A Fool's Errand? The Inverse Productivity Relationship Reconsidered*

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Abstract

An inverse unconditional relationship between farm or plot size (e.g., hectares) and productivity (e.g., kilograms per hectare) is often observed in low- and middle-income countries that appears to be at odds with economic theory. The traditional approach to studying the inverse relationship regresses yield (i.e., output divided by size) on size as well as control variables, testing the null hypothesis that the coefficient on size is zero. We first show that in many circumstances, the relevant null hypothesis is misspecified because the estimand cannot be zero. We further identify the stringent requirements that need to be satisfied to correctly estimate the relationship. Moreover, because size appears on both sides of the equation—indirectly on the left-hand side as denominator, and directly on the right-hand side as a measure of size—inherent issues arise with the identification of the relationship between size and productivity. Specifically, any measurement error in land or unobserved production factor, even if independent from size, will introduce bias in the estimated coefficient. We therefore highlight persistent methodological flaws and contradictions in the literature on the inverse size–productivity relationship, discussing how better controls and more precise measurements are unlikely to ensure unbiased estimates. Finally, we conduct a meta-analysis of the literature on the inverse relationship, discussing the evolution of empirical specifications and documenting evidence of publication bias in favor of negative and significant estimates of the relationship between size and productivity.

Keywords: Inverse Relationship, Internal Validity, Construct Validity, Publication Bias

JEL Codes: O13, Q12, Q18

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1 Introduction

In Shirley Jackson's 1948 short story "The Lottery," the residents of a small fictional American town come together every year to select a town resident at random and stone them to death.

Jackson's reader is never told why this is so. Ostensibly, the author's intention was to criticize those who blindly follow tradition, and whose explanations for doing the things that they do is "because that is how we have always done things."

Science is not immune to this type of thinking. Every discipline has its own examples of research agendas whose methods or questions persist because the methods used and the questions posed are the same methods used and the same questions posed by those who came before.

We show how the study of the inverse relationship between size and productivity, which has been shown to be an empirical regularity at both the farm and plot levels in low- and middle-income countries (LMICs), is such an example—one where both the question and the methods used to answer it persist.

The inverse size–productivity relationship was first documented for Russian agriculture by Alexander V. Chayanov in his 1926 *Theory of Peasant Economy* (Chayanov, 1986). It was then documented for Indian agriculture and studied by luminaries such as 1998 economics Nobel laureate Amartya K. Sen (Sen, 1962, 1966), and it has since been documented and studied in many different LMIC contexts, spawning a considerable literature at the intersection of agricultural and development economics.

Beyond looking at unconditional relationships, a great deal of the empirical literature on the inverse relationship tests for that relationship by regressing yield y (usually output q divided by farm or plot size a, or q/a) on a vector of ones (so that the regression includes a constant), farm or plot size a, and a vector of control variables x (which may or may not include inputs such as capital, labor, and so on). In this paper, when other inputs are controlled for, we will talk of the *structural* approach to studying the inverse relationship. When no other inputs are controlled for, we will talk of the *reduced-form* approach to studying the inverse relationship.[1] An inverse relationship is said to be found if the estimate of the size coefficient β is negative and statistically different from zero.[2]

We show that the use of a ratio variable (i.e., yield, or output divided by size) as the dependent variable leads to two fatal problems in that both (i) the estimand (i.e., the target of estimation β) will be misspecified, and (ii) the estimate (i.e., $\hat{\beta}$) will be biased. Specifically, under the maintained assumption of constant returns to scale (CRS), researchers test the null hypothesis H_0: $\beta = 0$ against the alternative hypothesis H_A: $\beta \neq 0$, but (i) the model is misspecified and β rarely equals zero even under the assumption of CRS, and (ii) even if $\beta = 0$, the presence of unobserved confounders and measurement error in size means that $\hat{\beta} \neq 0$. In many circumstances, this makes the study of the inverse size–productivity relationship akin to a fool's errand. Without addressing the model misspecification problem first, even studies with a cutting-edge empirical design will be misguided in their endeavor to recover a zero coefficient.

One strand of recent studies aims to measure output, size, or both more precisely relative to the self-reported measures used in earlier studies. These recent studies find that the inverse relationship attenuates with more precise measurements of yield such as full plot or subplot crop cut (Desiere and Jolliffe, 2018; Ayalew et al., 2023) and remotely sensed measures (Gourlay, Kilic and Lobell, 2019), as well as more precise measurements of size such as GPS (Burke et al., 2023; Carletto, Savastano and Zezza, 2013) or compass-and-rope measurements (Dillon et al., 2019) measurements. Yet the underlying true coefficient is unlikely to be zero due to the use of ratio variables. This explains why some of these studies find negative, though insignificant, coefficients (Ayalew et al., 2023), others find the inverse relationship strengthens (Burke et al., 2023; Carletto, Savastano and Zezza, 2013), and yet others find that it becomes positive (Desiere and Jolliffe, 2018).

[1] Instead of structural and reduced-form approaches, Barrett, Bellemare and Hou (2010) talk of production function and yield approaches. This is confusing, however, since what they dub "production function" is not a production function in the usual sense in that it regresses yield—not output—on inputs.

[2] For the remainder of this paper, we use "size" as shorthand for "farm or plot size".

While the estimates in the aforementioned studies improve upon estimates of the relationship between size and productivity by improving on *construct validity* (i.e., by using more precise measurements), other studies improve upon those same estimates by improving on *internal validity* (i.e., by controlling for a hitherto omitted variable correlated with size). Bevis and Barrett (2020), for instance, find that because plots are more productive closer to their boundaries, the negative, statistically significant relationship between productivity and becomes insignificant when controlling for the size of a plot's boundary relative to the size of the plot itself. A number of other studies look into input intensity differences across farm sizes because of market imperfections or transaction cost (Heltberg, 1998; Barrett, Bellemare and Hou, 2010; Deininger et al., 2018; Sheng, Ding and Huang, 2019; Ayaz and Mughal, 2022; Foster and Rosenzweig, 2022) or soil quality differences across farm sizes (Lamb, 2003; Barrett, Bellemare and Hou, 2010). While the inclusion of such omitted variables are well-justified and does change the estimated coefficient in theory and in practice, the effort is incomplete and cannot realistically be completed: without satisfying stringent conditions, the true coefficient is not zero and the estimate is prone to bias. Therefore, it is likely that studies in this group fail to reject the null only by chance.

Our contribution is threefold. First, we show that what is measured by the coefficient on size depends on model specification. More importantly, we also show that the estimand (or the target of estimation) is rarely zero.

Indeed, in a regression without input controls (i.e., the reduced-form approach), the estimated coefficient is a combination of (i) returns to scale minus one, (ii) the relationship between the total factor productivity (TFP) and farm size, and (iii) the relationship between input intensity and farm size. In a regression with input controls (i.e., the structural approach), the coefficient is equal to returns to scale minus one, but this only equals zero when the technology exhibits CRS and *all* inputs are controlled for—the latter of which is unfortunately never the case in the literature. In a linear regression without input controls, the coefficient is a combination of the underlying production function intercept and the relationship between input intensity and farm size. Finally, in the structural approach, the coefficient is zero only if the underlying production function intercept is zero—something that is not at all guaranteed even if the theoretical value of that intercept is zero—and if, once again, *all* inputs are controlled for.

Given the compound nature of the size coefficient in a reduced-form model, and the stringent requirement for correctly specifying a structural model, previous studies that focus on only one or two components of the compound estimate or which estimate a structural model with limited data on input intensity are prone to misinterpreting their findings.

Second, we show that the use of a ratio variable means that even classical measurement error in size will introduce bias. While it is well-established that measurement error in size is largely nonclassical (i.e., it varies systematically with size itself) (Abay et al., 2023, 2019; Carletto, Gourlay and Winters, 2015), our focus on classical measurement error makes it clear that efforts to eliminate nonclassical measurement error by using advanced measurement technologies will not guarantee that bias is eliminated. In addition, any omitted production factors—even those that are *not* correlated with the right-hand side variables—will lead to bias in a linear regression.

Third, we conduct a meta-analysis to show that the use of ratio variable is pervasive, even though its pitfalls have been addressed by researchers in related disciplines for nearly a half century (Kronmal, 1993; Firebaugh and Gibbs, 1985; Borjas, 1980). Among studies with a ratio variable as the dependent variable, we find that the estimate of the size is sensitive to the inclusion of input intensity variables, as predicted. We identify substantial publication bias towards negative estimates. Worse, eliminating publication bias would not yield a zero estimate because of the built-in issues caused by the use of a ratio variable. Our meta-analysis differs substantially from existing ones (Garzón Delvaux, Riesgo and Gomez y Paloma, 2020; Ricciardi et al., 2021) which synthesize findings taking the empirical methods in those studies as given. Rather, we examine the choice of method and how that affects findings.

Finally, while some of the problems we identify have no solution and can make the study of the inverse relationship a fruitless endeavor, we do propose solutions to some of those problems. To begin with, researchers should motivate their modeling choices based on what they are trying to estimate, as different model specifications lead to different conceptual interpretations. Second, whether researchers are interested in correctly describing the unconditional or the conditional relationship between size and yield, they should avoid using ratio variables that entail the special empirical challenges we laid out. Instead, they should use other yield measurements that do not involve dividing by a right-hand side variable, such as the crop cuttings of sample squares used in recent studies (Ayalew et al., 2023).

Then, to explain the estimated relationship—whether it is negative, positive, or zero—researchers will have to directly estimate the underlying production function and test whether the Cobb-Douglas production function exhibits constant returns to scale or the linear production function intercept is zero. If they are not, there is no use in attributing the relationship to other drivers. Lastly, it is unrealistic to make a causal claim based on the estimated relationship, given the challenge of correctly measuring all types of inputs (e.g., labor; see Arthi et al. (2018)). Even if researchers can randomly assign farm or plot size, farmers will adjust their inputs based on some unobserved expectations about the productivity of the land.

The remainder of this paper is organized as follows. Section 2 analytically shows how the interpretation of the size coefficient changes with model specification and the specific requirements for its true value to be zero. In Section 3 we demonstrate our arguments on the basis of simple simulations and show how a spurious inverse relationship can arise between size and productivity even in the absence of unobserved confounders and measurement error. In Section 4 we discuss two types of special empirical challenges to identifying the coefficient on size, regardless of its true value. In Section 5, we report the results of a meta-analysis of the literature with a focus on summarizing how method choice affects estimation results, and we report evidence of publication bias. Section 6 concludes with recommendations for both policy and research.

2 Analytical Derivations

In this section, we derive what is measured by the inverse size–productivity relationship coefficient and the conditions for it to be zero. We discuss the two approaches that have been used in the literature, which we dub the *structural* and *reduced-form* approaches to studying the inverse size–productivity relationship. We then show how even in the best-case scenario, empirical tests aimed at testing for the presence of an inverse relationship are misspecified for both Cobb-Douglas production functions (i.e., log-log models) and linear production functions (i.e., linear models). For simplicity, we assume there are no farm, plot, or household characteristics that both (i) affect production, and (ii) are correlated with size (e.g., soil quality, size of the plot edge).

2.1 Log-Log Models

Many researchers estimate log-log models, running a regression with the log of yield as the dependent variable to estimate the coefficient of the log of size. At a first glance, this means estimating the partial derivative of the log of yield with respect to the log of size, which is essentially the output elasticity of land input minus one, such that

$$\frac{\partial ln(\frac{q}{a})}{\partial ln(a)} = \frac{\partial\left(ln(q) - ln(a)\right)}{\partial ln(a)} = \frac{\partial ln(q)}{\partial ln(a)} - 1, \tag{1}$$

and therefore a negative estimate only means that the output elasticity of land is less than one. Upon closer inspection, the target of estimation also depends on whether other inputs are included in the regression and how they are included. To see this more clearly, we follow the majority of the literature, which assumes (often implicitly) a Cobb-Douglas functional form, such that

$$q = Aa^{\delta}b^{\lambda}e^{\omega}, \tag{2}$$

where q is the total output, a is the size, b is a vector of all inputs, δ and λ are elasticities, ω is an idiosyncratic production shock, and A denotes total factor productivity (TFP). Equation (2) can also be written as

$$ln\left(\frac{q}{a}\right) = ln(A) + (\delta - 1)ln(a) + \lambda ln(b) + \omega, \tag{3}$$

or

$$ln\left(\frac{q}{a}\right) = ln(A) + (\delta + \lambda - 1)ln(a) + \lambda ln\left(\frac{b}{a}\right) + \omega. \tag{4}$$

Based on this production function, we examine three types of commonly adopted modeling options.

Option 1: If researchers have the structural approach in mind and control for other inputs $ln(b)$ without any transformation in the regression

$$ln\left(\frac{q}{a}\right) = \alpha + \beta ln(a) + \tau ln(b) + \epsilon, \tag{5}$$

then β is equivalent to what we just described: it is the difference between output elasticity of land and one, i.e., $\delta - 1$, according to Equation (3). The true value of the size coefficient is zero when land has an output elasticity of one. Analytically, this approach is equivalent to estimating a log-log production function and testing whether the coefficient of land is smaller than one (e.g. Carletto, Gourlay and Winters (2015)). As we discuss in Section 5, this option has become less common over the years, perhaps because researchers no longer interpret the size–productivity relationship as the output elasticity of land, either implicitly or explicitly.

Option 2: If researchers control for other inputs per unit of land $ln\left(\frac{b}{a}\right)$ and estimates the following empirical model,

$$ln\left(\frac{q}{a}\right) = \alpha + \beta ln(a) + \tau ln\left(\frac{b}{a}\right) + \epsilon, \tag{6}$$

then $\beta = \delta + \lambda - 1$, i.e., returns to scale minus one, according to Equation (4). In this case, the true value of the size coefficient is zero when production is constant returns to scale.

In both options 1 and 2, linking the size coefficient to a production parameter requires two conditions that are practically impossible to satisfy:

1. All other inputs are observed, and
2. Total factor productivity, which is omitted because it is assumed to be a constant, is not correlated with size a.

Option 3: When researchers believe that the size-productivity relationship is not a production function parameter, but rather am unconditional or conditional relationship between size and productivity, they rely heavily on the reduced-form approach—one that omits all other inputs, to estimate the following naïve regression model

$$ln\left(\frac{q}{a}\right) = \alpha^* + \beta^* ln(a) + \epsilon^*. \tag{7}$$

Proposition 1. *The coefficient β^* on size is such that*

$$\beta^* = (\delta + \lambda - 1) + \frac{Cov\left(ln(A), ln(a)\right)}{Var\left(ln(a)\right)} + \lambda \frac{Cov\left(ln\left(\frac{b}{a}\right), ln(a)\right)}{Var\left(ln(a)\right)}. \tag{8}$$

The coefficient on size β^* is thus a combination of

1. *Returns to scale minus one (i.e., $\delta + \lambda - 1$),*
2. *How TFP changes with farm size (i.e., $\frac{Cov(ln(A), ln(a))}{Var(ln(a))}$), and*

3. *How inputs per acre changes with size* $\frac{Cov\left(ln\left(\frac{b}{a}\right),ln(a)\right)}{Var(ln(a))}$.

$\beta^* = 0$ when all the three components are zero, which is impossible even if (i) the production technology exhibits CRS, (ii) TFP is the same across all farm sizes. The last term is the coefficient on $ln(a)$ when regressing $ln\left(\frac{b}{a}\right) = ln(b) - ln(a)$ on $ln(a)$, which can never be zero by construction and is equal to -1 even when a and b are independent.

Proof β^* represents the correlation between $ln\left(\frac{q}{a}\right)$ and $ln(a)$ as

$$\beta^* = \frac{Cov\left(ln\left(\frac{q}{a}\right), ln(a)\right)}{Var\left(ln(a)\right)}. \tag{9}$$

Substituting $ln\left(\frac{q}{a}\right)$ with equation (4) and applying the distribution rule we can get

$$\begin{aligned}\beta^* &= \frac{Cov\left([ln(A) + (\delta+\lambda-1)ln(a) + \lambda ln\left(\frac{b}{a}\right) + \omega], ln(a)\right)}{Var\left(ln(a)\right)} \\ &= (\delta+\lambda-1) + \frac{Cov\left(ln(A), ln(a)\right)}{Var\left(ln(a)\right)} + \lambda\frac{Cov\left(ln\left(\frac{b}{a}\right), ln(a)\right)}{Var\left(ln(a)\right)}. \blacksquare\end{aligned} \tag{10}$$

The compound nature of the coefficient on size in this case means that the interpretation of a negative estimate is ambiguous.[3] Researchers have explored the three components of β^* separately by (i) examining returns to scale, (ii) showing that input intensity is lower among larger farms, i.e., $\frac{Cov\left(ln\left(\frac{b}{a}\right),ln(a)\right)}{Var(ln(a))} < 0$, or (iii) arguing that researchers should focus on how TFP varies across farm sizes; see for example Helfand and Taylor (2021) and Rada and Fuglie (2019). But unless one can assess all three components simultaneously, one cannot fully explain a negative estimate of the effect of size on productivity.

Often, a researcher will start with the reduced-form approach (Option 3), find a negative coefficient, and then seek to "explain" a negative coefficient on size by adding an

[3]A few studies have demonstrated a similar decomposition. Helfand and Taylor (2021) derive β^* as a combination of returns to scale and a relationship between total factor productivity (TFP) and farm size, missing the last piece. Aragón, Restuccia and Rud (2022) point out how this compound coefficient makes it challenging to identify the relationship between TFP and farm size. Neither address how this relates to producer theory.

input (e.g., labor) or a control variables (e.g., soil quality) to the RHS of her regression, hoping to make the inverse relationship go away (thereby confirming the assumption of constant returns to scale). But unless that researcher has the ideal data set—that is, a data set that allows controlling for *all* inputs—and estimates the structural model in Equation (6), she will not able to confirm that returns to scale are equal to one and attribute the initial negative coefficient on size estimate to the difference in the input intensity or TFP across farm sizes. Even if she is able to find a zero coefficient estimate with imperfect data, it will most likely be a biased estimate of some uninterpretable combination of production parameters.

2.2 Linear Models

We now show that a more general linear production function setup is even more problematic than the Cobb-Douglas setup as the coefficient on size is not identifiable and lacks a sensible theoretical interpretation. The partial derivative of yield with respect of size is, by the quotient rule,

$$\frac{\partial \frac{q}{a}}{\partial a} = \frac{\frac{\partial q}{\partial a}a - q}{a^2} = \frac{\frac{\partial q}{\partial a} \cdot \frac{a}{q} \cdot q - q}{a^2} = \frac{q(\varepsilon_a - 1)}{a^2}, \tag{11}$$

where ε_a is the output elasticity of land. Again, a negative value means the output elasticity of land is less than one, but the partial derivative is not identifiable as its value changes with q and a.

Consider the linear production function

$$q = \gamma + \delta a + \lambda b + \omega, \tag{12}$$

where all variables are defined as before and ω is an idiosyncratic shocks so that $E(\omega) = 0$ and ω is independent of a and b.

Proposition 2. *With the reduced-form approach, researchers regress yield on size without controlling for other inputs, so that what they estimate is*

$$\frac{q}{a} = \dot{\alpha} + \dot{\beta} a + \dot{\epsilon}. \tag{13}$$

In Equation 13, the coefficient of size is

$$\dot{\beta} = \frac{\gamma Cov\left(\frac{1}{a}, a\right) + \lambda Cov\left(a, \frac{b}{a}\right)}{Var(a)}. \tag{14}$$

Proof We start with

$$\dot{\beta} = \frac{Cov\left(\frac{q}{a}, a\right)}{Var(a)}. \tag{15}$$

Substituting q with Equation 12, applying the distribution rule, and knowing that $Cov\left(a, \frac{\omega}{a}\right) = 0$ since ω is independent of a, We get

$$\begin{aligned} \dot{\beta} &= \frac{Cov\left(\frac{\gamma}{a} + \delta + \lambda\frac{b}{a} + \frac{\omega}{a}, a\right)}{Var(a)} \\ &= \frac{\gamma Cov\left(\frac{1}{a}, a\right) + \lambda Cov\left(a, \frac{b}{a}\right) + Cov\left(a, \frac{\omega}{a}\right)}{Var(a)} \\ &= \frac{\gamma Cov\left(\frac{1}{a}, a\right) + \lambda Cov\left(a, \frac{b}{a}\right)}{Var(a)}. \blacksquare \end{aligned} \tag{16}$$

In the numerator of $\dot{\beta}$, $Cov\left(\frac{1}{a}, a\right)$ is negative. Theoretically, it is intuitive that γ should be zero since it means output is zero when no inputs are used. When this is the case, an estimate of $\dot{\beta}$ can be interpreted based on the second term that measures how input intensity changes with farm size. If input intensity decreases with farm size, i.e., if $Cov\left(a, \frac{b}{a}\right) < 0$, then $\dot{\beta}$ is negative.[4] *Empirically, however, there is no guarantee that the estimated γ is zero*, as we will discuss later, and therefore a non-zero estimate of $\dot{\beta}$ has no clear interpretation.

[4] Technically, $Cov\left(a, \frac{b}{a}\right) = 0$ only when b is stochastically proportionate of a (Firebaugh and Gibbs,

The decomposition of β mirrors that in the Cobb-Douglas case. It is a combination of a production function parameter and an indicator of how other inputs per acre change with size. Above all, β has nothing to do with δ, the contribution of land to production.

Proposition 3. *Suppose a researcher has access to data that are free of any measurement error and that she is able to control for all inputs precisely. She estimates the following regression equation:*

$$\frac{q}{a} = \ddot{\alpha} + \ddot{\beta} a + \ddot{\tau} \frac{b}{a} + \ddot{\epsilon}. \tag{18}$$

The coefficient on land is

$$\ddot{\beta} = \frac{\gamma \left[Cov\left(\frac{1}{a}, a\right) - \frac{Cov\left(a, \frac{b}{a}\right) Cov\left(\frac{1}{a}, \frac{b}{a}\right)}{Var\left(\frac{b}{a}\right)} \right]}{Var(\tilde{a})}. \tag{19}$$

Proof: See Appendix A.

In this case, the researcher can recover a true zero coefficient on size if the intercept of the production function in Equation 12 is zero (i.e., if $\gamma = 0$), even though this finding is not in and of itself informative about the productivity of land or input structure.

In summary, the interpretation of the size coefficient in a linear model is based on a simple diagnostic. Before even estimating a regression on the relationship between size and productivity, one should test whether $\gamma = 0$ with or without input controls, i.e., in both the structural and reduced-form cases.

If $\gamma = 0$ is not supported empirically—that is, if the researcher cannot find a true zero, and not merely a coefficient that fails to live up to statistical significance due to noise—there is no empirical interpretation of the size coefficient in either the structural

1985). That is

$$b = \pi a + \varepsilon, \tag{17}$$

where ε has a mean of zero and is independent of a. In the real world, however, there are always factors that determine land and other inputs jointly, some of them are unobservable. So ε is not independent of a. Even if this requirement is satisfied, there cannot be an intercept term in the relationship defined by Equation 17. This means that all other production factors increase proportionately with farm size, which can rarely be confirmed with real-world data.

or reduced-form case. If γ is empirically supported by a true zero, the coefficient on size from structural approach is bound to be zero, but this has nothing to do with the productivity of land itself.

As we discuss in Section 4, the requirement that $\gamma = 0$ is a stringent one. While one need not have ever visited a farm to know that in theory, output should equal zero in Equation 12 when "producing" without land or any other input (i.e., with both $a = 0$ and $b = 0$), there is often a wide chasm between theory and empirics, and just because $\gamma = 0$, nothing guarantees that $\hat{\gamma} = 0$.

3 Simulations

We now turn to using simulated data to demonstrate how the size coefficient of an inverse relationship study changes with model specification and what conditions must be satisfied for it to be zero, as established by the derivations in Section 2.

We begin by using the Monte Carlo method to draw repeated random samples from the two types of data-generating processes commonly assumed in the literature: (i) a Cobb-Douglas production function, and (ii) a linear production function. We show that in both cases, when some inputs are observed but others are not, regressing yield on size leads to a negative coefficient estimate by the construction of the left-hand side variable as a ratio of output over size. This spurious inverse relationship emerges even if production exhibits constant returns to scale, the omitted inputs are independent of size and there are no other unobservable confounders.

We start with a simple Cobb-Douglas production function, such that

$$q = a^{0.3} b^{0.3} c^{0.4} e^{\omega}, \tag{20}$$

where, once again, q denotes output, a denotes size, b denotes inputs that are observed by the researcher, c denotes inputs that are not observed by the researcher, and ω is an idiosyncratic production shock such that $\omega \sim N(0,1)$. As is almost always the case in the literature on the inverse size–productivity relationship, we assume that the production technology exhibits constant returns to scale (i.e., $0.3 + 0.3 + 0.4 = 1$), that farms or plots of all sizes are technically efficient, and there are no other unobservable production determinants. We further assume that a, b, and c are independent of one another and are all distributed $N(100, 50)$.

If the researcher regresses $ln\left(\frac{q}{a}\right)$ on a constant as well as $ln(a)$, she is bound to get a negative coefficient estimate on the latter. To show this, we simulate a population of size 10,000 (dropping observations with negative inputs or output), and we draw samples of 500 observations for 1,000 times. Plotting out the distribution of the 1,000 estimated βs in red in Figure (I), we see that that distribution is clearly centered around a negative value and away from zero. The distribution in yellow shows that if the researchers control for the observed inputs per unit of land $ln\left(\frac{b}{a}\right)$, the estimated coefficient moves toward zero, but it is still negative because $ln(\frac{c}{a})$ is missing from the regression. The distribution in green shows that it is only when *all* other inputs $ln\left(\frac{b}{a}\right)$ and $ln(\frac{c}{a})$ are controlled per unit of land that the researcher will be able to find a true zero, i.e., returns to scale minus one in equation (4).

Next, assume the researcher estimates a linear model with the assumption that the production function is linear, such that

$$q = 0 + 0.3a + 0.3b + 0.4c + \omega, \tag{21}$$

with all variables and simulations as before. A univariate regression of $\frac{q}{a}$ on a is again bound to be negative, as shown by the red distribution in Figure II. Indeed, applying a widely used nonparametric regression analysis in this literature (Foster and Rosenzweig, 2022; Debrah and Adanu, 2022; Helfand and Taylor, 2021) on the simulated population data, we can clearly see a negative relationship between yield and size in Figure III. Again,

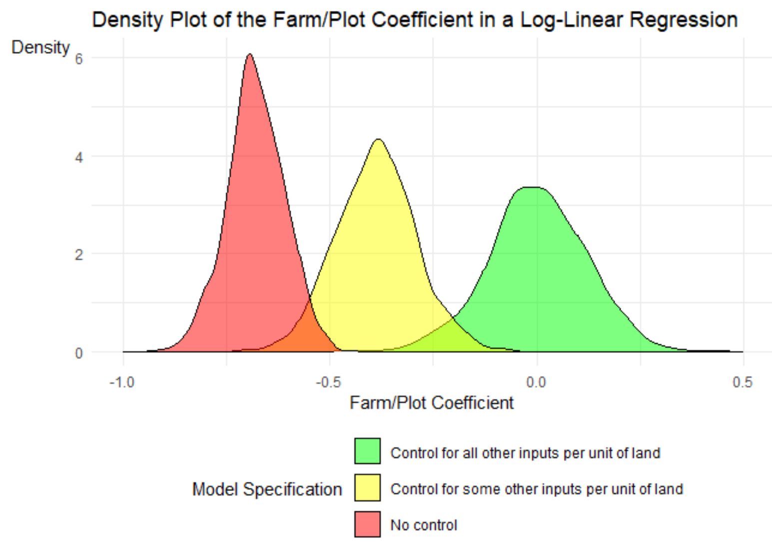

FIGURE I: Simulation Results for a Cobb-Douglas Production Function.

controlling for observable inputs per unit of land helps move the coefficient toward zero, as shown in the yellow distribution in Figure II, but a true zero can only be found only when the researcher controls for *all* inputs—both observable and unobservable—per unit of land, as shown in the green distribution in Figure II. In Section 2, we have shown that this zero stems from the fact that the intercept of the production function is zero, i.e., nothing can be produced in the absence of any input.

In both the Cobb-Douglas and linear cases, the spurious inverse relationship stems from how the left-hand side (LHS) variable is constructed, viz. yield as a ratio of output divided by size. When running a univariate variable regression of yield on size, we intentionally omit all other inputs per unit of land, which is negatively correlated with size even though the total inputs are simulated to be independent of size. If we were to estimate a production function with output instead of yield on the LHS, omitting other inputs in this simulation would not introduce any bias. But when the LHS variable is yield, other inputs should also be divided by size to correctly specify the underlying model, and thus the negative correlation between size and input intensity drives the true coefficient in Equations (10) and (16) negative.

In practice, other inputs per unit of land, however, are not independent of size. If

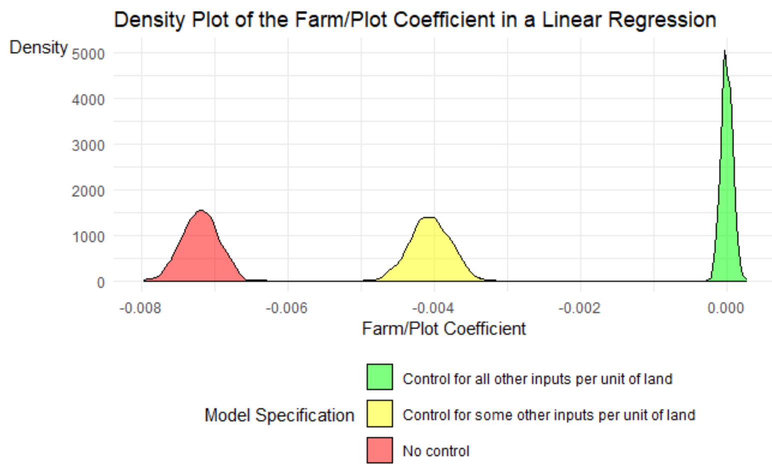

FIGURE II: Simulation Results for a Linear Production Function.

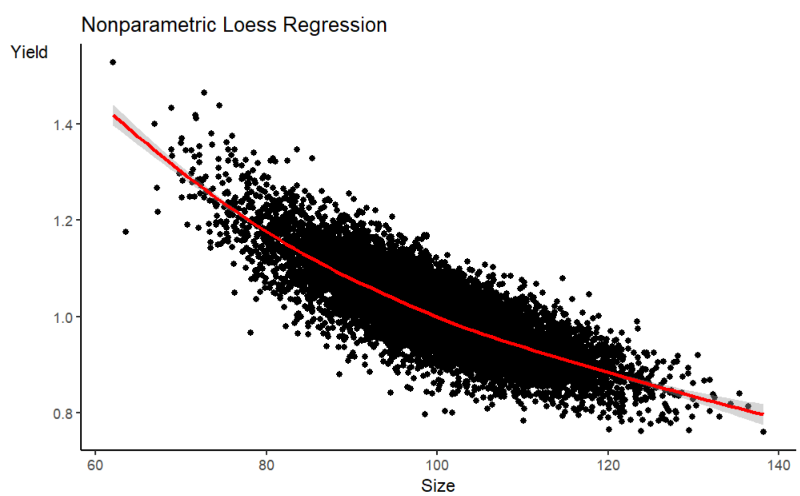

FIGURE III: Nonparametric Regression Results for a Linear Production Function.

they are negatively correlated with size, as suggested by the agricultural intensification hypothesis (Boserup, 1965), then omitting them will introduce negative bias (i.e., away from zero) in the coefficient on size, and vice versa. Over time, with improvements in the quality of data collection, researchers have increasingly added more input controls to their empirical models. As we show in the meta-analysis in Section 5, adding labor input controls mitigates some of the bias and moves the estimated coefficient closer to zero. While this may partially explain the disappearance of the inverse relationship in the recent literature (Garzón Delvaux, Riesgo and Gomez y Paloma, 2020), unless researchers control for *all* relevant inputs, a search for a zero coefficient on size is misguided, as we have shown analytically in Section 2.

4 Empirical Challenges

We now focus on the second problem caused by including size on both sides of the estimating equation: that of obtaining an unbiased estimate of the size coefficient regardless of its true value. The size–productivity relationship literature has explored empirical challenges including omitted variables and nonclassical measurement error as potential explanations for an inverse relationship that is believed to be a statistical artifact. Our goal here is to highlight that the ratio variable brings an additional layer of threat to identification beyond these challenges: even classical measurement error in size and omitted production factors that are not correlated with size will introduce bias.

4.1 Measurement Error in Size

The first type of threat was identified in the early 1980s by the labor economist Borjas (1980) who dubbed it "division bias." But it only received attention in this literature more than three decades later with Abay et al. (2019) as an additional note in their investigation of nonclassical measurement error: even when size suffers from classical measurement error, the coefficient on size is both attenuated and biased downward because that measurement error appears on both sides of the equation of interest. We further refine their conclusion by highlighting that the bias caused by classical measurement error in size applies to the reduced-form approach, but not to the fully specified structural approach in log-log models. We also add that the downward and attenuation biases also apply to the reduced-form approach with linear models.

Starting from the reduced-form log-log model, denote the observed area measured with error as $a^{\dagger} = ae$, where e is an independent measurement error scaling factor with $E(e) = 1$. Then, a bivariate regression of $ln\left(\frac{q}{a^{\dagger}}\right)$ on $ln(a^{\dagger})$ yields

$$\beta^{*\dagger} = \frac{Cov\left(ln\left(\frac{q}{a^{\dagger}}\right), ln(a^{\dagger})\right)}{Var\left(ln(a^{\dagger})\right)} \tag{22}$$

$$= \frac{Cov\left(ln\left(\frac{q}{a}\right) - ln(e), ln(a) + ln(e)\right)}{Var\left(ln(a+e)\right)} \tag{23}$$

$$= \frac{Cov\left(ln\left(\frac{q}{a}\right), ln(a)\right) - Var\left(ln(e)\right)}{Var\left(ln(a)\right) + Var\left(ln(e)\right)}. \tag{24}$$

The $Var\left(ln(e)\right)$ in the denominator and numerator of $\beta^{*\dagger}$ represent the attenuation and downward bias relative to β^{*} in equation (9), respectively.

Interestingly, classical measurement error in size does not threaten the identification of the size coefficient in the structural approach (i.e., a log-log model) if the intensity of all other inputs are controlled for. To see this, rewrite the Cobb-Douglas production function in Equation (6) by substituting $\frac{a^{\dagger}}{e}$ for a, such that

$$ln\left(\frac{q}{a^{\dagger}}\right) = \alpha + \beta ln(a^{\dagger}) + \tau ln\left(\frac{b}{a^{\dagger}}\right) - (1 - \beta + \tau) ln(e) + \epsilon, \tag{25}$$

Running a regression of $ln\left(\frac{q}{a^{\dagger}}\right)$ on $ln(a^{\dagger})$ and $ln\left(\frac{b}{a^{\dagger}}\right)$ while omitting $ln(e)$ does not bias the estimate of β because e is independent. Nevertheless, the measurement error enters the error term and inflates the estimation standard error. As a result, researchers are less likely to reject the null hypothesis.

To see the implication of classical measurement error in size for linear models, denote the observed area measured with error as $a^{\ddagger} = a + e'$, where e' is an independent measurement error with $E(e') = 0$. A bi-variate regression of $\frac{q}{a^{\ddagger}}$ on $a^{\ddagger}$ yields

$$\dot{\beta}^{\ddagger} = \frac{Cov\left(\frac{q}{a^{\ddagger}}, a^{\ddagger}\right)}{Var(a^{\ddagger})} = \frac{E(q) - E(\frac{q}{a+e'})E(a)}{Var(a) + Var(e')} \tag{26}$$

Compared to $\dot{\beta} = \frac{Cov\left(\frac{q}{a}, a\right)}{Var(a)} = \frac{E(q)-E(\frac{q}{a})E(a)}{Var(a)}$, $\dot{\beta}^{\ddagger}$ is attenuated and downward biased because it has a larger denominator and a smaller numerator.[5]

4.2 Omitted Variables Uncorrelated with Size

We show analytically that in the linear specification, any omitted variable—even those that are uncorrelated with the right-hand side (RHS) variables such as land or other inputs—will introduce bias in the estimated relationship between size and productivity. While the reader might think this is the same point we made with our derivations and simulations about omitted input intensity, it is not. The omitted input intensity has size in the denominator and therefore is almost surely correlated with size. The issue here is not the choice of model specification and the parameter of interest, but the observability of all production factors, including inputs, natural elements, and farm characteristics.

Specifically, if there is any unobservable production factor and this variable has a nonzero mean, the estimate of γ in a production function regression will be driven away from a true zero. Worse, the bias in the intercept persists even if the omitted input is independent of observed inputs and with both δ and λ consistently estimated.

To see this, let u be an omitted factor in a production function. Omitting u in a reduced-form regression without control variables as in Equation 13 means that the coefficient on

[5] $E(\frac{q}{a+e'}) > E(\frac{q}{a})$ since $a + e'$ is a mean-preserving spread of a and the inverse function is convex for positive values.

size is such that

$$\dot{\beta}_{biased} = \frac{\gamma Cov\left(\frac{1}{a}, a\right) + \lambda Cov\left(a, \frac{b}{a}\right) + Cov\left(\frac{u}{a}, a\right)}{Var(a)}. \tag{27}$$

Thus, even if the direct bias $\frac{Cov\left(\frac{u}{a},a\right)}{Var(a)}$ is zero because u and a are independent, an indirect bias arises from γ in the first term: An estimate of β is biased because the nonzero mean of the omitted u enters in the intercept of the production function.

Omitting u in the fully specified structural form model as in Equation 18 means that the coefficient on size is such that

$$\ddot{\beta}_{biased} = \frac{\gamma \left[Cov\left(\frac{1}{a}, a\right) - \frac{Cov\left(a, \frac{b}{a}\right)Cov\left(\frac{1}{a}, \frac{b}{a}\right)}{Var\left(\frac{b}{a}\right)}\right] + Cov\left(\frac{u}{a}, a\right) + \frac{Cov\left(\frac{b}{a}, \frac{u}{a}\right)Cov\left(\frac{b}{a}, a\right)}{Var\left(\frac{b}{a}\right)}}{Var(\tilde{a})}. \tag{28}$$

Again, even if the direct bias $\frac{Cov\left(\frac{u}{a},a\right)+\frac{Cov\left(\frac{b}{a},\frac{u}{a}\right)Cov\left(\frac{b}{a},a\right)}{Var\left(\frac{b}{a}\right)}}{Var(\tilde{a})}$ is zero, there is still an indirect bias stemming from γ which pushes the estimate of β away from zero.

Log-log models, however, are not subject to this special kind of threat since the transformation from the production function to the yield regression only involves subtracting the log of size from the log of output and the log of other inputs, not the transformation of the unobserved production determinant. But they are still subject to the endogeneity problem posed by unobservable production determinants is the same as in a production function, as summarized by Ackerberg, Caves and Frazer (2015).

5 Meta-Analysis

We now survey the literature to see what patterns emerge when it comes to model (mis-) specification. We focus on the literature related to the estimation of the size–productivity relationship in English since 1960 in the EconLit database.[6]

As of February 4, 2025, we found 143 records. Among those, we further reduce the list to 105 items published in scholarly journals,[7] leaving aside studies that are in unpublished working papers or dissertations. We then added three additional items identified from citations not captured by the search syntax. After examining each of the 108 retained papers, we further excluded papers that were not about agriculture, and the ones without direct estimation of the coefficient on size, most of which are nonparametric, descriptive, or theoretical. Eventually, we ended up with 589 regression estimates from 51 papers.[8]

5.1 Modeling Practices Over Time

Table I reports the number of estimates by publication year and model specification. From the 1960s to the early 2010s, there was a steady stream of papers published in the size–productivity literature. Around 2013, the number of studies published annually on the topic exploded and, in line with the Credibility Revolution, it became customary to report multiple regression tables in each study.

In the literature, using a ratio variable (e.g., yield) as the dependent variable became increasingly dominant over using a measure of total output. Starting in about 2016, a substantial number of studies started using TFP or technical efficiency as the dependent variable. But since many of those studies do not report coefficient estimates, they are underrepresented in our sample.

Among the studies with a ratio variable as the dependent variable, the majority of researchers choose a log-log specification. Before 2001, almost all cases were based on the reduced-form approach, with size as the only input variable on the right-hand side. After

[6] Specifically, the search syntax is `"title((size-productivity) OR (inverse productivity) OR (productivity* size*) OR (inverse relationship) OR (small large product*)) AND la.exact("English") AND pd(>19600101) AND (farm* OR plot* OR agriculture OR agricultural)"`.

[7] We did not restrict to peer-reviewed items because the "peer-reviewed" label is not always included, so restricting to it leads to missed papers that we know were actually peer-reviewed.

[8] Regression results in those papers' appendices are not included here.

2001, the reduced-form approach represented roughly half of the cases, often with farm or household characteristics and fixed effects as additional control variables. The other half of the cases are based on the structural approach. Among those, labor is included in almost all cases. The inclusion of other, nonlabor inputs is more sporadic—although the majority of regressions after 2020 included fertilizer, none included capital inputs.

TABLE I: Number of cases by model specification and publication year

Publication Year	1960 - 1980	1981 - 2000	2001 - 2020	Since 2021
Number of cases	73	53	286	177
Dependent variable: Ratio variables[1]	21	38	236	177
log-log	12	10	177	143
linear-linear	9	2	33	34
log-linear	0	4	0	0
linear-log	0	0	22	26
Reduced-form approach	21	38	111	91
Structural approach	0	0	125	86
Control for labor	0	0	117	78
Control for fertilizer	0	0	65	60
Control for capital	0	0	40	0
Control for soil quality	0	0	117	47
Control for household characteristics	0	1	114	98
Control for fixed effects[2]	0	5	83	47
Dependent variable: Total output[3]	52	13	25	0
Dependent variable: TFP/TE	0	2	25	0

Notes: [1] including yield, gross value per unit of land, and net value per unit of land. [2] including farm, plot, time, or farm-time fixed effects. [3] including output, an output index, gross value of output, and net value of output. Only 3 cases are linear-linear, others are log-log.

From the simulations presented in Figure I and II, we know that the inclusion of input intensity variables that decrease with size has a positive effect on the estimated coefficient on size (i.e., if it is negative, the inclusion of those input intensities moves the coefficient on size toward zero).

Here we explore how that coefficient estimate changes with the inclusion of covariates in published studies, focusing on the 342 cases from log-log models with a ratio variables as the dependent variable. We regress the estimated coefficient on binary variables capturing what types of covariates are included in the regression. Since the choice of model specification and the measurement of covariates are typically consistent within studies, we correct our standard errors by clustering at the study level.

Across the columns of Table II are regressions which pool all studies (column 1) or incorporate study fixed effects (column 2), country fixed effects (column 3), or both study and country fixed effects (column 4). We notice a consistent pattern whereby including a measure of labor inputs pushes the estimated coefficient toward zero (i.e., making those coefficient estimates less negative), indicating that labor intensity decreases with farm size. In contrast, the inclusion of various measures of capital inputs and household characteristics push the estimated coefficient away from zero (i.e., making those coefficient estimates more negative).

TABLE II: The impact of control variables on the farm/plot size coefficient

	Dependent variable:			
	Estimate of the farm/plot size coefficient			
	Pooled	Study FE	Country FE	Study and County FE
	(1)	(2)	(3)	(4)
Control for labor	0.169	0.252	0.273**	0.261
	(0.106)	(0.163)	(0.124)	(0.167)
Control for fertilizer	0.014	−0.014	−0.006	−0.016
	(0.062)	(0.092)	(0.097)	(0.094)
Control for capital	−0.158	−0.188	−0.445***	−0.197
	(0.101)	(0.179)	(0.150)	(0.182)
Control for soil quality	0.065	0.043	0.016	0.032
	(0.061)	(0.051)	(0.078)	(0.055)
Control for household characteristics	−0.144**	−0.178*	−0.154	−0.185*
	(0.066)	(0.102)	(0.102)	(0.108)
Control for fixed effects	0.018	−0.034	0.054	−0.032
	(0.060)	(0.042)	(0.052)	(0.041)
Constant	−0.241***	−0.252***	0.088	−0.022
	(0.082)	(0.086)	(0.111)	(0.147)
Observations	342	342	342	342
R^2	0.059	0.481	0.234	0.485
Adjusted R^2	0.042	0.431	0.192	0.429
Residual Std. Error	0.329 (df = 335)	0.254 (df = 311)	0.303 (df = 323)	0.254 (df = 308)
F Statistic	3.495*** (df = 6; 335)	9.625*** (df = 30; 311)	5.495*** (df = 18; 323)	8.774*** (df = 33; 308)
Note:				*p<0.1; **p<0.05; ***p<0.01

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors robust to clustering at the study level are reported in the parenthesis.

5.2 Testing for Publication Bias

From our derivations we know that the coefficient on size can be anything depending on model specification, returns to scale, and how input intensity and TFP change with size. Yet pervasive model mis-specification may have established the inverse relationship as a stylized fact, and this may have reinforced the expectation in the minds of researchers as well as journal editors and reviewers (or worse, in the minds of policy makers) that the coefficient should be negative, making it harder for studies finding statistically insignificant or statistically significant positive results to be published.

We explore this type of publication bias in the literature using the funnel asymmetry test (FAT), as in Egger et al. (1997). The test plots the estimated coefficients against their standard errors. If there is no publication bias, estimates from studies with a larger sample size (and thus smaller standard errors) will be closely centered around the overall mean effect, and estimates from smaller studies (and thus larger standard errors) will be more widely scattered, forming a funnel shape. If some estimates from smaller studies on one side of the funnel are missing, creating an asymmetry, then it is an indication of publication bias. This can be further confirmed with a simple regression of the estimated effects on the standard errors.[9] Funnel asymmetry means that the coefficient on the standard errors on that regression is significantly different from zero.

To focus on estimates that are comparable across studies, we limit our test to the 265 cases from studies with a log-log specification and with the left-hand-side variable as a ratio variable (i.e., yield, gross value per unit of land, and net value per unit of land).[10]

Figure IV shows clear selection in favor of statistically significant negative estimates. There are few publications of positive estimates with larger standard errors. Negative estimates are more likely to be published if they are statistically significant at the 1% or 5% levels, as shown by the downward pattern in the red and green points. There are,

[9] Ton et al. (2018) run such a test for publication bias in the contract farming literature.

[10] This is smaller than number of corresponding cases reported in Table II because, for some studies, standard errors are not reported.

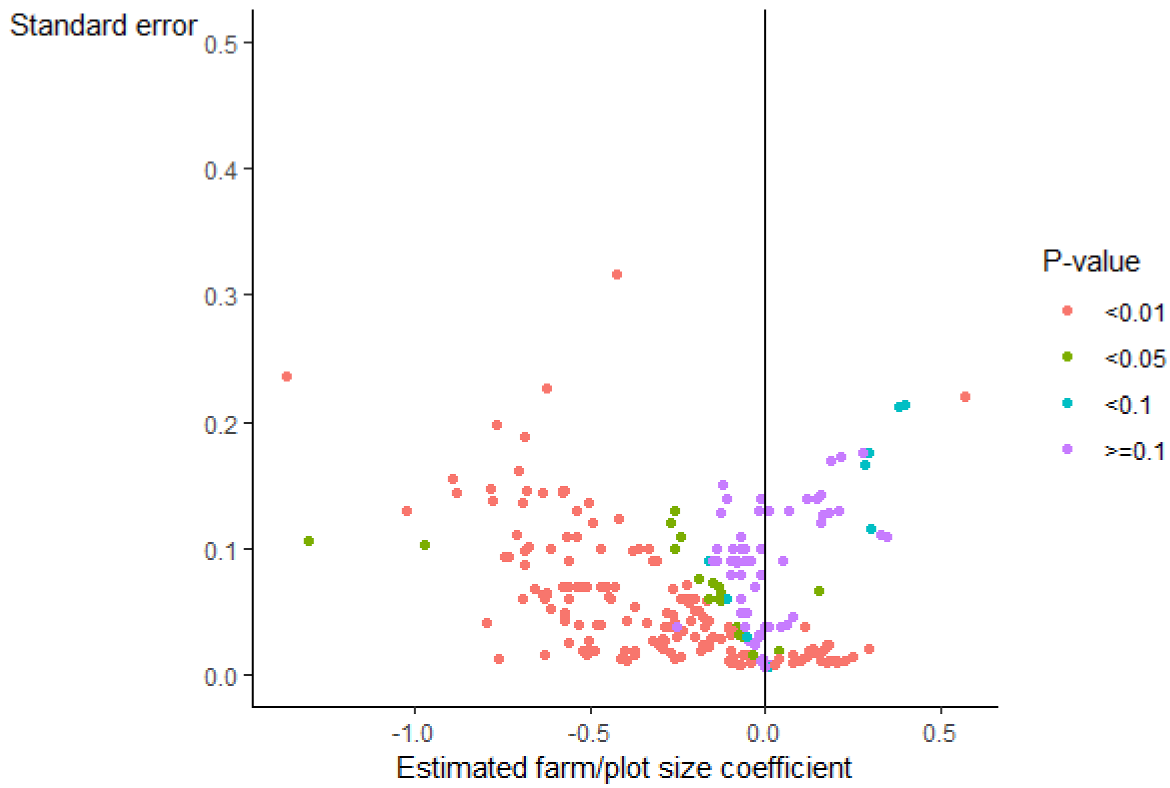

FIGURE IV: Funnel plot

however, some published estimates on both sides that are not statistically significant (in purple, with p-values greater than 0.1). Looking more closely, and in line with the recent push for transparency and replicability in economics, those are mainly from six studies published after 2019 in top field journals.

Next, we report linear regression results based on the same collection of estimates, essentially fitting a line across the points in Figure IV. As pointed out by Alinaghi and Reed (2018), the error term of the FAT is heteroskedastic since different studies have different precision levels. We correct for the clustering of precision at the study level in the results reported in Table III. Column 1 of Table III shows that the slope of the fitted line is -0.827 and is statistically significant at the 10% level. If we exclude the statistically insignificant estimates of the size coefficient reported in more recent studies, the slope of the regression line is even flatter (-1.228) and significant at the 5% level. The intercept, commonly referred to as the "effect beyond [publication] bias," is negative and statistically significant in both regressions, indicating that even *after* correcting for publication bias, the estimates are predominantly negative, presumably due to the bias from ratio variable specifications.

TABLE III: Funnel asymmetry test

	Dependent variable: Estimate of the farm/plot size coefficient	
	All estimates	Without non-significant estimates
	(1)	(2)
Standard error	-0.827^{*}	-1.228^{**}
	(0.480)	(0.582)
Constant	-0.164^{***}	-0.210^{***}
	(0.052)	(0.072)
Observations	265	203
R^2	0.038	0.094
Adjusted R^2	0.034	0.089
Residual Std. Error	0.307 (df = 263)	0.307 (df = 201)
F Statistic	10.419^{***} (df = 1; 263)	20.744^{***} (df = 1; 201)

Notes: *p<0.1; **p<0.05; ***p<0.01. Standard errors robust to clustering at the study level are reported in the parenthesis.

6 Summary and Concluding Remarks

We have looked at the literature on the inverse relationship, showing that it has seemingly talked itself into a number of practices which may ultimately be harmful and damaging to its core objective, which is to test whether there is indeed an inverse relationship between size and productivity.

First, we have shown that what exactly is measured by the coefficient on size in the usual regressions run in this literature depends on model specification. More importantly, we have also shown that the estimand—the target of estimation itself—is rarely zero.

Second, we have shown that even if the model is correctly specified with ideal input and output data, and the true value of the size coefficient is zero, one will only obtain an estimate close to zero by chance. In many cases, any measurement error in size or unobservable factor of production will drive the coefficient estimate away zero, even if those are independent of size and other inputs.

Third, we conduct a meta-analysis of the literature, showing how modeling practices have changed over time in the inverse size–productivity literature, and showing evidence of publication bias in that literature.

The implications for policy, while not unique to this paper, are obvious: It would be a mistake to conclude from the literature on the inverse size–productivity relationship that smaller farms are somehow in a better position to feed a growing world population than larger farms (see, for instance, a report by an international non-profit organization (GRAIN, 2014). While this was obvious from earlier contributions (Desiere and Jolliffe, 2018; Gourlay, Kilic and Lobell, 2019; Bevis and Barrett, 2020), the findings in this paper strengthen that argument by showing that the results of many hypothesis tests of the relationship between size and productivity can be difficult to interpret. In the limit, it is not clear that there is such a relationship between size and productivity.

The implications for research are straightforward. First and foremost, researchers interested in testing whether there is an inverse relationship between farm size and productivity need to adopt a structural approach to the problem, estimating a log-log model that regresses yield on *all* of the inputs that go into making the output in the numerator of the yield variable.

This is a stringent requirement. Indeed, when does one actually have a fully specified production function? Looking at the literature on the inverse relationship, the best attempts at estimating a production function only include a handful of inputs. Barrett, Bellemare and Hou (2010), for instance, include cultivated area, labor hours (broken down by category such as adult or child) draft animal hours, and soil quality measurements (i.e., carbon, nitrogen, potassium, soil pH, as well as the soil's clay, silt, and sand percentages), but nothing about inputs such as seeds, pesticides, or fertilizer, or natural inputs such as rainfall or sunlight. Desiere and Jolliffe (2018) include cultivated area, manure, compost, organic fertilizer, irrigation, fertilizer, and labor (broken down by category and task such as planting or harvesting), but nothing about soil quality or natural inputs such as rainfall. Bevis and Barrett (2020) include cultivated area, soil quality measurements, labor intensity, inputs such as inorganic amendments and organic fertilizer, but nothing about natural inputs such as rainfall. Gourlay, Kilic and Lobell (2019) include cultivated area, organic fertilizer, inorganic fertilizer, household labor, hired labor, and rainfall, but no soil quality measurements. Like Pardey and Alston (2021) note, "[m]any of the ... natural inputs to ... agricultural production are rarely measured" (p.118).

Even with access to precisely measured labor (i.e., labor hours broken down by category and by task) and to other inputs (e.g., seeds, pesticides, fertilizer, soil quality measurements, and natural inputs such as rainfall, sunlight, or air quality, as well as wind direction and speed, which have all been shown to affect agricultural productivity in ways which may or may not be mediated by behavioral factors), it likely remains impossible to have a fully specified structural version of the equation of interest, which is derived from a Cobb-Douglas production function. This is because a theoretical production function $q = f(a, x)$ is ultimately a static representation of a dynamic process, and how much of a factor of production was used *in toto* (e.g., 46.34 adult hours of labor dedicated to harvesting) says nothing about whether that input was applied at the right time.

In other words, estimating a production function necessarily involves taking a continuous phenomenon over a crop season and reducing it to a number of summary measures, thereby using imperfect stocks to measure flows. By writing down a production function and estimating it, economists may well be fooling themselves into thinking that their estimates accurately capture reality.

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Appendix A

Proof of $\ddot{\beta}$:

When estimating a linear function as below

$$\frac{q}{a} = \ddot{\alpha} + \ddot{\beta} a + \ddot{\tau}\frac{b}{a} + \ddot{\epsilon}. \tag{29}$$

The projection coefficient of farm size $\dot{\beta}$ is

$$\ddot{\beta} = \frac{Cov\left(\frac{\tilde{q}}{a}, \tilde{a}\right)}{Var(\tilde{a})}, \tag{30}$$

where $\tilde{a}$ is the error from regressing a on $\frac{b}{a}$ and a constant, and $\frac{\tilde{q}}{a}$ is the error from regressing $\frac{q}{a}$ on $\frac{b}{a}$ and a constant (Frisch-Waugh-Lovell theorem). That is,

$$\tilde{a} = a - \frac{Cov(a, \frac{b}{a})}{Var(\frac{b}{a})} \cdot \frac{b}{a} - \left(\bar{a} - \frac{Cov(a, \frac{b}{a})}{Var(\frac{b}{a})} \cdot \frac{\bar{b}}{a}\right) \tag{31}$$

$$= (a - \bar{a}) - \frac{Cov(a, \frac{b}{a})}{Var(\frac{b}{a})}\left(\frac{b}{a} - \frac{\bar{b}}{a}\right), \tag{32}$$

$$\frac{\tilde{q}}{a} = \left(\frac{q}{a} - \frac{\bar{q}}{a}\right) - \frac{Cov(\frac{q}{a}, \frac{b}{a})}{Var(\frac{b}{a})}\left(\frac{b}{a} - \frac{\bar{b}}{a}\right), \tag{33}$$

where $\frac{\bar{b}}{a}$ is the mean of $\frac{b}{a}$, and so on. As errors, $E(\tilde{a}) = 0$ and $E\left(\frac{\tilde{q}}{a}\right) = 0$. Therefore, the numerator of $\dot{\beta}$ in Equation 30 is

$$
\begin{aligned}
Cov\left(\frac{\tilde{q}}{a}, \tilde{a}\right) &= E\left(\frac{\tilde{q}}{a}\cdot\tilde{a}\right) - 0 \\
&= E\left((a-\bar{a})\left(\frac{q}{a}-\frac{\bar{q}}{a}\right)\right) - E\left(\frac{Cov(\frac{q}{a},\frac{b}{a})}{Var(\frac{b}{a})}\left(\frac{b}{a}-\frac{\bar{b}}{a}\right)(a-\bar{a})\right) \\
&- E\left(\frac{Cov(a,\frac{b}{a})}{Var(\frac{b}{a})}\left(\frac{b}{a}-\frac{\bar{b}}{a}\right)\left(\frac{q}{a}-\frac{\bar{q}}{a}\right)\right) + E\left(\frac{Cov(a,\frac{b}{a})Cov(\frac{q}{a},\frac{b}{a})}{Var^2(\frac{b}{a})}\left(\frac{b}{a}-\frac{\bar{b}}{a}\right)\left(\frac{b}{a}-\frac{\bar{b}}{a}\right)\right) \\
&= Cov\left(a,\frac{q}{a}\right) - \frac{Cov(\frac{q}{a},\frac{b}{a})Cov\left(a,\frac{b}{a}\right)}{Var(\frac{b}{a})} - \frac{Cov(a,\frac{b}{a})Cov\left(\frac{q}{a},\frac{b}{a}\right)}{Var(\frac{b}{a})} + \frac{Cov(a,\frac{b}{a})Cov(\frac{q}{a},\frac{b}{a})}{Var(\frac{b}{a})} \\
&= Cov\left(a,\frac{q}{a}\right) - \frac{Cov(\frac{q}{a},\frac{b}{a})Cov\left(a,\frac{b}{a}\right)}{Var(\frac{b}{a})} \\
&= \gamma Cov\left(\frac{1}{a},a\right) + \lambda Cov\left(a,\frac{b}{a}\right) - \frac{Cov(\frac{q}{a},\frac{b}{a})Cov\left(a,\frac{b}{a}\right)}{Var(\frac{b}{a})}
\end{aligned}
$$

Note that $\frac{Cov\left(\frac{q}{a},\frac{b}{a}\right)}{Var\left(\frac{b}{a}\right)}$ is the coefficient of regressing $\frac{q}{a}$ on $\frac{b}{a}$ with a constant that equals to λ iif $\gamma = 0$ (Firebaugh and Gibbs, 1985). And it can be shown that

$$
\frac{Cov\left(\frac{q}{a},\frac{b}{a}\right)}{Var\left(\frac{b}{a}\right)} = \lambda + \frac{\gamma Cov(\frac{1}{a},\frac{b}{a})}{Var(\frac{b}{a})} \tag{34}
$$

Substituting it back into Equation 30 we have

$$
\ddot{\beta} = \frac{\gamma\left[Cov\left(\frac{1}{a},a\right) - \frac{Cov\left(a,\frac{b}{a}\right)Cov(\frac{1}{a},\frac{b}{a})}{Var(\frac{b}{a})}\right]}{Var(\tilde{a})} \tag{35}
$$