone unnoticed in previous analyses of this data set. A few things may be driving this result. First, the model might be mis-specified any number of ways (although the parameters for capital and fuel were statistically significant at the 95% level). Second, the bootstrap produced 185 instances where $\hat{\sigma}_{ub}^2 \leq 0$ (out of 1,000). This could be producing a bias in the bootstrap and confounding the results. Of course, there is no way to know this, so it is difficult to speculate whether the sample $\hat{\mu}_{*j}$ are biased from ignoring the sampling variability in $\hat{\beta}$ or the bootstrap $\hat{\mu}_{*jb}$ are biased from discarding 18.5% of the resamples. This is particularly

⁵ Slight differences between P_{\max}^j in Table 3 and those reported in Horrace (2005) are due to differences in the integration algorithms employed.

⁶ Because they are along different metrics, it is difficult to make direct comparison between this interval and those reported in Horrace and Schimdt (1996). However, their 90% confidence interval for the *absolute* efficiency of firm 5 was [0.9949, 0.9999], a much tighter interval which ignores the variability in $\hat{\beta}$.

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unfortunate, because this data set possesses the classic features that cause the stochastic frontier model to be reliable for making distinctions among firms: large T and relatively large signal to noise ratio: σ_u^2/σ_v^2 (see Horrace and Schmidt, 1996, p. 281). Nonetheless, these probability results seem highly sensitive to the addition of the sampling variability through the bootstrap. To gain further insight into what may be driving this outcome, we turn to estimating a cost function for U.S. banks, an exercise which produces far less pessimistic results.

3.2 U.S. Banks 1996-2000

The data for this analysis are derived from the Commercial Bank Holding Company Database of the Federal Reserve Bank of Chicago. Compiled for analysis by Kumbhakar and Tsionas (2002), the data set is a panel of 5,000 banks from 1996 to 2000 in the Federal Reserve database. Similar to Kumbhakar and Tsionas, we draw a random sample of 500 banks for analysis. The observed variables, for use in a cost function, are total cost, five outputs, and five prices (calculated as average expenditures). Here, we estimate a Cobb-Douglas cost function that is identical to the primary model in Greene (2005, equation 10), except that technical efficiency is time invariant over the 5 year period. Let:

$$\ln(C_{jt}/W_{5jt}) = \alpha + \sum_{i=1}^4 \beta_i \ln(W_{ijt}/W_{5jt}) + \sum_{i=1}^5 \gamma_i \ln Y_{ijt} + \delta t + v_{ji} + u_j,$$

where C_{jt} = total cost, Y_{1jt} = installment loans to individuals for personal or household expenses, Y_{2jt} = real estate loans, Y_{3jt} = business loans, Y_{4jt} = federal funds sold and securities purchased under agreements to resell, Y_{5jt} = other assets, W_{1jt} = price of labor,

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W_{2jt} = price of capital, W_{3jt} = price of purchased funds, W_{4jt} = price of interest-bearing accounts in transaction accounts, W_{5jt} = price of interest-bearing accounts in non-transaction accounts, and t = time trend.

Corrected GLS estimation results are contained in Table 4. The most compelling result is that the sample estimates and bootstrap estimates are similar for the parameters σ_u^2 and $E(u)$. For example at three significant digits, the results are nearly identical for the estimates of σ_u^2 (around 0.140), suggesting that ignoring the sampling variability in these data may not introduce a bias into technical efficiency results. This was not the case for the Texas utilities. Why might this difference exist? First, for the banks there were no cases where the bootstrap resample was discarded because $\hat{\sigma}_u^2 < 0$, so biases in the bootstrap from this phenomenon were irrelevant. Second, it appears that large n is preferred to large T when *bootstrapping* over n is employed, even though conventional wisdom suggests that for the purpose of differentiating efficiency scores across banks *in the sample* large T is preferred (Horrace and Schmidt, 1996, p. 281). No doubt this is being driven by the higher probability of perverse bootstrap draws in the utilities data (small n), relative the to bank data (large n). That is, when n is small the unique combinations of potential bootstrap draws are small in number, causing the probability of drawing any particular bootstrap sample to be large. In particular, perverse draws, which differ substantially from the original sample (in terms of their implied estimates) are more likely. Therefore, large departures from sample results are more likely when n is small. This is occurring in the Texas utility data, even though they appear to be less noisy (based on the sum of the variance of the errors components).

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It does not seem to be occurring in the banking data since n is large. This is clearly not a universal truth; it is probably just an artifact of these data. However, it does suggest that ignoring sampling variability may be less of a problem in data sets where n is large. To complicate matters, in the Texas utilities data these large departures may be increasing the effects of the problem of bias created by $\hat{\sigma}_u^2 < 0$. Suffice it to say that, the sample results for the banks seem more reliable than those for the utilities, in terms of any potential biases that may have been uncovered by the bootstrap.

The variance components map into the estimates $\hat{\mu}_{*j}$, so we see that for U.S. banks the $\hat{\mu}_{*j}$ are also fairly close across the sample and bootstrap estimates, indicating that sample bias may be relatively small. These are ranked by sample $\hat{\mu}_{*j}$ in Table 5, for the fourteen largest banks in the sample, banks with average assets in excess of \$1 billion over the 5 year period.⁷ That is, the selection criterion is $\Omega =$ "select banks with average assets exceeding \$1 billion," and then:

$$J_{\Omega} = \{432, 267, 405, 280, 327, 261, 112, 239, 377, 211, 60, 418, 115, 461\}.$$

For example, bank 432 (first column), which was the thirteenth largest bank in the sample (second column) has an estimated μ_{*j} of -0.052 in the sample (third column), -0.053 for the bootstrap mean (fourth column), and -0.052 for the bootstrap median (fifth column). These are much closer for the banks than for the Texas utilities, and this closeness is consistent across the the rank statistic. For example, the eighth largest bank, bank 377, has $\hat{\mu}_{*j} = 0.376$, mean $\hat{\mu}_{*jb} = 0.378$, and median $\hat{\mu}_{*jb} = 0.379$. The estimated μ_{*j} are also closer

⁷ Selecting the largest banks seemed reasonable, for the performance of large banks may be empirically relevant.

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to the center of the bootstrap confidence intervals than those of the Texas utility data, strengthening the argument that the banking sample estimates are more reliable than the utility sample estimates.

Turning to the probability statements of Table 6, we see nice results here also. The magnitudes of the probability of maximal efficiency are similar across the three measures: sample $P_{\Omega_{\max}}^j$ (column 3), mean $P_{\Omega_{\max}b}^j$ (column 4), or median $P_{\Omega_{\max}b}^j$ (column 5). For example, the probability that bank 432 is efficient relative to the others in the rank statistic is 0.778, 0.739, and 0.746, based on the three measures, respectively. For bank 267 the same three measures are: 0.199, 0.200, and 0.211, respectively. Also, the confidence intervals (columns 6 and 7) are fairly tight across all the banks relative to those of the Texas utilities in Table 3. For example, the most efficient bank in Table 6 had a fairly tight confidence interval of [0.515, 0.923], while the most efficient utility in Table 3 had an extremely wide interval of [0.162, 0.932]. The second most efficient bank (bank 267) had an equally tight confidence interval of [0.074, 0.299]. Surprisingly, the confidence intervals of the first and second most efficient large banks do not overlap, so the evidence here for bank 432 being the most efficient large bank (in a relative sense) is fairly strong.⁸

The second column of Table 6 contains the "Overall Rank" of each bank in terms of its efficiency (based on $\hat{\mu}_{*j}$). For example, bank 432 was the most efficient of the 14 large banks presented in the table, but it was only the 252nd most efficient bank in the overall sample of 500 banks; bank 239 was the eighth most efficient large bank, but the 460th most

⁸ It is interesting to note the similarity between a probability of maximal efficiency and an efficiency score: both are on the unit interval. As such, they may be easily confused. Therefore, it is important to interpret the results in Table 6 as probabilities and *not* as efficiency scores.

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efficient in the sample; bank 461 was the least efficient large bank in the table and the least efficient of all 500 banks. From these results one might conclude that large banks tend to be less efficient, however this was not formally tested, so the conclusion may be spurious.⁹ It is interesting to note the vague parallel between the literature on two-step estimation to "explain" efficiency and the use of a selection criterion Ω to calculate relative efficiency probabilities. In this literature (e.g. Wang and Schmidt, 2000), one regresses the estimated $\hat{\theta}_j$ on some secondary variables z_j to determine the causes of (in)efficiency (e.g. z_j might be the education or experience of the managers in bank j). One can envision Ω as information outside of the production or cost function that is used to understand relative (in)efficiency in some secondary analysis. For example, we could have looked at the efficiency of the 30 oldest banks in the sample or the 10 smallest banks in terms of consumer loans, to see how these banks fare relative to each other and other banks in the sample. This kind of secondary informational analysis seems reasonable in the context of efficiency probabilities, while "it is widely recognized that [the usual] two-step procedures are biased" (Wang and Schmidt, 2000, p. 130).

Notice that the probabilities in the third column of Table 6 sum to 1. The implication is that the events associated with each probability form a partition of the probability space. Therefore, cumulations of the probabilities return joint statements on the events. For example, one can say "the probability that bank 432 *or* bank 267 is efficient is $0.778 + 0.199 = 0.977$ ". Equivalently, this is saying, "the probability that the most efficient large

⁹ Indeed, Kumbhakar and Tsionas (2002) argue that large banks tend to be more efficient and that the recent spate of bank mergers is justified on efficiency grounds.

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bank is either bank 432 or 276 is almost certain", although when one considers the lower bound of the bootstrap confidence interval (column 6), this probability may be as low as $0.515 + 0.074 = 0.589$, or about 60%. These types of joint statements imply the "subsets of best and worst" discussed in Horrace (2005) and provide powerful probabilistic conclusions that point estimates of efficiency in equation 4 cannot (even when the point estimates are augmented with confidence intervals or variance estimates).

4 Conclusions

This study used the bootstrap to incorporate sample variability into probability statements of relative maximal (minimal) efficiency from parametric stochastic frontier models. Using panel data on Texas utilities and U.S. banks, we have shown that the effect of the sampling variability on the probability statement is sensitive to a number of potential factors: the signal to noise ratio in the data, the relative sizes of n and T , and perhaps the bootstrap itself (the relative frequency of discarded bootstrap observations). Therefore, it is important to include some sort of bootstrapping results on efficiency statements for these types of models in the future (particularly when n is small). The study also underscores the strengths and importance of *relative* efficiency probability calculations when one is interested in making efficiency comparisons across firms, or when one is interested in a particular subset of the firms based on some informational criterion. The usual absolute efficiency scores are limited in this regard, and it has been argued that they are simply the wrong measures for characterizing efficiency. Finally, it is interesting to note that the contribution of Horrace

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and Schmidt (1996) was to demonstrate that technical efficiency is plagued by high variance, while here we demonstrate that it may be further plagued by non-trivial bias. In the future, perhaps a Monte Carlo study could be performed to disentangle the factors which seem to be driving the sensitivity of the probability statements to the sampling variability. This might help us to better understand both the nature of parametric stochastic frontier models under sampling variability and the nature of the bootstrap as a means of incorporating this variability.

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Tables

Table 1. GLS Production Function Estimates, Texas Electric Utilities, 1966 to 1985.

Variable	Sample Estimate	Bootstrap Mean	Bootstrap Median	Bootstrap 90% Lower	Bootstrap 90% Upper
<i>Constant</i>	-5.053	-4.929	-5.000	-5.818	-3.882
<i>K</i>	0.588	0.574	0.587	0.304	0.779
<i>F</i>	0.581	0.584	0.577	0.437	0.747
<i>L</i>	-0.097	-0.092	-0.092	-0.254	0.061
$\hat{\sigma}_u^2$	0.008	0.004	0.002	0.000	0.009
$\hat{\sigma}_v^2$	0.003	0.003	0.003	0.002	0.004
$E(u)$	0.071	0.043	0.038	0.013	0.074
$\hat{\sigma}_*$	0.013	0.012	0.012	0.009	0.013

$n = 10, B = 1000, B^* = 815.$

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Table 2. Texas Electric Utilities, ranked by $\hat{\mu}_{*j}$.

Utility j	$\hat{\mu}_{*j}$	Mean $\hat{\mu}_{*jb}$	Median $\hat{\mu}_{*jb}$	$\hat{\mu}_{*jb}$ 90% Lower	$\hat{\mu}_{*jb}$ 90% Upper
5	-0.089	-0.110	-0.091	-0.240	-0.018
3	-0.033	-0.050	-0.037	-0.145	-0.004
10	0.036	0.012	0.017	-0.056	0.070
1	0.070	0.039	0.027	-0.014	0.106
8	0.087	0.053	0.043	-0.007	0.128
9	0.106	0.069	0.061	-0.004	0.149
2	0.108	0.074	0.063	0.014	0.147
6	0.124	0.088	0.081	0.022	0.162
7	0.129	0.094	0.091	0.025	0.168
4	0.156	0.114	0.107	0.032	0.201

$B = 1000, B^* = 815.$

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Table 3. Texas Electric Utilities, Probability of Maximal Relative Efficiency

Utility	P_{\max}^j	Mean	Median	$P_{\max b}^j$	$P_{\max b}^j$
j		$P_{\max b}^j$	$P_{\max b}^j$	90% Lower	90% Upper
5	0.709	0.607	0.650	0.162	0.932
3	0.290	0.308	0.279	0.045	0.632
10	0.001	0.014	0.001	0.000	0.217
1	0.000	0.008	0.000	0.000	0.064
8	0.000	0.005	0.000	0.000	0.049
9	0.000	0.001	0.000	0.000	0.041
2	0.000	0.000	0.000	0.000	0.007
6	0.000	0.000	0.000	0.000	0.001
7	0.000	0.000	0.000	0.000	0.000
4	0.000	0.000	0.000	0.000	0.000

$B = 1000, B^* = 815.$

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Table 4. GLS Cost Function Estimates for U.S. Banks, 1996 to 2000.

Variable	Sample Estimate	Bootstrap Mean	Bootstrap Median	Bootstrap 90% Lower	Bootstrap 90% Upper
<i>Constant</i>	0.603	0.601	0.603	0.272	0.937
W_1/W_5	0.526	0.524	0.524	0.486	0.563
W_2/W_5	0.038	0.038	0.037	0.020	0.055
W_3/W_5	0.039	0.039	0.039	0.023	0.057
W_4/W_5	0.119	0.120	0.120	0.095	0.149
Y_1	0.061	0.063	0.063	0.034	0.096
Y_2	0.376	0.376	0.376	0.339	0.413
Y_3	0.205	0.206	0.206	0.159	0.249
Y_4	0.023	0.023	0.022	0.018	0.028
Y_5	0.166	0.164	0.164	0.131	0.198
t	-0.030	-0.030	-0.030	-0.034	-0.025
$\hat{\sigma}_u^2$	0.143	0.138	0.137	0.119	0.159
$\hat{\sigma}_v^2$	0.012	0.011	0.011	0.010	0.013
$E(u)$	0.302	0.296	0.296	0.275	0.318
$\hat{\sigma}_*$	0.048	0.047	0.047	0.044	0.051

$n = 500, B^* = B = 1000.$

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Table 5. Fourteen U.S. Banks with Average Assets over \$1B, ranked by $\hat{\mu}_{*j}$.

Bank j	Asset Rank	$\hat{\mu}_{*j}$	Mean $\hat{\mu}_{*jb}$	Median $\hat{\mu}_{*jb}$	$\hat{\mu}_{*jb}$ 90% Lower	$\hat{\mu}_{*jb}$ 90% Upper
432	13	-0.052	-0.053	-0.052	-0.160	0.047
267	14	0.041	0.040	0.040	-0.062	0.145
405	7	0.115	0.113	0.112	0.008	0.225
280	9	0.148	0.147	0.147	0.037	0.256
327	10	0.209	0.209	0.210	0.099	0.319
261	6	0.219	0.217	0.216	0.104	0.331
112	3	0.292	0.286	0.284	0.162	0.420
239	11	0.292	0.295	0.296	0.192	0.393
377	8	0.376	0.378	0.379	0.246	0.513
211	2	0.407	0.406	0.405	0.262	0.552
60	5	0.587	0.595	0.596	0.394	0.779
418	12	0.689	0.697	0.698	0.523	0.866
115	4	0.807	0.803	0.799	0.665	0.938
461	1	2.03	2.043	2.045	1.700	2.366

$B^* = B = 1000$.

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Table 6. Fourteen Largest U.S. Banks, Probability of Maximal Relative Efficiency

Bank j	Overall Rank	$P_{\Omega_{\max}}^j$	Mean $P_{\Omega_{\max b}}^j$	Median $P_{\Omega_{\max b}}^j$	$P_{\Omega_{\max b}}^j$ 90% Lower	$P_{\Omega_{\max b}}^j$ 90% Upper
432	252	0.778	0.739	0.746	0.515	0.923
267	331	0.199	0.200	0.211	0.074	0.299
405	375	0.019	0.042	0.027	0.001	0.127
280	399	0.004	0.016	0.006	0.000	0.070
327	429	0.000	0.002	0.000	0.000	0.009
261	435	0.000	0.002	0.000	0.000	0.008
112	459	0.000	0.000	0.000	0.000	0.000
239	460	0.000	0.000	0.000	0.000	0.000
377	479	0.000	0.000	0.000	0.000	0.000
211	484	0.000	0.000	0.000	0.000	0.000
60	492	0.000	0.000	0.000	0.000	0.000
418	494	0.000	0.000	0.000	0.000	0.000
115	496	0.000	0.000	0.000	0.000	0.000
461	500	0.000	0.000	0.000	0.000	0.000

$B^* = B = 1000$. $\Omega =$ "select banks with average assets exceeding \$1 billion."