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QISHLOQ XO‘JALIGI FANLARI

COMPARING PARAMETRIC AND NON-PARAMETRIC EFFICIENCY MEASUREMENT UNDER DATA QUALITY CONSTRAINTS: STOCHASTIC FRONTIER AND DEA EVIDENCE FROM SMALLHOLDER SWEET SORGHUM FARMS IN UZBEKISTAN

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Abstract. Agricultural economists must choose between stochastic frontier analysis (SFA) and data envelopment analysis (DEA) when assessing efficiency — the two methods treat measurement error differently, which matters greatly for smallholder survey data where reporting error is common. We test this on 98 sweet sorghum farms in Karakalpakstan, Uzbekistan. A Cobb-Douglas SFA model on four inputs (land, labor, seed, irrigation water) yields a plausible labor elasticity (0.428, p=0.002) and a large negative water elasticity (−2.744, p<0.001), but finds no meaningful inefficiency (gamma≈0.005; LR test p=0.545/1.000), with efficiency scores nearly identical across farms (mean 0.9916, SD 0.00016). A DEA model on the same data tells a different story: efficiency ranges from 0.567 to 1.000 (mean 0.883), and scale efficiency varies significantly with soil salinity (Kruskal-Wallis p=0.0007) — a relationship SFA could not detect. This gap follows mechanically from how each method handles noise: SFA attributes most variation to noise, while DEA treats every deviation as inefficiency. A smaller 16-farm sample further shows DEA loses discriminatory power as unit count shrinks. We close with practical recommendations for working with imperfect field data.
Keywords: stochastic frontier analysis; data envelopment analysis; technical efficiency; measurement error; smallholder agriculture; sorghum.

Annotatsiya. Qishloq xo'jaligi iqtisodchilari samaradorlikni baholashda stoxastik chegara tahlili (SFA) va ma'lumotlarni qamrab olish tahlili (DEA) o'rtasida tanlov qiladi — bu ikki usul o'lchov xatoligiga turlicha yondashadi, bu esa hisobot xatolari keng tarqalgan kichik xo'jaliklar ma'lumotlarida muhim ahamiyat kasb etadi. Qoraqalpog'istondagi 98 ta shakarli jo'xori fermer xo'jaligi namunasida buni tekshiramiz. To'rtta kirish resursi (yer, mehnat, urug', sug'orish suvi) asosidagi Kobb-Duglas SFA modeli ishonarli mehnat elastikligini (0,428, p=0,002) va katta manfiy suv elastikligini (−2,744, p<0,001) ko'rsatadi, biroq sezilarli samarasizlikni aniqlay olmaydi (gamma≈0,005; LR-test p=0,545/1,000), samaradorlik ko'rsatkichlari esa deyarli bir xil (o'rtacha 0,9916, standart og'ish 0,00016). Xuddi shu ma'lumotlar asosidagi DEA modeli boshqacha natija beradi: samaradorlik 0,567–1,000 oralig'ida farqlanadi (o'rtacha 0,883), masshtab samaradorligi esa tuproq sho'rlanishi bilan sezilarli bog'liq (Kraskal-Uollis p=0,0007) — bu bog'liqlikni SFA aniqlay olmagan. Bu farq mexanik ravishda ikki usulning shovqinga yondashuvidan kelib chiqadi: SFA farqlarni asosan shovqinga, DEA esa har qanday og'ishni samarasizlikka bog'laydi. 16 ta xo'jalikdan iborat kichik namuna DEA ning yana bir kamchiligini ko'rsatadi — birliklar soni kamayganda uning farqlash qobiliyati pasayadi. Yakunda nomukammal dala ma'lumotlari bilan ishlash uchun amaliy tavsiyalar beramiz.
Kalit so'zlar: stoxastik chegara tahlili; ma'lumotlarni qamrab olish tahlili; texnik samaradorlik; o'lchov xatoligi; kichik fermer xo'jaligi; qand jo'xori.

Аннотация. Экономистам-аграрникам приходится выбирать между стохастическим анализом границы (SFA) и анализом среды функционирования (DEA) при оценке эффективности — оба метода по-разному учитывают ошибки измерения, что особенно важно для данных мелких хозяйств, где ошибки отчётности распространены. Мы проверяем это на 98 хозяйствах, выращивающих сахарное сорго в Каракалпакстане. Модель SFA Кобба-Дугласа по четырём факторам (земля, труд, семена, вода) даёт правдоподобную эластичность труда (0,428, p=0,002) и большую отрицательную эластичность воды (−2,744, p<0,001), но не выявляет значимой неэффективности (гамма≈0,005; тест LR p=0,545/1,000), а оценки эффективности почти одинаковы для всех хозяйств (среднее 0,9916, ст. откл. 0,00016). Модель DEA на тех же данных

даёт иную картину: эффективность варьируется от 0,567 до 1,000 (среднее 0,883), а эффективность масштаба значимо связана с засолённостью почвы (Краскел-Уоллис, p=0,0007) — связь, которую SFA не смогла обнаружить. Это расхождение механически следует из того, как каждый метод обрабатывает шум: SFA относит большую часть вариации на шум, DEA же трактует любое отклонение как неэффективность. Меньшая выборка из 16 хозяйств показывает, что DEA теряет различительную способность при уменьшении числа единиц. В заключение даём практические рекомендации по работе с несовершенными полевыми данными.
Ключевые слова: стохастический анализ границы; анализ среды функционирования; техническая эффективность; ошибка измерения; мелкое сельское хозяйство; сахарное сорго.

INTRODUCTION

Two families of method dominate the technical-efficiency literature in agricultural economics. Stochastic frontier analysis (SFA), proposed independently by Aigner, Lovell and Schmidt (1977) and Meeusen and van den Broeck (1977), assumes a functional form for the production technology and splits the gap between observed and frontier output into two pieces: random noise and inefficiency. Data envelopment analysis (DEA), due to Charnes, Cooper and Rhodes (1978) and later extended to variable returns to scale by Banker, Charnes and Cooper (1984), makes no such assumption — it builds the frontier directly from the best-performing units in the sample and attributes any shortfall entirely to inefficiency. Textbooks are clear about this distinction. What gets less attention is what it actually does to a study's conclusions once real, noisy survey data are involved — and self-reported cost and input data from smallholder farms are about as noisy as agricultural data gets (Baiyegunhi & Fraser, 2009).

This paper works through that question using a survey of 98 sweet sorghum farms in Karakalpakstan, Uzbekistan. As we show below, the cost data these farmers reported have an obvious problem: labor costs are missing entirely, even though labor use itself was recorded in full. Running both SFA and DEA on the same production data, we find that the two methods do not just disagree at the margins — they disagree about whether there is any inefficiency to measure in the first place. That disagreement is the point of the paper. Method choice here is not a matter of taste; it determines whether a study has a result to report at all, and that is a hazard that anyone working with self-reported smallholder data should probably know about before they commit to one method over the other.

Three things come out of this exercise. One is simply a same-data, side-by-side comparison of SFA and DEA in a Central Asian smallholder setting, which is not a context the efficiency-measurement literature has said much about. Another is a fairly concrete demonstration — with actual numbers, not just the usual textbook caveat — of how measurement error moves through the two estimators differently. The third is a cautionary note on sample size: a 16-farm subsample lets us show, rather than just assert, how quickly DEA's discriminatory power breaks down as the number of units shrinks toward the rule-of-thumb minimum.

LITERATURE REVIEW

The SFA production function has a composite error term, $\varepsilon = v - u$. The symmetric part, v, is ordinary noise — measurement error, weather, luck — and

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is usually assumed normal. The one-sided part, u, is inefficiency, and can be specified as half-normal, truncated-normal, or exponential (Aigner, Lovell, & Schmidt, 1977; Battese & Coelli, 1995). How much of the total variance belongs to each component is summarized by $\gamma = \sigma_u^2/(\sigma_u^2+\sigma_v^2)$. A γ near zero says the model is attributing almost everything to noise; near one, almost everything to inefficiency. Battese and Coelli (1995) extended the basic model so that farm characteristics can explain the mean of the inefficiency distribution in a single estimation step, though this one-step version requires software that is not available in every Stata installation — a practical constraint we return to below.

DEA builds a piecewise-linear frontier out of whichever units in the sample happen to perform best, and scores everyone else by their distance to it (Charnes, Cooper, & Rhodes, 1978). Banker, Charnes and Cooper's (1984) variable-returns-to-scale (VRS) version drops the assumption that doubling inputs must double output, which matters in smallholder agriculture where farm size varies a great deal. The method's weak point is well known: because any single observation can end up defining part of the frontier, one badly reported input or output value can distort the scores of everyone compared against it (Charnes, Cooper, & Rhodes, 1978; Cooper, Seiford, & Tone, 2007). A related problem shows up with small samples — Cooper, Seiford and Tone (2007) suggest n should be at least max(m×s, 3×(m+s)), with m and s the numbers of inputs and outputs, and below that threshold DEA tends to call an implausible share of units “efficient” for lack of anything to compare them against.

Studies that run both methods on the same data are less common than studies that run one or the other. When they do exist, the conclusions vary with the setting: sometimes the two methods rank units similarly, sometimes they do not, and where they diverge the usual explanation is DEA's inability to separate signal from noise (see the discussion in Coelli, Rao, O‘Donnell, & Battese, 2005). Sweet sorghum itself does not appear to have an efficiency study of either kind yet. The nearest indirect evidence is the repeated observation, in Baiyegunhi and Fraser (2009) and Gautam et al. (2021), that family labor tends to disappear from smallholder cost accounts — a data problem that would presumably affect any efficiency estimate built on top of it, regardless of method.

Two expectations follow. Given the cost-reporting problems documented below, SFA should push more of the observed variation into the noise term than DEA does — possibly enough that no meaningful inefficiency is left to report (H1). And DEA results from the smaller of our two subsamples should show visible signs of the discriminatory-power problem described above (H2).

MATERIALS AND METHODS

The data come from the same 98-farm survey of sweet sorghum growers in Karakalpakstan, Uzbekistan used in a companion paper on this crop's value chain (see that paper's Section 3.1 for full sampling detail). Before running either efficiency model, we checked the reported cost figures against the underlying line items and found them hard to reconcile: itemized costs for seed, fertilizer, fuel and transport summed to roughly 4% of the reported total, and the questionnaire had no field at all for the monetary value of labor, despite recording labor use in physical terms (a median of 3 permanent and 9 seasonal workers per farm). This is the data problem that motivates the comparison that follows.

Both models use the same four inputs — land (ha), labor (permanent plus seasonal workers), seed (kg), and irrigation water (m^3) — and one output, yield (kg/ha), converted from the tonnage reported in the survey. We ran DEA separately for grain farms in Karakalpakstan (n=82) and green-mass farms in Tashkent region (n=16) rather than pooling them: yields differ by an order of magnitude between the two (roughly 4 t/ha versus 62 t/ha), and pooling them in an earlier pass produced an efficiency gap that tracked the underlying technology rather than anything a farmer actually controlled. The grain subsample is well above Cooper, Seiford and Tone's (2007) minimum (n≥15 for this many inputs and outputs) and carries the main results; the green-mass subsample sits right at that minimum and is used only to illustrate what happens near the threshold.

We estimated a Cobb-Douglas stochastic production frontier:

$$ln(Yi) = \beta0 + \beta1ln(Land_i) + \beta2ln(Labor_i) + \beta3ln(Seed_i) + \beta4ln(Water_i) + v_i - ui$$

with vi ~ N(0,σv^2), and ui specified both as truncated-normal and, as a robustness check, half-normal, via maximum likelihood in Stata's frontier command. Under each specification we tested H0: γ=0 with a likelihood-ratio test, which is the standard way of asking whether the data support an inefficiency component at all.

The DEA model is input-oriented, estimated under both VRS and constant returns to scale (CRS) with the same four inputs and one output, using Ji and Lee's (2010) Stata routine. Input orientation made more sense here than output orientation: a farmer decides how much seed, water and labor to use, but does not decide the weather. Dividing the CRS score by the VRS score for each farm gives a scale-efficiency measure, separating out how much of a farm's inefficiency, if any, is simply a matter of operating at the wrong size.

We used Kruskal-Wallis tests to check whether SFA-derived efficiency scores and DEA-derived VRS and scale-efficiency scores moved together with soil salinity category, which lets us ask whether the two methods point to the same substantive story about what drives efficiency, quite apart from how much variation each method finds in the first place.

RESULTS

SFA results.

Table 1

Stochastic frontier estimates (Cobb-Douglas, truncated-normal specification)

Variable	Coefficient	p-value
ln(Land)	0.0153	0.877
ln(Labor)	0.4276	0.002
ln(Seed)	0.1200	0.503

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ln(Water)	−2.7436	0.000
Constant	29.561	0.000
gamma	0.0046	—
LR test (H0: γ=0)	z=0.114	0.545

Labor and water come out significant; land and seed do not, which is plausible enough given how collinear these inputs tend to be on farms this size. The water coefficient deserves a second look: it is large, negative, and unlikely to be a pure production effect. A companion analysis of this same sample found that farms using more water tend to be on more saline land requiring more frequent irrigation without a matching yield gain, and the coefficient here is probably picking up some of that. What matters most for this paper, though, is the test for inefficiency itself, and it simply does not go the way one might expect: p=0.545 under the truncated-normal specification, and p=1.000 once we tried a half-normal one instead. The predicted efficiency scores tell the same story in a different way — mean 0.9916, standard deviation 0.00016, a range of 0.00069 across all 98 farms. There is nothing here to rank, and nothing to explain.

DEA results (grain subsample, n=82)/

Table 2

DEA efficiency scores (grain, Karakalpakstan, n=82)

Measure	Mean	Median	Std. Dev.	Min	Max
Technical efficiency (VRS)	0.883	0.904	0.094	0.692	1.000
Technical efficiency (CRS)	0.526	0.490	0.161	0.238	1.000
Scale efficiency (SE)	0.599	0.574	0.176	0.286	1.000

Here the picture is the opposite of Section 4.1: real spread, real ranking. Twenty of the 82 farms (24.4%) sit on the efficient frontier; the rest trail off down to 0.567–0.692, which is a wide enough range to actually say something about.

Table 3

DEA efficiency by soil salinity category (grain subsample)

Salinity level	n	VRS (median)	SE (median)
Low	6	0.883	0.457
Moderate	41	0.857	0.623
High	35	0.938	0.514
Kruskal-Wallis		χ^2=4.264, p=0.119	χ^2=14.447, p=0.0007

Technical efficiency itself does not track salinity in any statistically convincing way (p=0.119), but scale efficiency does, and not in a straight line — farms with moderate salinity score highest, while both the least and the most salt-affected farms score lower. SFA had nothing to say here, for the simple reason that it found no inefficiency to begin with. What the pattern seems to say is that salinity is pushing farms toward the wrong operating scale rather than making them worse managers, a distinction the Cobb-Douglas specification used here is not built to pick up.

Small-sample DEA behavior (green-mass subsample, n=16).

Run on the 16-farm Tashkent subsample, sitting right at the recommended minimum, the same VRS model calls 10 of 16 farms (62.5%) fully efficient — well over double the 24.4% figure from the much larger grain subsample. Mean VRS and scale-efficiency scores here (0.986 and 0.972) are implausibly high for the same reason: with so few farms to compare against, DEA runs out of rivals long before it runs out of "efficient" units to declare. This is exactly the small-sample problem Cooper, Seiford and Tone (2007) warn about, and it is worth taking seriously rather than treating as a footnote.

Direct comparison.

Table 4

Summary comparison of SFA and DEA results

Criterion	SFA	DEA (grain, n=82)
Functional form required	Yes (Cobb-Douglas)	No
Noise/inefficiency separated	Yes (by construction)	No
Inefficiency detected	No (γ≈0, p=0.545–1.000)	Yes (range 0.567–1.000)
Salinity–efficiency relationship detectable	No	Yes (via scale efficiency, p=0.0007)
Sensitivity to small samples	Not directly comparable (MLE requires asymptotics)	High (62.5% "efficient" at n=16)

DISCUSSION

Why the two methods diverge.

A small example makes the mechanism easier to see than the equations do. Take five farms that use identical inputs. Four of them report roughly the same output; the fifth, for no reason more interesting than a bad guess at harvest weight, reports 50% more. DEA takes that fifth number at face value and puts it on the frontier — which then makes the other four, who reported honestly, look 33% inefficient, even though nothing about how they actually farm is any different. SFA handles the same five farms differently: fitting a frontier to the central tendency of the whole group, it reads the fifth farm as an unlikely draw from the noise distribution rather than as the new standard to beat, and leaves the other four close to fully efficient. Scale that story up to 98 farms with the kind of reporting problems documented in Section 3.1, and the pattern in our results — SFA finding almost no inefficiency, DEA finding quite a lot — stops looking like a puzzle and starts looking like exactly what the theory predicts.

None of this means the DEA numbers are wrong, exactly. It is probably more accurate to read them as an upper bound on inefficiency plus noise combined, rather than a clean read on inefficiency alone. Where a DEA result lines up with something we would expect on independent grounds — the salinity pattern here is the clearest case — that is some reassurance that real signal is in there somewhere. Where a small subsample calls most of its units efficient with no such supporting pattern, as happened with the green-mass farms, that reassurance is not available, and the results are better treated as illustrative than as a finding.

Implications for method choice in smallholder settings.

A few things follow for anyone working with self-reported farm data of the kind that is standard in this literature (Baiyegunhi & Fraser, 2009; Gautam et al., 2021). An SFA model that fails to find inefficiency is not necessarily telling you there is none — it may just be telling you that measurement error is swamping the signal, which is a real risk in modestly sized cross-sections where the variance components are hard to pin down separately to begin with. A DEA score, meanwhile, is safer read as a ceiling than as a point estimate, particularly without some independent check like the salinity pattern we happened to have here. And before reporting DEA results at all, it is worth confirming the sample clears Cooper, Seiford and Tone's (2007) rule of thumb; where it barely does, as with our 16-farm subsample, that should be said plainly rather than left for the reader to work out.

This is one survey, one crop, one year. Whether the SFA/DEA gap we found here would show up elsewhere, or would narrow with panel data that could separate noise from a farm's persistent inefficiency over time, is not something these data can answer. We also did not manage to run the one-step Battese-Coelli (1995) inefficiency-effects model, which would have let us estimate inefficiency determinants jointly with the frontier, because the software it requires was not available to us; a two-step version was possible in principle but pointless in practice, given that the SFA efficiency scores have almost no variance left to explain.

CONCLUSION

Run on the same smallholder survey data, SFA and DEA arrived at almost opposite conclusions about whether sweet sorghum farms in Karakalpakstan differ in technical efficiency at all. SFA, built to separate noise from inefficiency, found essentially none of the latter in a dataset with well-documented cost-reporting problems. DEA, which cannot make that separation, found a wide spread of efficiency scores that lined up sensibly with an independent measure — soil salinity — in the larger of our two subsamples, and looked considerably less trustworthy in the smaller one. Neither method comes out of this as simply the right choice.

The broader point is not really about sweet sorghum, or even about Uzbekistan. Most primary agricultural surveys in developing-country settings share the same features that drove the divergence here: small-to-moderate samples, self-reported costs, and labor that farmers rarely price in the way an economist would want them to. Under those conditions, a researcher who runs only SFA risks concluding, wrongly, that a farming population is uniformly efficient simply because the model had nowhere to put the noise except into the inefficiency term's complement; a researcher who runs only DEA risks reporting inefficiency estimates that are really measurement error in disguise, especially with samples near the discriminatory-power threshold. Running both, and reporting where they agree and where they do not, costs little beyond an extra

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afternoon of estimation and is considerably more honest about what the data can and cannot support. Future work with panel data — allowing noise and time-invariant inefficiency to be separated in a way a single cross-section cannot manage — would be the natural next step for testing how much of the gap reported here survives a longer observation window.

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