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A Monte Carlo study of ranked efficiency estimates from frontier models

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Keywords Truncated normal, Stochastic frontier, Efficiency, Multivariate probabilities

Abstract

Parametric stochastic frontier models yield firm-level conditional distributions of inefficiency that are truncated normal. Given these distributions, how should one assess and rank firm-level efficiency? This study compares the techniques of estimating (a) the conditional mean of inefficiency and (b) probabilities that firms are most or least efficient. Monte Carlo experiments suggest that the efficiency probabilities are easier to estimate (less noisy) in terms of mean absolute percent error when inefficiency has large variation across firms. Along the way we tackle some interesting problems associated with simulating and assessing estimator performance in the stochastic frontier model.

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 or panel data, cost frontier or production frontier, time-invariant or time-varying efficiency, parametric stochastic frontier models yield inefficiency distributions that are truncated normal. See, for example, Jondrow et al. (1982), Battese and Coelli (1988, 1992), Kumbhakar (1990), Cuesta (2000), and Greene (2005). After estimating the cost or production function for a sample of firms, parametric assumptions on the composed error are typically used to calculate the mean and variance of normal distributions, which (when truncated at zero) represent the conditional distributions of technical inefficiency for each firm. Given these truncated normal, conditional distributions, a reasonable and commonly asked question is, “how does one assess the relative efficiency ranks of the firms in the sample?” There are currently two very different approaches used to assess the relative efficiency ranks of individual firms in the sample.

The traditional approach is to estimate each firm’s technical efficiency by calculating the conditional mean of its truncated normal inefficiency distribution. See Jondrow et al. (1982) and Battese and Coelli (1988). The conditional means are rational point estimates of inefficiency that, when ranked, reveal information on relative magnitudes of realizations from the truncated normal distributions. However, interpretation of ranked conditional means is somewhat contentious, because the conditional mean is merely a point estimate and is not intended as a ranking device. This is the essence of the arguments in Horrace and Schmidt (1996), who find that the ranks of the conditional means may be unreliable once variability of the distributions is considered. There have been several solutions proposed to assess this reliability (or lack of reliability). Horrace and Schmidt recommend calculating confidence (prediction) intervals for the truncated normal distributions. Bera and Sharma (1999) provide formulae for the conditional variance of the truncated normal distribution. Both of these methods may be used to assess the reliability of the ranked conditional means of technical inefficiency. Recently, Simar and Wilson (2009) show that the prediction intervals of Horrace and Schmidt have poor coverage probabilities and propose “bagging” and bootstrapping approaches to assess the variability of the conditional mean as a point estimate of technical efficiency.

Horrace (2005) proposes an alternative (and valid) ranking device for technical efficiency. He calculates probabilities on relative efficiency (“efficiency probabilities”) that allow statements to be made on which firm (in the sample) is most or least efficient. That is, the approach yields statements like, “firm A is most (least) efficient relative to the rest with probability 0.3.” Unlike confidence intervals and conditional variances, this accounts for the multiplicity implied by the joint inferential statement that firm A is better than B, and better than C, and better than D, etc.¹ The approach is applicable for cross sectional data, panel data or any case where firm-level efficiency is characterized by a conditional distribution that is truncated normal. The approach also controls for the multiplicity associated with inferential statements on the rank statistic that the traditional approach does not.

The two measures are entirely different, as are their interpretations, so comparisons are hard to make. Nonetheless, these comparisons are the goal of this study, which makes recommendations as to when each measure will be more accurately estimated in terms of mean absolute percentage error (MAPE). This information may be

¹ 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).

useful to empiricists interested in assessing relative ranks of technical efficiency. In empirical exercises where the conditional distributions of inefficiency prior to truncation have common variance, the firm rankings based on the conditional mean will be identical to those based on the efficiency probabilities of Horrace (2005). As such, calculating rank correlations with the true inefficiency rankings for each measure reveals nothing about the relative merits of the two approaches.

This paper uses Monte Carlo simulations to compare the precision of the conditional mean estimates and efficiency probability estimates in terms of MAPE. That is, the simulations assess the ability of a firm's conditional mean estimator to serve as an estimate of its (unknown) condition mean; they also assess the ability of a firm's efficiency probability estimator to serve as an estimate of its (unknown) efficiency probability. In particular, the simulations are not concerned with assessing the ability of a firm's conditional mean and efficiency probability estimators to serve as estimates of its unknown technical efficiency (a realization of the error component u in a typical stochastic frontier specification). The simulations also present several complications that underscore the difficulties of efficiency estimation, in general, and that provide insights into the inherent differences of the two estimation approaches. These are discussed in the sequel. We find that the efficiency probabilities are more reliable when the variance of technical inefficiency is large; this is the "usual" case in the sense that it is the only time when estimation of inefficiency is at all precise and when it may be even warranted. In addition to the MAPE results, we present mean squared error (MSE) and bias calculations to examine the effects of changes in the variance parameters and sample sizes on the performance of each estimator (in isolation). We also demonstrate that 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 criterion which does not enter into the frontier estimation. (In fact, we use this technique to simplify our Monte Carlo study when the number of firms is large.)

The next section reviews the stochastic frontier model and defines the estimates to be studied, including the new subset probabilities. Section 3 contains the Monte Carlo study, and Sect. 4 provides a final discussion of the results and concludes.

2. Efficiency Estimation

The parametric stochastic frontier model was introduced simultaneously by Aigner et al. (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:

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

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 (a half-normal distribution). 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} .² 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 Eq. 1 is merely to parallel the model and discussions in Horrace (2005) and should not be construed as a limitation on the applicability of the results that follow. In fact, the inferential 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). In this model per Jondrow et al. (1982), the distribution of u_j conditional on $\epsilon_{jt} = v_{jt} \pm u_j$ is a $N(\mu_j, \sigma_\epsilon^2)$ random variable truncated below zero. Per Battese and Coelli (1988), the μ_j and σ_ϵ^2 are:

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

and

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

where $\bar{\epsilon}_j = T^{-1} \sum_{t=1}^T \epsilon_{jt}$. (The right-hand side of Eq. 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),

² Since large n , small T is typical in panel datasets, perhaps timeinvariant technical inefficiency is the empirically relevant case. In what follows we only consider the time-invariant case.

yielding estimates $\hat{\beta}_j$, $\hat{\sigma}_{\epsilon_j}^2$, and $\hat{\sigma}_v^2$. Then, defining $e_{ij} = y_{ij} - x_{ij}'\hat{\beta}_j$; ‘‘estimation’’ of μ_{ij} and σ_{ij}^2 follows by substituting e_{ij} for ϵ_{ij} , $\hat{\sigma}_{\epsilon_j}^2$ for $\sigma_{\epsilon_j}^2$, and $\hat{\sigma}_v^2$ for σ_v^2 in Eqs. 2 and 3. Then, for a log-production function, the usual measure of technical efficiency based on a $N(\hat{\mu}_{ij}, \hat{\sigma}_{ij}^2)$ assumption is the conditional mean:

$$\begin{aligned}\hat{\theta}_j &= E(\exp\{-u_j\}|\bar{\epsilon}_j) \\ &= \exp\{-\hat{\mu}_{ij} + \frac{1}{2}\hat{\sigma}_{ij}^2\} \frac{1 - \Phi\left(\frac{\hat{\sigma}_{ij} - \hat{\mu}_{ij}}{\hat{\sigma}_{ij}}\right)}{1 - \Phi\left(\frac{-\hat{\mu}_{ij}}{\hat{\sigma}_{ij}}\right)},\end{aligned}\quad (4)$$

$j = 1, \dots, n.$

This is the sample equivalent of $\theta_j = E(\exp\{-u_j\}|\bar{\epsilon}_j)$, assuming that substitution of e_{ij} for ϵ_{ij} does not change the shape of the conditional distribution (or at least asymptotically). In the next section, we are interested in understanding how precisely $\hat{\theta}_j$ estimates θ_j and not how precisely $\hat{\theta}_j$ estimates μ_{ij} .³

Horrace (2005) argues that the point estimate in 4 is ‘‘misleading.’’ Granted the shape of the conditional distribution is truncated normal, 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, so one should also quantify the second moment by constructing confidence intervals (Horrace and Schmidt 1996) or calculating the variance of the truncated distributions (Bera and Sharma 1999). Ideally, one might calculate higher moments as well, particularly odd moments, which affect the probability of extreme realizations of inefficiency in clear ways.⁴ This suggests that the point estimate, $\hat{\theta}_j$, does not adequately account for (or inform our understanding of) the varying shape of the conditional distribution of u across firms.

Horrace (2005) addresses these shortcomings in $\hat{\theta}_j$ by calculating multivariate probabilities conditional on ϵ_j given that the distribution of u_j is truncated (at zero) normal. These probabilities are:

$$P_{\max}^j = \Pr\{u_j < u_i \quad \forall \quad i \neq j | \bar{\epsilon}_1, \dots, \bar{\epsilon}_n\} \quad j = 1, \dots, n, \quad (5)$$

$$P_{\min}^j = \Pr\{u_j > u_i \quad \forall \quad i \neq j | \bar{\epsilon}_1, \dots, \bar{\epsilon}_n\} \quad j = 1, \dots, n. \quad (6)$$

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 ($u_j < u_i \quad \forall \quad i \neq j$) 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’’ ($u_j > u_i \quad \forall \quad i \neq j$ in a probabilistic sense). Specifically, the probabilities are given by:

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

and

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

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

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

and

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

$u \in \mathbb{R}_+$, where Φ is the cumulative distribution function of the standard normal. The probabilities in Eqs. 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. In particular, they more adequately capture the effect of the shape of the

³ The question of ‘‘how precisely $\hat{\theta}_j$ estimates θ_j ?’’ is interesting, but it not addressed here.

⁴ For example, Feng and Horrace (forthcoming) consider the effects of the skewness of the technical inefficiency distribution on various technical efficiency estimates.

distribution on the magnitude of a firm's realization of u than the point estimates $\hat{\theta}_j$. Estimates of the probabilities, \hat{P}_{\max}^j and \hat{P}_{\min}^j follow by substituting estimates $\hat{\mu}_{u_j}$ and $\hat{\sigma}_u$ into Eqs. 5 and 6. (In the next section, we are interested in understanding how precisely \hat{P}_{\max}^j and \hat{P}_{\min}^j estimate P_{\max}^j and P_{\min}^j and not how precisely they estimate u_j .)

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 is often empirically relevant, as when the research question is about the most or least efficient firms within an industry. In addition, one may be interested in understanding relative performance among a subset of the sample of firms $j = 1, \dots, n$; based on a certain information criteria or decision rule. For example, one may be interested in estimating a cost function for a sample of 500 firms, but then only calculating probabilities of maximal cost efficiency for a small subset of the firms with an observable characteristic that is empirically relevant.⁵ The probabilities P_{\max}^j and P_{\min}^j will change as the cardinality of and the membership within this subset changes. Let $N = \{1, \dots, n\}$ be the set of all firm indices in the sample, and let the subset of interest be $J_{\Omega} \subset N$, based on some external information or decision rule, Ω . Then the probabilities in Eqs. 5 and 6 become:

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

and

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

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 Eqs. 5 and 6 are a special case of Eqs. 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 Eqs. 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 Eqs. 7 and 8 than they do from absolute efficiency measures like $\hat{\theta}$ in Eq. 4.

The next section examines the small and large sample performance of the estimates of P_{\max}^j , P_{\min}^j and θ_j via Monte Carlo analysis. For each estimate we calculate MSE and bias for various sample sizes, (n, T) , and various selections of σ_u^2/σ_v^2 . Reliability comparisons across the different measures are made using the unitless MAPE.

3. Monte Carlo Experiment

The specification used for the experiment is the production function:

$$y_{jt} = \beta_0 + \beta_1 x_{jt} + v_{jt} - u_{jt}. \quad (9)$$

Following Olsen et al. (1980), we fix the variance of the composed error term to $\sigma_u^2 = \sigma_v^2 + \sigma_w^2 = 1$. Hence, the individual variances of v_{jt} and u_{jt} may be characterized by a single parameter—we use the ratio $\gamma = \sigma_u^2/\sigma_v^2$. However, unlike the estimates in Olsen et al. (1980), the $\hat{\theta}_j$, \hat{P}_{\max}^j , and \hat{P}_{\min}^j are more complicated transformations of the data, so we cannot say immediately what the effect of changes in γ would be.⁶

While we estimate the production function in Eq. 9 for the entire sample, we only estimate the various efficiency measures for a subset of five randomly chosen firms. This is done primarily for ease of computation of P_{\max}^j and P_{\min}^j , which involve integration over a product of functions, one for each firm in the comparison group, but it also demonstrates the usefulness of the probabilities in Eqs. 7 and 8. In essence, we calculate b

$\hat{P}_{\Omega \max}^j$ and $\hat{P}_{\Omega \min}^j$ for $j \in J_{\Omega}$ where Ω is the rule ‘‘randomly select five firms from N .’’ Consequently, we only calculate five values of $\hat{\theta}_j$, $j \in J_{\Omega}$ in each simulation iteration for comparison. This randomization introduces an additional source of variability into the exercise, which may cause some instability in the convergence results, but the instability is the price we pay for computational ease. Fortunately, the additional variability is common to all estimators considered, so any instability will be globally manifest.

⁵ There is a price one pays when selecting a subsample based on some external rule. That is, the firms with a similar characteristic (e.g. large size) may have a different technology from those firms that do not have the characteristic. Empiricists may select or group the firms from the sample based on some rule, but different groups may have different technologies.

⁶ This is particularly difficult to predict for the efficiency probabilities.

3.1 Simulation procedure

The experiment is designed to assess $\hat{\theta}_j$, $\hat{P}_{\Omega \max}^j$, and $\hat{P}_{\Omega \min}^j$ (vis a vis θ_j , $P_{\Omega \max}^j$, and $P_{\Omega \min}^j$) over a range of common panel sizes (n and T) and variance ratios (γ). We use eight panel configurations: $T = 5$ and $n = 25, 100, 500$; $T = 10$ and $n = 25, 100, 500$; and $T = 20$ and $n = 25, 100$.⁷ In all cases we are concerned with the usual panel setting of large n and fixed T , so asymptotic arguments are along the dimension n . For each panel configuration we conduct simulation exercises for five variance ratios $\gamma = 0.1, 0.5, 1, 5, \text{ and } 10$, so there are forty simulations in total. For reasons discussed above, we fix the number of firms for calculation of $\hat{\theta}_j$, $\hat{P}_{\Omega \max}^j$, and $\hat{P}_{\Omega \min}^j$ to five (randomly selected from $N = 1, \dots, n$).⁸

Each iteration within a simulation exercise (indexed by $m = 1, \dots, M$), goes through the following sampling and estimation procedure, which is repeated $M = 5,000$ times. First, the errors u_{jm} and v_{jm} are drawn from the appropriate half-normal and normal distributions (with respective variances σ_u^2 and σ_v^2), and the regressors x_{jm} are drawn from an independent uniform $[0,1]$ distribution.⁹ Then y_{jm} is generated for $\beta_0 = 0$ and $\beta_1 = 1$ (the only parameterization of the conditional mean function considered). Since each ϵ_{jm} is observed, we can calculate the true values of μ_{jm} and σ_{*m} for each draw, m . These map into the true values for θ_{jm} , $P_{\Omega \max}^{jm}$, and $P_{\Omega \min}^{jm}$ for each m , so the ‘‘parameters’’ of interest are *not* constant across m . Estimation of $\hat{\theta}_{jm}$, $\hat{P}_{\Omega \max}^{jm}$, and $\hat{P}_{\Omega \min}^{jm}$ proceeds with corrected GLS (the ‘‘random effects’’ estimator).¹⁰ After estimating $\hat{\mu}_{jm}$ and $\hat{\sigma}_{*m}$ using ϵ_{jm} for ϵ_{jm} and $\hat{\sigma}_u^2, \hat{\sigma}_v^2$ for σ_u^2, σ_v^2 in Eqs. 2 and 3, five firms are randomly selected to produce the subset $J_{\Omega m} \subset N$. From these results we calculate estimates $\hat{\theta}_{jm}$, $\hat{P}_{\Omega \max}^{jm}$, and $\hat{P}_{\Omega \min}^{jm}$ for the five firms $j \in J_{\Omega m}$, using Eqs. 4, 7, and 8.

In what follows it is very important to remember that the *true values* θ_{jm} , $P_{\Omega \max}^{jm}$, and $P_{\Omega \min}^{jm}$, $j \in J_{\Omega m}$ are not fixed across iterations, m . (This should be clear, since all three of these measures are indexed by m .) This produces nonstandard formulae for the MSE, bias, and MAPE, although their interpretations are, indeed, standard. It also underscores the difficulties in estimating efficiency in these models: we are trying to make inferences about the distribution of efficiency for each firm from what amounts to a single draw from the distribution, and that single draw u_j is not even observed; it is merely ‘‘estimated’’ from the convolution, ϵ_{jm} .

With the results from the 5,000 iterations for each simulation exercise, we calculate the mean square error of $\hat{\theta}_j$, $\hat{P}_{\Omega \max}^j$, and $\hat{P}_{\Omega \min}^j$. Our nonstandard formula is (typically):

$$MSE(\hat{\theta}) = \frac{1}{5M} \sum_{m=1}^M \sum_{j \in J_{\Omega m}} (\hat{\theta}_{jm} - \theta_{jm})^2,$$

and similarly for $MSE(\hat{P}_{\Omega \max}^j)$, and $MSE(\hat{P}_{\Omega \min}^j)$.¹¹ Even though the MSE is nonstandard because it includes sampling variability across the true parameters (even asymptotically), it still seems theoretically sensible. As we shall see, it also produces results that are sensible. Again, this is an unavoidable feature of efficiency estimation from these models (in general).

For the bias and MAPE, we separately use only the best or worst firms within each five-firm subsample. This is necessary as the probability statements within a comparison group automatically sum to one (e.g., $\sum_{j \in J_{\Omega m}} \hat{P}_{\Omega \max}^{jm} = 1$), so there is no average bias for the whole group for these estimators. This is an artifact of their ‘‘relative nature’’ and perhaps a nice feature. More specifically, using the population ranking of u_{jm} among the five randomly selected firms, $u_{[1]m} < u_{[2]m} < \dots < u_{[5]m}$, we calculate the bias and MAPE of $\hat{P}_{\Omega \max}^{[1]m}$, $\hat{P}_{\Omega \min}^{[5]m}$, $\hat{\theta}_{[1]m}$, and $\hat{\theta}_{[5]m}$ for each iteration. Hence, the biases for each extremum measure are (typically):

$$Bias(\hat{\theta}_{[1]}) = \frac{1}{M} \sum_{m=1}^M (\hat{\theta}_{[1]m} - \theta_{[1]m})$$

and

$$Bias(\hat{\theta}_{[5]}) = \frac{1}{M} \sum_{m=1}^M (\hat{\theta}_{[5]m} - \theta_{[5]m}),$$

and similarly for $Bias(\hat{P}_{\Omega \max}^{[1]})$, and $Bias(\hat{P}_{\Omega \min}^{[5]})$.¹ We could have selected any firms in the ranking for this purpose

⁷We omitted $n = 500, T = 20$ to save computing time for the entire exercise.

⁸This also allowed us to indirectly examine the validity of the subset efficiency probabilities introduced in Eqs. 7 and 8.

⁹We could have allowed the x_{jtm} to be correlated within firms but did not.

¹⁰When CGLS fails due to $\hat{\sigma}_u^2 < 0$; we set $\hat{\sigma}_u^2 = 0$; per Waldman (1982).

¹¹We also calculated mean absolute error for each measure, but the results were similar to those for MSE and are not reported.

Table 1 Mean squared error, $T = 5$

n	Statistic	Variance ratio ($\gamma = \frac{\sigma_u^2}{\sigma_v^2}$)				
		0.1	0.5	1.0	5.0	10
25	<i>COLS failure rate</i>	0.3332	0.0454	0.0024	0	0
	$\hat{\theta}$	0.0223	0.0145	0.0065	0.0046	0.0051*
	$\hat{P}_{\Omega_{\max}}^j$	0.0031	0.0021	0.0011	0.0007	0.0007
	$\hat{P}_{\Omega_{\min}}^j$	0.0053	0.0030	0.0010	0.0003	0.0002
100	<i>COLS failure rate</i>	0.1624	0	0	0	0
	$\hat{\theta}$	0.0116	0.0020	0.0012	0.0010	0.0012*
	$\hat{P}_{\Omega_{\max}}^j$	0.0016	0.0003	0.0002	0.0002	0.0002
	$\hat{P}_{\Omega_{\min}}^j$	0.0030	0.0004	0.0002	0.0001	0.0001
500	<i>COLS failure rate</i>	0.0110	0	0	0	0
	$\hat{\theta}$	0.0025	0.0004	0.0002	0.0002	0.0002
	$\hat{P}_{\Omega_{\max}}^j$	0.0004	0.0001	0.0000	0.0000	0.0000
	$\hat{P}_{\Omega_{\min}}^j$	0.0007	0.0001	0.0000	0.0000	0.0000

* Indicates a non-monotonicity in γ . MSE's are calculated across all $j \in J_n$

(i.e., [2], [3] or [4]), but the best and the worst seemed appropriate for evaluating the performance of ranked estimators. Also, the extreme firms map into efficiency probabilities from the population that tend to be large, precluding a “divide-by-zero” problem in the MAPE calculation, as we shall see. $Bias(\hat{\theta}_{[1]})$ quantifies the extent to which the estimate of technical efficiency for the *most* efficient firm in the randomly selected subsample is mis-measured on average. Similarly, the $Bias(\hat{P}_{\Omega_{\max}}^j)$ quantifies the extent to which the estimate of the probability of being most efficient for the most efficient firm in the randomly selected subsample is mis-measured on average. Finally, since the units of θ_j and $P_{\Omega_{\max}}^j$ are different, the *MSE* and *Bias* measures are only relevant for making comparisons for a single measure (in isolation).

To make comparisons across measures we employ the unitless MAPE (typically):

$$MAPE(\hat{\theta}_{[w]}) = \frac{1}{M} \sum_{m=1}^M \left| \frac{\hat{\theta}_{[w]m} - \theta_{[w]m}}{\theta_{[w]m}} \right|, \quad w = 1, 5.$$

With the MAPE, we wish to avoid division by numbers close to zero, so we calculate it only for $\hat{P}_{\Omega_{\max}}^j$ and $\hat{P}_{\Omega_{\min}}^j$, the efficiency probability of the most efficient firm and the inefficiency probability of the least efficient firm, respectively, in the population. That is, efficiency probabilities like $\hat{P}_{\Omega_{\max}}^j$ and $\hat{P}_{\Omega_{\min}}^j$ may be very close to zero in the denominator of the MAPE formula, so it is only calculated for $\hat{P}_{\Omega_{\max}}^j$ and $\hat{P}_{\Omega_{\min}}^j$; which should both be fairly large in each draw. The results of the simulations and their discussion follow.

3.2 Results

First, the experiment shows that failure of the CGLS procedure ($\sigma_u^2 < 0$) is a problem only for extremely “noisy” variance ratios (small γ) and for small n in Tables 1, 2, 3. There are no failures with $\gamma > 1$, and with $\gamma = 1$ only a small number of failures (less than 1%) occur using the smallest sample $n = 25$, $T = 5$.

As expected, the *MSE* of all measures decreases with increasing n and fixed T . Of course, Tables 1, 2, 3 do not allow us to make comparisons across measures, since the units are different across measures. Also, it is not surprising that as the signal-to-noise ratio (σ_u^2/σ_v^2) increases, the *MSE* of the estimates is usually non-increasing, but not always. In Tables 1, 2, 3 the $MSE(\hat{P}_{\Omega_{\max}}^j)$; the average *MSE* of the probability that j is most efficient over $j = 1, \dots, 5$, is always non-increasing in γ . However, this is not true for the $MSE(\hat{\theta})$, the average *MSE* of the conditional mean of firm j over $j = 1, \dots, 5$, and the $MSE(\hat{P}_{\Omega_{\min}}^j)$; the average *MSE* of the probability that j is least efficient over $j = 1, \dots, 5$. For example, in Table 3 for $n = 25$ and moving from γ equal 1 to 5 to 10, the $MSE(\hat{\theta})$ is increasing from 0.0032 to 0.0048 to 0.0055. Similarly the $MSE(\hat{P}_{\Omega_{\min}}^j)$ is increasing across these γ 's in the same simulations. The non-monotonicities are highlighted with asterisks in Table 1, 2, 3. Why might these non-monotonicities in γ arise? It is well-known that the random effects estimator of β_1 is a weighted sum of the between estimator and the within (or

fixed effects) estimator (e.g., see Hsiao 1986 p36). The between estimator ignores the within firm variation, σ_u^2 , so when σ_u^2 is large the random effects

Table 2 Mean squared error, $T = 10$

n	Statistic	Variance ratio ($\gamma = \frac{\sigma_u^2}{\sigma_\epsilon^2}$)				
		0.1	0.5	1.0	5.0	10
25	<i>COLS failure rate</i>	0.1942	0.0026	0	0	0
	$\hat{\theta}$	0.0134	0.0052	0.0038	0.0045*	0.0051*
	$\hat{P}_{\Omega_{\max}}$	0.0028	0.0009	0.0006	0.0005	0.0004
	$\hat{P}_{\Omega_{\min}}$	0.0047	0.0007	0.0002	0.0001	0.0007*
100	<i>COLS failure rate</i>	0.0312	0	0	0	0
	$\hat{\theta}$	0.0041	0.0010	0.0009	0.0011	0.0013*
	$\hat{P}_{\Omega_{\max}}$	0.0009	0.0002	0.0001	0.0001	0.0001
	$\hat{P}_{\Omega_{\min}}$	0.0015	0.0001	0.0001	0.0000	0.0004*
500	<i>COLS failure rate</i>	0	0	0	0	0
	θ	0.0006	0.0002	0.0002	0.0002	0.0003*
	$\hat{P}_{\Omega_{\max}}$	0.0001	0.0000	0.0000	0.0000	0.0000
	$\hat{P}_{\Omega_{\min}}$	0.0002	0.0000	0.0000	0.0000	0.0002*

* Indicates a non-monotonicity in γ . MSEs are calculated across all $j \in J_\Omega$

Table 3 Mean squared error, $T = 20$

n	Statistic	Variance ratio ($\gamma = \frac{\sigma_u^2}{\sigma_\epsilon^2}$)				
		0.1	0.5	1.0	5.0	10
25	<i>COLS failure rate</i>	0.0572	0.0004	0	0	0
	$\hat{\theta}$	0.0063	0.0030	0.0032*	0.0048*	0.0055*
	$\hat{P}_{\Omega_{\max}}$	0.0019	0.0005	0.0004	0.0003	0.0003
	$\hat{P}_{\Omega_{\min}}$	0.0028	0.0002	0.0001	0.0006*	0.0051*
100	<i>COLS failure rate</i>	0	0	0	0	0
	$\hat{\theta}$	0.0011	0.0007	0.0008*	0.0012*	0.0014*
	$\hat{P}_{\Omega_{\max}}$	0.0003	0.0001	0.0001	0.0001	0.0001
	$\hat{P}_{\Omega_{\min}}$	0.0003	0.0000	0.0000	0.0002*	0.0031*

* Indicates a non-monotonicity in γ . MSEs are calculated across all $j \in J_\Omega$

estimator places more weight on the within variation and the random effect estimator is close to the fixed effect estimator. It is also well-known that the random effects estimator is asymptotically efficient relative to the fixed effects estimator (e.g., see Baltagi 2005 p17), so when σ_u^2 is very large, the random effects estimator may have a larger variance than when σ_u^2 is small. This imprecision feeds into the estimates $\hat{\theta}_j$, $\hat{P}_{\Omega_{\max}}$, and $\hat{P}_{\Omega_{\min}}$; so non-monotonicities in Tables 1, 2, 3 may reflect this lack of precision. Notice that they (highlighted with asterisks) occur primarily for the largest γ (and hence for largest σ_u^2).¹² Another factor that may induce the non-monotonicities is the size of σ_{u^*} , which appears as $-\mu_{*j}/\sigma_{u^*}$ in the formulae for the conditional mean and efficiency probabilities. For our simulations, the true value of σ_{u^*} reaches a maximum between $\gamma = 0.4$ and $\gamma = 0.7$ depending on the value of T . Obviously, smaller values of σ_{u^*} ceteris paribus inflate any error in the ratio μ_{*j}/σ_{u^*} so the estimators may be less precise for large γ .¹³

Why is the MSE of the $\hat{P}_{\Omega_{\max}}$ non-increasing in γ ? More accurately, why is the maximal efficiency probability immune to the variability of the random effects estimator when γ is large? Consider $\hat{P}_{\Omega_{\max}}$. When γ (and hence σ_u^2) is large, the probability of $u_{[1]} < u_j, j \neq [1]$ is large, so that differences in $\hat{\mu}_{[1]}$ and $\hat{\mu}_j, j \neq [1]$ tend to be large. The efficiency probabilities are based on differences of these means ($\hat{\mu}_{[1]} - \hat{\mu}_j$) and their relative variability.

¹² The imprecision may be worsen by the fact that the fixed effects estimator cannot exploit correlations between x and u , as they have not been built into the DGP.

¹³ Of course there is no way to disentangle this phenomenon from the effect of the random effects estimator approaching the fixed effects estimator, but it is interesting to note.

When the differences are large, the ability of the probabilities to distinguish the efficiency distributions is improved. It must

Table 4 Bias at maximal or minimal efficiency, $T = 5$

n	Statistic	Variance ratio ($\gamma = \frac{\sigma_1^2}{\sigma_2^2}$)				
		0.1	0.5	1.0	5.0	10
25	$\hat{\theta}_{[1]}$	0.0396	0.0251	0.0097	0.0068	0.0081
	$\hat{P}_{\Omega \max}^{[1]}$	-0.0043	-0.0092	-0.0050	-0.0017	-0.0029
	$\hat{\theta}_{[5]}$	0.0346	0.0436	0.0225	0.0077	0.0062
	$\hat{P}_{\Omega \min}^{[5]}$	-0.0082	-0.0161	-0.0084	-0.0009	-0.0009
100	$\hat{\theta}_{[1]}$	0.0281	0.0040	0.0026	0.0015	0.0003
	$\hat{P}_{\Omega \max}^{[1]}$	-0.0033	-0.0012	-0.0014	-0.0009	-0.0004
	$\hat{\theta}_{[5]}$	0.0297	0.0077	0.0058	0.0020	0.0010
	$\hat{P}_{\Omega \min}^{[5]}$	-0.0086	-0.0026	-0.0017	-0.0002	-0.0003
500	$\hat{\theta}_{[1]}$	0.0069	0.0004	0.0003	0.0004	0.0003
	$\hat{P}_{\Omega \max}^{[1]}$	-0.0011	-0.0003	-0.0002	-0.0002	0.0000
	$\hat{\theta}_{[5]}$	0.0079	0.0011	0.0008	0.0004	0.0003
	$\hat{P}_{\Omega \min}^{[5]}$	-0.0024	-0.0004	-0.0003	0.0000	0.0000

Table 5 Bias at maximal or minimal efficiency, $T = 10$

n	Statistic	Variance ratio ($\gamma = \frac{\sigma_1^2}{\sigma_2^2}$)				
		0.1	0.5	1.0	5.0	10
25	$\hat{\theta}_{[1]}$	0.0321	0.0102	0.0074	0.0041	0.0084
	$\hat{P}_{\Omega \max}^{[1]}$	-0.0071	-0.0050	-0.0041	-0.0021	-0.0025
	$\hat{\theta}_{[5]}$	0.0351	0.0213	0.0144	0.0055	0.0050
	$\hat{P}_{\Omega \min}^{[5]}$	-0.0131	-0.0071	-0.0025	-0.0004	-0.0026
100	$\hat{\theta}_{[1]}$	0.0105	0.0019	0.0016	0.0017	0.0015
	$\hat{P}_{\Omega \max}^{[1]}$	-0.0031	-0.0011	-0.0012	-0.0004	-0.0007
	$\hat{\theta}_{[5]}$	0.0137	0.0047	0.0033	0.0017	0.0011
	$\hat{P}_{\Omega \min}^{[5]}$	-0.0070	-0.0014	-0.0006	-0.0001	-0.0004
500	$\hat{\theta}_{[1]}$	0.0017	0.0003	0.0003	0.0006	-0.0003
	$\hat{P}_{\Omega \max}^{[1]}$	-0.0003	-0.0002	-0.0001	0.0001	-0.0001
	$\hat{\theta}_{[5]}$	0.0023	0.0008	0.0006	0.0002	0.0000
	$\hat{P}_{\Omega \min}^{[5]}$	-0.0010	-0.0001	-0.0001	-0.0001	-0.0009

be the case that this ability to distinguish outweighs the increased variability in the random effects estimator. Of course this phenomenon does not occur for $\hat{P}_{\Omega \min}^{[5]}$. Why? It may be related to approximation error in $\Phi(-\mu_{*j}/\sigma_*)$ caused by very large (in absolute value) μ_{*j}/σ_* . Since $\hat{P}_{\Omega \max}^{[1]}$ follows from relatively small μ_{*1}/σ_* ; it is immune to approximation error. In fact, absent approximation error, we believe that $\hat{P}_{\Omega \min}^{[5]}$ would exhibit the same monotonicities as $\hat{P}_{\Omega \max}^{[1]}$.

The results for the MSE in Tables 1, 2, 3 are similar (for the most part) to the Bias results in Tables 4, 5, 6, which are tabulated for extreme-efficiency firms ([1] and [5]) from the ranked subsample of five. As expected, the biases of all measures are generally non-increasing in n (in absolute value), and they are generally decreasing in γ with a few exceptions that are similar in nature to those of Tables 1, 2, 3. While the imprecision of the random effects estimator for large σ_w^2 manifests itself in the variance of the efficiency estimates and, hence, the MSE of each estimator

Table 6 Bias at maximal or minimal efficiency, $T = 20$

n	Statistic	Variance ratio ($\gamma = \frac{\sigma_u^2}{\sigma_\epsilon^2}$)				
		0.1	0.5	1.0	5.0	10
25	$\hat{\theta}_{[1]}$	0.0153	0.0053	0.0049	0.0034	0.0052
	$\hat{P}_{\Omega_{\max}}^{[1]}$	-0.0078	-0.0031	-0.0026	-0.0020	-0.0016
	$\hat{\theta}_{[5]}$	0.0233	0.0117	0.0087	0.0043	0.0043
	$\hat{P}_{\Omega_{\min}}^{[5]}$	-0.0157	-0.0027	-0.0010	-0.0013	-0.0019
100	$\hat{\theta}_{[1]}$	0.0032	0.0017	0.0015	0.0002	0.0002
	$\hat{P}_{\Omega_{\max}}^{[1]}$	-0.0013	-0.0010	-0.0007	-0.0006	0.0002
	$\hat{\theta}_{[5]}$	0.0055	0.0031	0.0021	0.0010	0.0010
	$\hat{P}_{\Omega_{\min}}^{[5]}$	-0.0031	-0.0006	-0.0001	-0.0009	-0.0017

(Tables 1, 2, 3), it may also affect the bias of the estimates in this exercise. To see this, remember that the nonstandard bias formula is not based on a fixed parameter across all 5,000 draws. Our formulation does not “average out” deviations around a fixed parameter, so the possibility for large deviations persists. These persistent deviations may appear as bias in our results. Notice also that the probability measures are almost always negatively biased, while the conditional mean measures are almost always positively biased. We suspect that this reversal comes from the fact that the probabilities are based on the distribution of u while the conditional means are based on the distribution of $\exp\{-u\}$. Across Tables 4, 5, 6, only $\hat{\theta}_{[5]}$ is uniformly improving in both n and γ (in the sense that the absolute value of the bias is non-increasing). However, comparisons of the bias across different measures is not possible due to inconsistency of the units of measure.

To make comparisons across different measures, mean absolute percentage errors (*MAPE*) for the extreme ends of the population order statistic are presented in Tables 7, 8, 9. Across all three tables the results are clear: $MAPE(\hat{P}_{\Omega_{\max}}^{[1]})$ is less than $MAPE(\hat{\theta}_{[1]})$ for values of $\gamma > 1$, and $MAPE(\hat{P}_{\Omega_{\min}}^{[5]})$ is less than $MAPE(\hat{\theta}_{[5]})$ for values of $\gamma > 0.1$. In other words, the probabilities are out-performing the conditional mean measures, when the variance of inefficiency, σ_u^2 , is large. For example in Table 7, $n = 25$, $\gamma = 5.0$, the *MAPE* for $\hat{\theta}_{[1]}$, $\hat{\theta}_{[5]}$, $\hat{P}_{\Omega_{\max}}^{[1]}$, $\hat{P}_{\Omega_{\min}}^{[5]}$ are 0.0890, 0.1633, 0.0688 and 0.0347, respectively. Our results are complicated by the fact that $MAPE(\hat{P}_{\Omega_{\max}}^{[1]})$ had extremely large values in some simulations with large γ . These instances are indicated in the tables with double asterisks (**) and were due to a few draws where the true values of $u_{[5]m}$ were so large, that they generated

Table 7 MAPE at maximal or minimal efficiency, $T = 5$

n	Statistic	Variance ratio ($\gamma = \frac{\sigma_u^2}{\sigma_\epsilon^2}$)				
		0.1	0.5	1.0	5.0	10
25	$\hat{\theta}_{[1]}$	0.1344	0.0911	0.0731	0.0890	0.1044
	$\hat{P}_{\Omega_{\max}}^{[1]}$	0.1960	0.1339	0.1119	0.0688	0.0616
	$\hat{\theta}_{[5]}$	0.1943	0.2170	0.1875	0.1633	0.1651
	$\hat{P}_{\Omega_{\min}}^{[5]}$	0.2455	0.1461	0.0912	0.0347	0.0266
100	$\hat{\theta}_{[1]}$	0.0865	0.0363	0.0336	0.0422	0.0494
	$\hat{P}_{\Omega_{\max}}^{[1]}$	0.1258	0.0582	0.0471	0.0355	0.0315
	$\hat{\theta}_{[5]}$	0.1255	0.0934	0.0862	0.0769	0.0780
	$\hat{P}_{\Omega_{\min}}^{[5]}$	0.1673	0.0565	0.0368	0.0176	0.0135
500	$\hat{\theta}_{[1]}$	0.0360	0.0157	0.0145	0.0192	0.0219
	$\hat{P}_{\Omega_{\max}}^{[1]}$	0.0579	0.0244	0.0209	0.0166	0.0137
	$\hat{\theta}_{[5]}$	0.0551	0.0405	0.0376	0.0347	0.0343
	$\hat{P}_{\Omega_{\min}}^{[5]}$	0.0727	0.0239	0.0165	0.0077	0.0053

Table 8 MAPE at maximal or minimal efficiency, $T = 10$

n	Statistic	Variance ratio ($\gamma = \frac{\sigma_v^2}{\sigma_e^2}$)				
		0.1	0.5	1.0	5.0	10
25	$\hat{\theta}_{[1]}$	0.0872	0.0561	0.0588	0.0925	0.1067
	$\hat{P}_{\Omega_{\max}}^{[1]}$	0.1650	0.0908	0.0718	0.0494	0.0389
	$\hat{\theta}_{[5]}$	0.1478	0.1398	0.1342	0.1447	0.1498
	$\hat{P}_{\Omega_{\min}}^{[5]}$	0.2065	0.0679	0.0409	0.0174	**
100	$\hat{\theta}_{[1]}$	0.0408	0.0253	0.0286	0.0458	0.0523
	$\hat{P}_{\Omega_{\max}}^{[1]}$	0.0822	0.0403	0.0345	0.0259	0.0216
	$\hat{\theta}_{[5]}$	0.0781	0.0641	0.0634	0.0714	0.0735
	$\hat{P}_{\Omega_{\min}}^{[5]}$	0.0989	0.0280	0.0184	0.0088	**
500	$\hat{\theta}_{[1]}$	0.0162	0.0114	0.0124	0.0201	0.0234
	$\hat{P}_{\Omega_{\max}}^{[1]}$	0.0335	0.0176	0.0151	0.0109	0.0092
	$\hat{\theta}_{[5]}$	0.0319	0.0279	0.0277	0.0313	0.0324
	$\hat{P}_{\Omega_{\min}}^{[5]}$	0.0403	0.0119	0.0081	0.0037	**

Table 9 MAPE at maximal or minimal efficiency, $T = 20$

n	Statistic	Variance ratio ($\gamma = \frac{\sigma_v^2}{\sigma_e^2}$)				
		0.1	0.5	1.0	5.0	10
25	$\hat{\theta}_{[1]}$	0.0493	0.0448	0.0561	0.0973	0.1130
	$\hat{P}_{\Omega_{\max}}^{[1]}$	0.1233	0.0638	0.0568	0.0339	0.0278
	$\hat{\theta}_{[5]}$	0.1073	0.1005	0.1070	0.1346	0.1464
	$\hat{P}_{\Omega_{\min}}^{[5]}$	0.1326	0.0301	0.0194	**	**
100	$\hat{\theta}_{[1]}$	0.0201	0.0220	0.0276	0.0474	0.0558
	$\hat{P}_{\Omega_{\max}}^{[1]}$	0.0507	0.0313	0.0268	0.0185	0.0142
	$\hat{\theta}_{[5]}$	0.0473	0.0476	0.0520	0.0662	0.0708
	$\hat{P}_{\Omega_{\min}}^{[5]}$	0.0496	0.0140	0.0093	0.0225	**

approximation errors in the computer calculations of the probabilities. (This is the same approximation error discussed for the MSE, but made worse since we are now selecting $\hat{\theta}_{[5]}$) This is an unfortunate feature of the probabilities, but it is purely computational in nature (i.e., it could be corrected with a more accurate algorithm for calculating Φ). As for monotonicities in the MAPE, all measures improve with n as expected. Both $\hat{P}_{\Omega_{\max}}^{[1]}$ and $\hat{P}_{\Omega_{\min}}^{[5]}$ appear to have MAPE non-increasing in γ as well, except in one case for $\hat{P}_{\Omega_{\min}}^{[5]}$ (and this may be due to approximation error in Φ). The MAPE of $\hat{\theta}_{[1]}$ and $\hat{\theta}_{[5]}$ usually reaches a minimum MAPE at or below $\gamma = 1$ in all panel configurations.

4. Conclusions

This study provides evidence on the sampling performance of two very different technical efficiency estimators that are used to assess absolute and relative firm-level efficiency, based on parametric stochastic frontier models. We find that both the traditional conditional mean estimates and the efficiency probabilities appear to be monotonically more precise as n increases. However, the effect of the variance ratio ($\gamma = \sigma_v^2/\sigma_e^2$) is more complicated. The efficiency probabilities out-perform the conditional mean when c is strictly greater than one. This is the empirically (and theoretically) important case for the frontier model. Our precision assessments are based on the unitless mean absolute percentage error, the only measure that could be used for comparison of these different estimators.

We are aware that we have introduced two other sources of variability in our study. One follows from the quantities of interest varying over m , and the other follows from our random sample of five firms for each m to calculate the measures of interest. The first source of variability could not be avoided and underscores the fact that efficiency “estimates” are not estimates of *traditional population parameters*. They are, in fact, proxies for an unobserved realization from inefficiency distributions. This is precisely the challenge that the frontier literature presents, and it is manifest in our study. The second source of variability was included by choice to relieve some

computational burden. However, this variability is purely random and affects all efficiency estimators in similar ways. Finally, approximation error in calculating $\hat{\theta}$ may have invalidated (or precluded) some simulation results for the largest values of τ , but the results for moderate values of τ are to be believed.

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