Measurement Error Mechanisms Matter: Agricultural intensification with farmer misperceptions and misreporting

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The mechanism(s) that generate measurement error matter to inference. Survey measurement error is typically thought to represent simple misreporting correctable through improved measurement. But errors might also or alternatively reflect respondent misperceptions that materially affect the respondent decisions under study. We show analytically that these alternate data generating processes imply different appropriate regression specifications and have distinct effects on the bias in parameter estimates. We introduce a simple empirical technique to generate unbiased estimates under more general conditions and to apportion measurement error between misreporting and misperceptions in measurement error when one has both self-reported and objectively-measured observations of the same explanatory variable. We then apply these techniques to the longstanding question of agricultural intensification: do farmers increase input application rates per unit area as the size of the plots they cultivate decreases? Using nationally representative data from four sub-Saharan African countries, we find strong evidence that measurement error in plot size reflects a mixture of farmer misreporting and misperceptions. The results matter to inference around the intensification hypothesis and call into question whether more objective, precise measures are always preferable when estimating behavioral parameters.

Keywords: Agricultural inputs, Boserup, non-classical measurement error, smallholders

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1 Introduction

Applied economists rely heavily on survey data to study decision-making. Yet survey data often include measurement error. Most of the measurement error literature assumes that survey respondents know and base decisions upon the true value of an explanatory variable that they misreport.[1] When that assumption holds, measurement error in self-reported survey data is remediable through improved data collection or (sometimes) by statistical correction.

Yet a vast behavioral economics literature finds that people routinely misperceive actual conditions – about others' behaviors, relative prices, social norms, the probability of stochastic events, etc. – and act upon those systematic biases, mistaken beliefs, or misperceptions (Tversky and Kahneman, 1973; Kahneman and Tversky, 1984; Angner and Loewenstein, 2012). If respondents accurately report their misperceptions, rather than misreporting their true beliefs, then it is less clear how to address measurement error. The appropriate regression specification will then depend on whether one wants to make inferences about (potentially misinformed) choices, about the biophysical relationship between objectively measured variables, or both. In making inferences about economic hypotheses that revolve around human choices, researchers might easily over-correct for measurement error statistically, or inappropriately substitute an objective measure for the subjective measure that a respondent believes and reports, and on which she acts.

Of course, analysts typically have no way of distinguishing misreporting from misperceptions in survey data. The mechanism(s) generating measurement error in survey data nonetheless matter to econometric inference on a wide range of issues. For example, parents may know and act in accord with the exact age of their child and knowingly misreport a simple, rounded number – e.g., 2 years, rather than, say, 20 months – with important consequences for inferences about child anthropometric status (Larsen, Headey and Masters, 2019). Or perhaps they do not know the child's true age, with potential consequences for medication dosing and growth monitoring. Similarly, measurement error in the hours laborers report working could lead to 'division bias' in wage elasticity estimates, as Borjas (1980) demonstrates. Self-reported hours might, however, reflect salaried workers' mistaken beliefs about the true hours they work, and thereby provide better information regarding behavioral responses to changes in pay.

In this paper we explore analytically the differences that arise when estimating a parameter of interest using an explanatory variable that contains measurement error generated by respondent misreporting versus misperceptions, and develop an estimable parameter to decompose measurement error between these two mechanisms. We apply these insights to a longstanding topic attracting newfound attention: agricultural intensification in Africa.

Rising population densities and low levels of agricultural productivity, especially in sub-Saharan Africa (SSA), have brought renewed attention to agricultural productivity in poor countries (Gollin, Lagokos and Waugh, 2013; Adamopoulos and Restuccia, 2014; Van Ittersum et al., 2016), to agricultural factor allocation (Restuccia and Santaeulalia-Llopis, 2017; Gollin and Udry, 2019), and to the agricultural intensification

[1]Bound et al. (2001) offers an excellent review of the measurement error literature.

hypothesis first articulated by Boserup (1965). This hypothesis maintains that poor farmers will intensify modern input use in order to compensate for shrinking land size, thus thwarting Malthusian predictions (Binswanger-Mkhize and Savastano, 2017; Holden, 2018). While rural populations decline in the rest of the world, they continue to rise in SSA, even in many land-constrained countries that have little or no surplus agricultural land to bring into cultivation (Jayne, Chamberlin and Headey, 2014; Headey and Jayne, 2014). Average farm and plot sizes are therefore declining and expected to continue to shrink for many years. Agricultural intensification is therefore critical to SSA countries' ability to sustain current welfare, as well as to support structural transformation and economic development (Gollin, Parente and Rogerson, 2002; Bustos, Caprettini and Ponticelli, 2016). And scholars recognize that measurement error may confound analysis of agricultural productivity (Carletto, Savastano and Zezza, 2013; Gollin, Lagokos and Waugh 2013; Desiere and Jolliffe, 2018; Gollin and Udry 2019).

Relatively low aggregate rates of input intensity across SSA suggest to many observers that intensification is not occurring, although input use rates are relatively high in large parts of some of the most densely populated SSA countries, such as Ethiopia, Malawi, or Nigeria (Sheahan and Barrett, 2017). Several recent studies fail to find evidence of intensification in SSA agriculture, with no statistically significant relationship between individual landholdings and input intensity (Holden and Yohannes, 2002; Binswanger-Mkhize and Savastano, 2017). Others find support for the intensification hypothesis (Pender and Gebremedhin, 2007; Headey, Dereje and Taffesse, 2014; Josephson, Ricker-Gilbert and Florax, 2014; Ricker-Gilbert, Jumbe and Chamberlin, 2014). Still other papers contain more nuanced findings: a positive relationship between population density and intensification over lower population density ranges and a reversal thereafter (Jayne and Muyanga, 2012), or increased cropping intensity but no increase in fertilizer or irrigation use (Headey and Jayne, 2014).

Virtually all studies on agricultural input intensification use farmer self-reported land size survey data, which is now known to suffer systematic measurement error (Carletto, Savastano and Zezza, 2013; Holden and Fisher, 2013; Carletto, Gourlay and Winters, 2015). Indeed, several recent studies on the farm size-productivity relationship in SSA find strong evidence that what appears as intensification – manifest as an inverse size-productivity relationship – seems merely an artifact of non-classical measurement error (NCME) in plot size, crop output, or both (Desiere and Jolliffe, 2018; Gourlay, Kilic and Lobell, 2017; Abay et al., 2018). NCME in plot size must then also affect the estimated relationship between area and input intensity, although this possibility has not yet been addressed. Moreover, the recent literature on measurement error and farm productivity in SSA – like the broader literature on measurement error in economics – assumes that farmers know and base farm management decisions upon true plot size, so that measurement error in self-reported survey data is merely an econometric obstacle to overcome through improved data collection using geographic positioning systems (GPS), satellite-based remote sensing, and other reasonably new methods. The possibility that farmers hold, act upon, and accurately report their mistaken beliefs about plot sizes has thus far not attracted attention.

In this paper we propose three distinct data generating processes behind measurement error in farmer-reported plot size and analytically derive the biases these imply for agricultural intensification parameter estimates. The first data generating process assumes that farmers know their plot size, but misreport it when surveyed, just as a parent might know her child's true age but round the value when responding to survey enumerators (Larsen, Headey and Masters, 2019). Measurement error might follow a simple regression to the mean pattern (Carletto, Gourlay and Winters, 2015), or a focal point bunching process, or one where respondents report their best (but almost surely inaccurate) predictions of the true regressor value (Hyslop and Imbens, 2001). The econometric implications of misreporting are quite similar regardless of precisely which of these underlying mechanisms holds: downward bias on the true intensification parameter when one uses farmer-reported plot size, versus an unbiased estimate of the intensification parameter when one uses instead objective, GPS-measured plot size. Importantly, due to 'division bias' (Borjas, 1980), this downward bias exists even when measurement error is classical, i.e., uncorrelated with true plot size.

Under the second data generating process, 'measurement error' in plot size solely reflects farmers' misperceptions, which guide true, observed behavior. That is, farmers may misperceive plot size and make input intensity decisions accordingly, with implications for efficiency. Such a process is consistent with a range of recent observations. Farmers adjust their inputs according to perceived crop variety, even when those perceptions are wrong (Wossen et al., 2018), or invest in inputs according to climactic predictions, even when those predictions are incorrect (Rosenzweig and Udry, 2014). As we show, the econometric implications of measurement error based purely on misperception differ from those of measurement error that reflects only misreporting. When measurement error arises from misperceptions, the estimated intensification parameter is an unbiased representation of the farmer's decision process only when one uses farmer (mis)reported plot size, or when also one controls for measurement error while using GPS-measured plot size. While the statistical association between biophysical input intensity and plot size can be unbiasedly estimated using GPS-measured plot size, this association represents a downwardly biased estimate of the farmer's intensification choice parameter when measurement error is negatively correlated with true plot size, as is generally observed and true in our data. Unlike in the misreporting case, however, no downward bias exists under classical measurement error.

Our third data generating process allows plot size measurement error to reflect both farmer misperceptions and misreporting. In this hybrid case, one must control for both GPS-measured plot size and measurement error in order to recover the farmer's intensification choice parameter. This specification also permits recovery of a novel parameter that reflects the proportion of total measurement error arising from conventional misreporting (a mere econometric challenge to be corrected) versus misperceptions (that truly affect farmer choices). This measurement error composition parameter matters to interpretation of the intensification parameter estimate. Since analysts cannot know ex ante the mechanism generating observed measurement error, this seems the most prudent approach to follow. We show that the measurement error mechanism matters materially to conclusions about agricultural intensification in SSA.

To illustrate these concepts and estimate the agricultural intensification parameter empirically, we employ nationally representative survey data from four SSA countries – Ethiopia, Malawi, Tanzania and Uganda – that include both farmer self-reports and objective, GPS measures of plot size. For each country, we study the intensity with which farmers apply the most commonly used inputs observed in the data: fertilizer, improved seed, labor, and pesticides. We test the intensification and misperceptions hypotheses for each input in each country. Consistent with the Boserupian intensification hypothesis, input intensity is indeed inversely associated with plot size in all four countries, regardless of whether measurement error represents misreporting, misperception, or a combination of the two. But the magnitudes of the intensification choice parameter differ significantly depending on the nature of measurement error.

Furthermore, conditional on true, GPS-measured plot size, measurement error in self-reported plot size is significantly, positively associated with input intensity for virtually all input-country combinations.[2] This indicates that measurement error in plot size at least partly reflects farmers' misperceptions of the land area they manage, which then affect input use decisions. The measurement error composition parameter estimates suggest that misreporting and misperceptions both arise, in roughly equal parts, for most country-input combinations. This finding implies that the use of objective (e.g., GPS) measures to 'correct' for measurement error in self-reported plot size generates an unbiased estimate of the biophysical relationship between plot size and input intensity, but such corrections can bias downwards estimates of the choice parameters that guide farmer production decisions, exaggerating the magnitude of the true (negative) intensification parameter. Meanwhile, farmer misperceptions of plot size seem to drive input allocation, with implications for the efficiency of input allocation and for agricultural productivity. The finding that measurement error represents a hybrid of misperceptions and misreporting, with consequences for estimates of key behavioral parameters, likely applies to a wide range of empirical economics questions.

The remainder of the paper proceeds as follows. In Section 2, we explore three plausible data generating processes behind observed measurement error in plot size, and examine the implications of each scenario for estimates of the input use–plot size gradient that defines Boserupian intensification. Appendix A holds full derivations for all analytical results. Section 3 lays out an estimation strategy informed by our analytical findings. Section 4 summarizes the four nationally representative panel datasets we use. Section 5 reports our results, Appendix B holds additional results, and Section 6 concludes.

2 Two-sided Measurement Error With Misreporting, Misperceptions, or Both

For many years, the most common method to explore the Boserupian intensification hypothesis was to study agricultural input intensity as a function of self-reported (log) plot size, X. More recently, analysts generally prefer to use a more objective measure of (log) plot size, X^* – collected by the survey enumeration team using a GPS or a compass and rope – so as to avoid the systematic measurement error that plagues self-reported area measures. To be more precise, the older literature often estimates equation 1, where input intensity (the log of input use, Y, per unit self-reported area,

[2]Bevis and Barrett (2018) note a similar relationship in a different dataset from Uganda.

X, expressed as log differences), as a linear function of log self-reported area X:[3]

$$Y - X = \beta^o X + \varepsilon \tag{1}$$

Under equation 1, the Boserupian intensification parameter of interest is β^o and the Boserupian hypothesis implies rejection of the zero null hypothesis in favor of $\beta^o < 0$. Given the voluminous evidence that self-reported plot size differs systematically from GPS-measured plot size, recent papers tend to assume that (log) self-reported land size, X, is measured with error, and instead estimate equation 2, using true, GPS-measured (log) plot size X^* on both the left-hand size and the right-hand side:

$$Y - X^* = \beta^* X^* + \epsilon \tag{2}$$

Following the standard representation of measurement error (Bound, Brown and Mathiowetz, 2001), log self-reported land size, X, is assumed to be a combination of true log plot size, X^*, and measurement error, v, also expressed in logarithmic form, as given in equation 3.[4]

$$X = X^* + v \tag{3}$$

Note that the measurement error enters on both sides of equation 1, which differs in important ways from the one-sided, classical measurement error model familiar from textbooks, as Borjas (1980) and Abay et al. (2018) explain.[5]

The intensification parameter represents a statistical association, with the potential for causal interpretation only if ε or ϵ are uncorrelated with X or X^*, respectively. In this analytical section, we explicitly assume that ε and ϵ are both orthogonal to X, and thus to both X^* and v. In our empirical application, below, we add household fixed effects and plot-level controls to make this assumption more plausible. It certainly remains possible, however, that omitted, relevant variables bias our estimated intensification parameters, on which more later.

We document below three measurement error scenarios. The first assumes, as is standard in the literature, that farmers misreport plot size while acting on accurate private knowledge (or predictions) of true plot size. In the second scenario, farmers misperceive their plot size and allocate inputs accordingly. In this case measurement error perfectly reflects misperception rather than misreporting. In the third scenario, farmers both misperceive and misreport plot size. We show that the appropriate regression specification to recover the intensification parameter of interest differs under each of these three scenarios. We also develop a simple test to establish the degree to which measurement error arises from misreporting versus from misperceptions.

[3] In this analytical section we use a simple bivariate regression so as to simplify the math as much as possible and make the core intuition clear. The multivariate generalization is reasonably straightforward. The empirical section uses a more general, multivariate regression framework.

[4] This functional form implies multiplicative error in the non-log version.

[5] In this analytical section, we abstract from the possibility of non-classical measurement error in Y, largely for the practical reason that objective measures of input applications remain infeasible and unavailable in most settings, certainly in the SSA agriculture data we study. In the empirical section below, however, we offer a robustness check on the possibility that NCME in farmer-reported input volumes biases the intensification parameter estimates of interest.

2.1 Scenario 1: Misreporting

Assume that farmers have perfect information on plot size, and thus base the intensification decision on true (log) plot size, X^*, as in equation 2. In responding to the survey, however, they (mis)report (log) land size with error, v, as in equation 3. Several previous studies have found measurement error in self-reported plot size is correlated with true plot area (Carletto et al. 2013; Carletto et al. 2015; Gourlay et al. 2017; Bevis and Barrett, 2018; Abay et al., 2018). This form of NCME in observed plot size can be easily represented as:

$$v = \alpha X^* + \psi \tag{4}$$

where ψ is assumed orthogonal to X^* and ϵ from equation 2. Combining equations 3 and 4 provides the following representation of NCME.

$$X = (1 + \alpha)X^* + \psi \tag{5}$$

Equation 5 nests classical measurement error, which occurs when $\alpha = 0$. If $\alpha < 0$ then this specification encompasses both regression-to-the-mean and focal-point-bunching processes commonly observed in self-reported data and found in our data.

Because equation 2 reflects the true, data-generating process and bypasses the NCME problem, estimation of equation 2 yields an unbiased estimation of the intensification parameter, β^*, which reflects both the decision-making process of the farmer and also the biophysical association between input intensity and plot size. Estimation of equation 1, however, will result in the intensification parameter estimate, $\widehat{\beta^o}$, in equation 6. This derivation and all subsequent derivations are detailed in Appendix A.[6]

$$\widehat{\beta^o} = \left[\beta^* \frac{Cov\left(X^*, X^* + v\right)}{Var\left(X^* + v\right)} - \frac{Cov\left(v, X^* + v\right)}{Var\left(X^* + v\right)}\right] = \beta^* - (\beta^* + 1)\frac{Cov\left(v, X\right)}{Var\left(X\right)} \tag{6}$$

The first term within brackets reflects the bias that enters through the right-hand side of the regression equation, and leads to attenuation bias under classical measurement error, as is well-known (Bound, Brown and Mathiowetz, 2001; Gibson and Kim, 2010). That is, when $Cov\left(X^*, v\right) = \alpha = 0$, this first term reduces to $\beta^* \frac{Var(X^*)}{Var(X)} \leq \beta^*$. The second term within brackets represents the bias arising through the dependent variable, which creates downward bias even under classical measurement error; the labor supply literature refers to this as 'division bias' (Borjas, 1980).[7] We can alternatively derive bias as in the rightmost expression, which implies that if $-1 < \beta^* < 0$ as consistent with the Boserupian intensification hypothesis, then plot size measurement error creates downward bias when $Cov\left(v, X\right) > 0$, and creates upward bias when $Cov\left(v, X\right) < 0$.

[6]We report results both in terms of $Cov(v, X)$ and of $Cov(v, X^*)$, as appropriate to the mechanism. The $Cov(v, X^*)$ framing is the more familiar measurement error representation, while $Cov(v, X)$ reflects the misperceptions hypothesis, which implicitly treats the true, objective measure X^* as the mis-measured variable from the behavioral perspective.

[7]Borjas' (1980) 'division bias' refers to a slightly different situation in which measurement error in the dependent variable (hours worked) also enters the denominator of the independent variable (hourly wage rate). The inverse labor supply function would take the same form as the intensification equation we estimate. So the two are directly analogous.

One cannot in general sign $Cov(v, X)$. But given the typical empirical finding that $Cov(v, X^*) < 0$, if $-Cov(v, X^*) > (<)\ Var(v)$, then $Cov(v, X) < (>)\ 0$. Bias only disappears in the knife edge case where $-Cov(v, X^*) = Var(v)$, making $Cov(v, X) = 0$.

Other measurement error processes can also lead to misreporting, with similar results. In Appendix A, we outline a second such process, in which survey respondents report their best estimate of a key explanatory variable (e.g., plot size) based in part on information regarding a proxy closely correlated with the dependent variable (Hyslop and Imbens, 2001). In this context, farmers accurately perceive plot size but report it based on an exactly known quantity of inputs applied. The prediction error with respect to X^* leads to slight differences in structural parameters but still yields equation 6 under this alternative form of respondent misreporting. Thus, when any of several respondent misreporting mechanisms give rise to measurement error, a spurious negative relationship could lead to mistaken inference that Boserupian intensification exists even if it does not, or might bias the intensification estimate.

2.2 Scenario 2: Misperceptions

Now we relax the assumption that farmers have and act upon perfect information on plot size. Instead, we assume that farmers misperceive plot size, make the intensification decision based on these (mistaken) beliefs, and precisely report that misperceived plot size in the survey data. More specifically, farmers mistakenly perceive (log) plot size as X and equation 1 accurately reflects their intensification decision. Equation 2 only reflects the statistical association – i.e., the true biophysical relationship – between true input intensity and true (GPS-measured) plot size; it no longer reflects the farmer's decision-making process. Equation 3 is again assumed to capture the general form of farmer misperceptions, inclusive of the measurement error in self-reported plot size.

In this scenario, we remain agnostic about the data generating process behind farmer misperceptions, except to allow that misperception v may be correlated with true plot size X^* , as in the reduced form relationships in scenario 1 (equation 4) and as we find in the data. These relationships describe the well-documented empirical pattern of regression-to-the-mean in self-reported plot size in sub-Saharan Africa (Carletto, Savastano and Zezza, 2013; Carletto, Gourlay and Winters, 2015; Bevis and Barrett, 2018; Desiere and Jolliffe, 2018; Gourlay, Kilic and Lobell, 2017; Abay et al., 2018).[8]

Combining equation 1, which represents the farmer's decision process, with equation 3, which gives the general form of measurement error in X, results in:

$$Y - X = \beta^o X^* + \beta^o v + \varepsilon \tag{7}$$

As such, the farmer's intensification parameter may be recovered by estimating either equation 1, where self-reported plot size is used on both sides, or – less typically – equation 7, where the input intensity dependent variable is computed using

[8] A more general form of equation 4, $v = f(X^*) + \psi$, accommodates focal-point-bunching for which $v = \alpha X^* + \psi$ is locally true within the basin of attraction of any given focal point, but $f(X^*)$ may be non-monotone, even discontinuous across the support of X^*. We also see evidence of focal point bunching in the data.

self-reported plot size, and self-reported plot size on the right-hand side is split into its two components: GPS-measured plot size, X^*, and measurement error, v.

Estimating equation 2, on the other hand, leads to the biased choice parameter estimate $\widehat{\beta^*}$ in equation 8:

$$\widehat{\beta^*} = \beta^o + (\beta^o + 1)\,\frac{Cov\,(v, X^*)}{Var\,(X^*)} \tag{8}$$

If $-1 < \beta^o < 0$ as one would expect under the Boserupian intensification hypothesis, then 'correcting' for plot size measurement error by estimating equation 2 recovers an downwardly biased intensification parameter when $Cov\,(v, X^*) < 0$, that is, when measurement error is negatively correlated with true plot size, as is typical and observed in our data. In comparing equations 6 and 8, note that similar bias arises over the same range for β^o or β^*, whether one estimates equation 1 when measurement error arises from misreporting or estimates equation 2 when misperceptions generate the measurement error. The main difference relative to the misreporting scenario is that under classical measurement error, the bias in equation 8 disappears completely because $Cov\,(v, X^*) = 0$. So under classical measurement error, no division bias exists in the behavioral parameter estimate.

The econometrician might also be interested in the effect of plot size misperceptions on "true" input intensity (as calculated using GPS-measured plot size), holding true plot size itself constant. That is, econometricians might wish to estimate equation 9, where the parameters on X^* and v are derived from equation 7, as shown in Appendix A.

$$Y - X^* = \beta^o X^* + (\beta^o + 1)v + \varepsilon \tag{9}$$

Note that equation 9 uses true plot size on the left-hand side rather than the farmer self-reported value, since the object of interest is the true biophysical relationship on the landscape.

2.3 Scenario 3: Misreporting and Misperception

Now assume that farmers *report* log plot size $X = X^* + v$ as in equation 3, but *perceive* log plot size as the convex combination of true and reported size, weighted by the parameter $0 \leq \theta \leq 1$:

$$\widetilde{X} = \theta X + (1-\theta)\,X^* = \theta v + X^* \tag{10}$$

Put differently, self-reported plot size now reflects both farmer misreporting and misperceptions. However, farmers base input application decisions on perceived plot size, as in equation 11, where u is assumed orthogonal to both X^* and v. This equation replaces equations 1 and 2.

$$Y - \widetilde{X} = \beta^{\triangle}\widetilde{X} + u \tag{11}$$

Under this hybrid measurement error process, the farmer's true intensification choice parameter is $\beta^{\triangle}$. Estimating equation 1 will then lead to the parameter estimate:

$$\widehat{\beta^o} = \beta^{\triangle} - \left(\beta^{\triangle} + 1\right)(1-\theta)\,\frac{Cov\,(v, X)}{Var\,(X)} \tag{12}$$

Similarly, estimation of equation 2 will lead to the parameter estimate

$$\widehat{\beta^{*}} = \beta^{\triangle} + (\beta^{\triangle} + 1)\theta \frac{Cov\,(v, X^{*})}{Var\,(X^{*})} \tag{13}$$

Equations 12 and 13 necessarily blend scenarios 1 and 2, generalizing our prior results. For example, just as in equation 6, equation 12 indicates that estimation of equation 1 – i.e., using farmer-reported plot size – generates no bias at all if there is no misreporting ($\theta = 1$), otherwise downward bias arises when $\beta^{\triangle} > -1$ and $\frac{Cov(v,X)}{Var(X)} > 0$, even with classical measurement error. The bias is now mitigated by $(1 - \theta)$, reflecting the proportion of farmer perceptions that reflect true plot size values. Similarly, equation 13 indicates that estimation of equation 2 – i.e., using GPS-measured plot size – generates no bias at all if the farmer suffers no misperceptions ($\theta = 0$), indicating that measurement error is purely misreporting. But if the farmer does suffer misperceptions ($\theta > 0$), then no bias exists if measurement error is classical ($Cov\,(v, X^{*})$=0). But under the more common finding that $Cov\,(v, X^{*}) < 0$, then the intensification estimate is downwardly biased for $\beta^{\triangle} > -1$.

Equations 12 and 13 illustrate that when $0 < \theta < 1$, neither equation 1 nor equation 2 estimate the intensification parameter that guides farmer behavior. Estimating equation 11 is of course impossible because $\widetilde{X}$ is not observed. However, we may derive equation 14 (as done in Appendix A), which includes only observed variables:

$$Y - X^{*} = \beta^{\triangle} X^{*} + \left(\beta^{\triangle} + 1\right)\theta v + u \tag{14}$$

Equation 14 models true input intensity as a function of both true plot size and plot size measurement error, recovering the unbiased estimate of the intensification choice parameter no matter the mechanism generating the measurement error. The fact that equation 14 estimates $\beta^{\triangle}$ by including v as a regressor should not be surprising; the functional form of the bias in equations 6, 8, 12, and 13 all match the functional form of omitted variable bias. That is, the bias is weighted precisely by the coefficient on measurement error v, should v be regressed on the endogenous variable of interest, namely X^{*} or X (Wooldridge, 2010). Thus equation 14 offers a preferable specification to estimate the relationship of interest in the presence of measurement error generated by an unknown mechanism.

In this more flexible, hybrid case, since $0 \leq \theta \leq 1$, the coefficient estimate on v might be less than $(\beta^{\triangle} + 1)$ as is implied in the more restricted version in equation 9. In fact, one can estimate the parameter θ by dividing the coefficient estimate on v by one plus the coefficient estimate on X^{*}. If $\theta = 1$, then farmers perceive log plot size precisely as they report it, and act accordingly. In the other bounding case, if $\theta = 0$, then farmers' plot size perceptions perfectly align with true plot size, implying that measurement error reflects misreporting only. So the θ parameter estimate directly decomposes measurement error between farmer misreporting and misperceptions.W

Table 1 summarizes the bias associated with estimation of equation 1 (using self-reported measures of plot size, X), equation 2 (using true plot size, X^{*}), and the more general equation 14 (including measurement error, v, along with X^{*}), under each scenario. We use the parameter β to denote the farmer's intensification parameter in all cases (i.e., to denote β^* under scenario 1, β^o under scenario 2, and $\beta^\triangle$ under scenario 3). Note that the covariance conditions are satisfied in our data and $\beta \leq -1$ seems highly implausible. The key takeaway of the table and this analytical section is that the standard regression specifications used to estimate the agricultural intensification parameter – equations 1 or 2 – only generate an unbiased estimate of the farmer behavioral parameter of interest under the strong assumption that one knows the true mechanism behind the measurement error – misreporting or misperceptions – and that the true process is not a hybrid of the two. The preferred specification includes both the true plot size and the measurement error as regressors in order to generate unbiased coefficient estimates under more general conditions.

Table (1) Bias in estimated farmer's behavioral intensification parameter

Measurement error is: **Regression includes:**	**Misreporting only (scenario 1):** $\theta = 0$	**Misperception only (scenario 2):** $\theta = 1$	**Mixed misreporting and misperception (scenario 3):** $0 < \theta < 1$
Only self-reported X **(equation 1)**	Downward bias when $\beta > -1, \frac{Cov(v,X)}{Var(X)} > 0$: $-(\beta+1)\frac{Cov(v,X)}{Var(X)}$	Unbiased	Downward bias when $\beta > -1$, $\frac{Cov(v,X)}{Var(X)}$ >0: $-(\beta+1)(1-\theta)\frac{Cov(v,X)}{Var(X)}$
Only true measure X^* **(equation 2)**	Unbiased	Downward bias when $\beta > -1, \frac{Cov(v,X^*)}{Var(X^*)} < 0$: $+(\beta+1)\frac{Cov(v,X^*)}{Var(X^*)}$	Downward bias when $\beta > -1, \frac{Cov(v,X^*)}{Var(X^*)} < 0$: $+(\beta+1)\,\theta\frac{Cov(v,X^*)}{Var(X^*)}$
Both true measure X^* **and measurement error** v **(equation 14)**	Unbiased	Unbiased	Unbiased

Finally, we note that $\widehat{\theta}$ is only a sample mean estimate. This could reflect a mixture distribution among farmers, with some $\widehat{\theta}$ share of respondents accurately reporting misperceptions and the complementary $1-\widehat{\theta}$ share misreporting. Or it could reflect hybrid measurement error throughout the population of the sort we model in equation 14. Although one can only make sample-level inferences with $\widehat{\theta}$, it nonetheless tells us a great deal about whether the conventional approach to measurement error, of assuming it all reflects misreporting, holds in the population sampled.

3 Empirical Strategy

Section 2 points to an empirical strategy for exploring the implications of plot size measurement error for inference about the agricultural intensification, and for testing for misreporting versus misperception in measurement error more generally. First, we study the patterns of measurement error in self-reported plot size. Then, we explore the consequences of such measurement error for inference about agricultural intensification, relying on a multivariate extension of equation 14 for our preferred specification. Finally, we test the misperceptions hypothesis and estimate the measurement error decomposition parameter in farmer self-reported plot size. We retain the notation of section 2, but now interpret X^* more specifically as log GPS-measured plot size, and X as log farmer-reported plot size. Measurement error v is defined precisely as before, obtained through simple subtraction, as $v \equiv X - X^*$.

3.1 Understanding Measurement Error

We begin by testing whether measurement error in farmer-reported plot size behaves classically or non-classically, i.e., whether it is correlated with variables of interest, especially true plot size. The misreporting scenario 1 implies a reduced form correlation between true log plot size and measurement error, meaning that error is non-classical. Equation 15 lets us test whether we can reject the classical measurement error null hypothesis that $\pi = 0$, in favor of the NCME alternate hypothesis.[9]

$$v = \pi X^* + \eta Z + \xi \tag{15}$$

Even if we fail to reject the NCME null, two-sided measurement error has the potential to negatively bias the intensification parameter when estimated using self-reported area under the misreporting or hybrid misreporting and misperceptions scenarios. Appendix Figure A1 illustrates that the relationship between v and X^* indeed appears reasonably linear throughout most of our sample. However, we also relax the linearity assumption in subsequent robustness checks.

The parameter π estimates a conditional version of $\frac{Cov(v,X^*)}{Var(X^*)}$, with implications for the direction of bias in equations 8 and 13. Because the bias specified by equations 6 and 12 relies instead on $\frac{Cov(v,X)}{Var(X)}$, we also estimate equation 16, in which the parameter ϖ similarly estimates that partial correlation coefficient:

$$v = \varpi X + \rho Z + \varrho \tag{16}$$

We estimate equations 15 and 16 with and without controls Z, though always including a vector of ones, corresponding to an intercept term in η. In all four countries, Z includes household and year fixed effects, all the household and plot characteristics listed in Table 2, and country-specific dummy variables indicating the crops being grown on each plot.[10]

3.2 Testing the Intensification Hypothesis in the Presence of Measurement Error

The second step in our empirical strategy explores the impact of measurement error for estimates of the Boserupian intensification parameter of interest. Our dependent variables are input intensity with respect to four different factors of production: labor, which is central to the original Boserupian hypothesis, and the three most common modern inputs found in the data: inorganic fertilizer, improved seed, and pesticides. We begin by operationalizing equations 1 and 2, again adding control variables, Z:

$$Y - X = \beta^o X + \tau^o Z + \zeta \tag{17}$$

$$Y - X^* = \beta^* X^* + \tau^* Z + \varsigma \tag{18}$$

[9] The parameter π may reflect any of several structural parameters arising under different measurement error processes, as enumerated in Appendix A, each producing a similar, reduced form relationship between v and X^*.

[10] In Uganda, where we view two agricultural seasons per year, a season fixed effect is also included.

The conditional regressions make more plausible the assumption that the error term is uncorrelated with GPS-measured plot size, although we cannot claim this to necessarily be so. In the empirical section below, we do not make strict causal claims.

Under scenario 1 farmers act on perfect information on true plot size, now captured by β^*. Therefore, the Boserupian intensification parameter of interest, β^* from equation 2, can be consistently estimated via equation 18 if ς is conditionally orthogonal to X^*. The intensification hypothesis holds that one can reject the null $\beta^* = 0$ in favor of the one-sided alternate hypothesis that $\beta^* < 0$, i.e., that input intensity declines with plot size. Under scenario 1, $\widehat{\beta^o}$ from equation 17 is a biased estimate of the true intensification parameter, downwardly biased if $\beta^* > -1$ and measurement error in plot size is negatively correlated with GPS-measured plot size.

Under scenario 2, however, farmers accurately report their misperceptions, and so their decision making process is specified by equation 17, even if the biophysical relationship on the landscape is more accurately represented by equation 18. In this case, $\widehat{\beta^o}$ from equation 17 is an unbiased estimate of the Boserupian intensification parameter of interest when ζ is conditionally orthogonal to X, and the $\widehat{\beta^*}$ estimate from equation 18 captures only a biophysical relationship, not patterns of human choice. Additionally, farmers may act on a perception of plot size that falls somewhere between GPS-measured plot size and reported plot size as under scenario 3. In this case, neither the $\widehat{\beta^o}$ estimate from equation 17 nor the $\widehat{\beta^*}$ estimate from equation 18 provide an unbiased estimate of the Boserupian intensification parameter of interest, per Table 1. Correct interpretation of the estimation results of equations 17 and 18 therefore requires knowing whether farmers misreport plot size, misperceive plot size, or both. Next, we test the misperceptions hypothesis in order to identify the underlying measurement error data generating process.

3.3 Testing the Misperceptions Hypothesis

Under scenarios 2 and 3 in section 2, v reflects a measurement error that is less an econometric challenge due to a flawed survey process than a misperception that leads farmers to make systematic errors. We test the misperceptions hypothesis for this difference by estimating the conditional analogue of the unconditional relationship in equation 14:

$$Y - X^* = \gamma X^* + \varphi v + \tau^\diamond Z + w \tag{19}$$

Note that equation 19 is just a more general version of equation 18, which relaxes the exclusionary restriction that $\varphi = 0$, as is implied by equation 18. That restriction holds only under scenario 1, when measurement error has no behavioral effect and thus should be unrelated to input intensity once conditioned on true plot size, X^*. Thus, under our assumptions from section 2, rejecting the null hypothesis that $\varphi = 0$ in equation 19 confirms the misperceptions hypothesis, that at least part of v reflects misperceptions on which farmers truly act. In the conditional context these assumptions necessitate that the random component of measurement error is orthogonal to input levels Y, or more simply that v is conditionally orthogonal to w in equation 19.

If scenario 2 holds and the farmer perceives plot size exactly as she reports it in survey data – i.e., if v contains *only* farmer misperception and no misreporting and the farmer's decision process therefore follows equation 17 – then we should find $\widehat{\varphi} = 1 + \widehat{\gamma}$ and also $\widehat{\gamma} = \widehat{\beta^o}$. Conversely, if scenario 1 holds, and v exclusively represents farmer self-reporting errors on which farmers do not act, then we should find $\widehat{\gamma} = \widehat{\beta^*}$. That is, under scenario 1, the intensification parameter estimate is equivalent under equation 18 and its more general form, equation 19, because then v is just a noise parameter.

If the farmer's perception of plot size lies somewhere between GPS-measured plot size and self-reported plot size, however, then v contains both farmers' misperception and misreporting, thus $\widehat{\varphi}$ may be less than $\widehat{\gamma} + 1$, because in this case $\theta<1$. We can estimate $\widehat{\theta}$ from equation 19 as in equation 20.

$$\widehat{\theta} = \widehat{\varphi}/\left(\widehat{\gamma} + 1\right) \tag{20}$$

The parameter estimate $\widehat{\theta}$ represents the share of the observed measurement error due to farmer misperceptions. Tests of the two bounding null hypotheses, that v represents purely misperceptions (when $\widehat{\theta} = 1$), or only misreporting (when $\widehat{\theta} = 0$), therefore help identify which measurement error scenario most accurately describes the data. Since $\theta \in [0, 1]$, the predicted value $\widehat{\theta}$ is necessarily censored at the lower and upper bounds of the unit interval.

So as to increase the efficiency of the non-nested hypothesis tests, we estimate equations 17-19 via seemingly unrelated regressions (SUR) with shared fixed effects (Blackwell, 2005). Due to the fact that fixed effects are shared across equations, however, SUR estimates vary slightly from the single equation OLS estimates. We therefore exploit SUR for conducting joint hypothesis tests of parameters, but base hypothesis tests around single parameters on the single equation OLS results, which includes equation-specific fixed effects. We solve for $\widehat{\theta}$ according to equation 20, using the OLS-estimated parameters $\widehat{\gamma}$ and $\widehat{\varphi}$ from equation 19, bootstrapping our confidence intervals for $\widehat{\theta}$ so as to accommodate prospective non-normality in the distributions of either parameter estimate.[11]

4 Data

We use Living Standard Measurement Study-Integrated Surveys on Agriculture (LSMS-ISA) data from four countries: Ethiopia, Malawi, Tanzania, and Uganda. These datasets have been collected by national statistical offices, in partnership with the World Bank, under country-specific labels. These high quality, nationally representative, agriculture-focused panel data are fairly comparable across countries. The Ethiopian Socioeconomic Survey data contain three rounds of household panel data, collected in 2011/12, 2013/14, and 2015/16. Malawi's Integrated Household Panel Survey also includes three rounds of household panel data, collected in 2010/11, 2013, and 2016/17. In both Tanzania and Uganda we have three rounds of household panel data and a fourth wave of non-panel data (i.e., the fourth waves cover new respondent households). Uganda's National Panel Surveys took place in 2009/10, 2010/11, 2011/12, and 2012/14. Tanzania's National Panel Surveys took place in 2008/9, 2010/11, 2012/13, and 2014/15.

[11] We use Stata's percentile confidence intervals, and run 1,000 replications.

In each country we observe either multiple agricultural plots or multiple agricultural land parcels per household, and agricultural input and output data are reported at the plot/parcel level. Parcels are defined as contiguous land under the same ownership system. Plots are defined by cropping system, and are located within parcels. In Malawi and Ethiopia data are at the plot level; in Uganda and Tanzania data are necessarily at the parcel level, since parcels were measured for size rather than plots.[12] For convenience we refer to all plots and parcels as "plots" throughout the paper, and return during discussion to the possible implications of viewing size and measurement error for parcels, rather than plots, in Uganda and Tanzania. Uganda's panel includes two observations of many plots per year, covering both agricultural seasons, which are relatively equal in importance. In Tanzania we include only data from the main *masika* season, as the great majority of farmers do not grow crops during the secondary *vuli* season. In both Ethiopia and Malawi, only the main rainy season is included.

In every survey farmer respondents report plot size and then surveyors measure the land via handheld GPS units, to avoid influencing self-reported size (Carletto, Gourlay and Winters, 2015). These two measures allow us to examine measurement error in farmer-reported size, relative to GPS measures of the same plot. Plots without GPS-measured size are necessarily dropped: this amounts to about 5, 15, 39, and 52 percent of the listed plots in Ethiopia, Malawi, Tanzania, and Uganda, respectively.[13] Pooled across all survey rounds, this leave us with 36,304 plots in Ethiopia, 53,460 plots in Malawi, 13,855 plots in Tanzania, and 13,855 plots in Uganda, all with both measures of size. Because we drop a substantial portion of plots due to missing GPS measures, Table A1 examines this attrition according to covariate characteristics. In most countries it appears that more valuable plots (irrigated plots, plots growing cash crops or crops central to food security) are more likely to be measured via GPS. In Tanzania and Uganda, self-reported plot size is not statistically significantly related to attrition from the nationally representative data. However, that is not the case in Ethiopia and Malawi. Patterns in attrition underscore that our findings only hold in this sample.

Table 2 provides summary statistics for households and plots for all four countries. Ethiopia, Malawi, and Uganda are densely populated and land constrained (Headey and Jayne 2014, Jayne, Chamberlin and Headey, 2014). Since Tanzania is less densely populated and less land-constrained, it is unsurprising that farms are larger in Tanzania than in the other countries. While Tanzanian farms average 8.4 acres, Ugandan farms average just 5.1 acres, Ethiopian farms 3.6 acres, and Malawian farms only 3.5 acres. Malawi and Ethiopia also have the smallest average plot sizes: 0.4 acres in Ethiopia, and 1.1 acres in Malawi. Uganda and Tanzania have larger plots (2.4 and 2.9 acres, respectively), since the units of observations are actually land parcels. Per capita farm size average is largest in Tanzania (1.5 acres) and smallest in Malawi (0.7 acres).

[12] More specifically, Ethiopian plots (called fields in survey) are small, defined units of land within a parcel, demarcated by hedges or paths and generally assigned for a specific crop. In Malawi plots are defined as "a continuous piece of land on which a unique crop or a mixture of crops is grown under consistent crop management system." In Tanzania parcels (called plots in survey) are defined as a contiguous piece of land under a single form of tenure. In Uganda, a parcel is defined as a contiguous piece of land with uniform tenure and land characteristics.

[13] In Ethiopia we also dropped about 15 percent of plots, for which we cannot find the conversion factor for local land area measurement units. This problem has also been noted in other studies relying on these data. In Tanzania 47 percent of dropped plots are in round 1 (2008/9), during which very few plots were measured using GPS.

Table (2) Pooled summary statistics of households and plots

	Ethiopia	Malawi	Tanzania	Uganda
Land area measurement				
Self-reported plot size (acres)	0.40 (1.48)	0.97 (0.78)	2.58 (6.07)	1.92 (5.22)
GPS-measured plot size (acres)	0.43 (1.37)	1.12 (9.08)	2.94 (7.06)	2.43 (14.53)
Farm size (acres)	3.60 (11.6)	3.49 (3.42)	8.41 (18.65)	5.11 (23.45)
Household characteristics:				
Male household head (%)	0.85 (0.36)	0.74 (0.44)	0.77 (0.42)	0.72 (0.45)
Age of household head (years)	47 (14.26)	44 (16.32)	49 (15.65)	47 (14.99)
Household-head literate (%)	0.58 (0.49)	0.13 (0.34)	0.70 (0.46)	0.65 (0.48)
Household size (# persons)	5.65 (2.21)	4.81 (2.17)	5.95 (3.50)	7.02 (3.27)
Acres per person (acres/person)	0.85 (0.36)	0.74 (0.44)	1.52 (2.79)	0.93 (7.63)
Plot characteristics				
Pure stand cropping (%)	0.62 (0.48)	0.60 (0.49)	0.47 (0.50)	0.44 (0.49)
Irrigated plot (%)	0.04 (0.2)	0.01 (0.08)	0.02 (0.13)	0.02 (0.13)
Soil quality perceived as good (%)	0.31 (0.46)	0.48 (0.5)	0.40 (0.49)	0.60 (0.49)
Steep slope (%)	0.12 (0.32)	0.11 (0.31)	0.03 (0.18)	0.11 (0.31)
Plot is owned (%)	0.87 (0.33)	0.85 (0.36)	0.86 (0.35)	0.89 (0.32)
Number of crops grown (#)	1.52 (0.84)	1.98 (1.01)	1.69 (0.92)	2.48 (1.77)
Observations	36,304	53,460	13,855	19,754

Notes: Except for land areas, the variables above are self-reported by farmers. Farm size relies on GPS-measured size for plots with GPS measurement, and self-reported size for plots without GPS measurement. Mean values are outside parenthesis, standard deviations are inside parentheses.

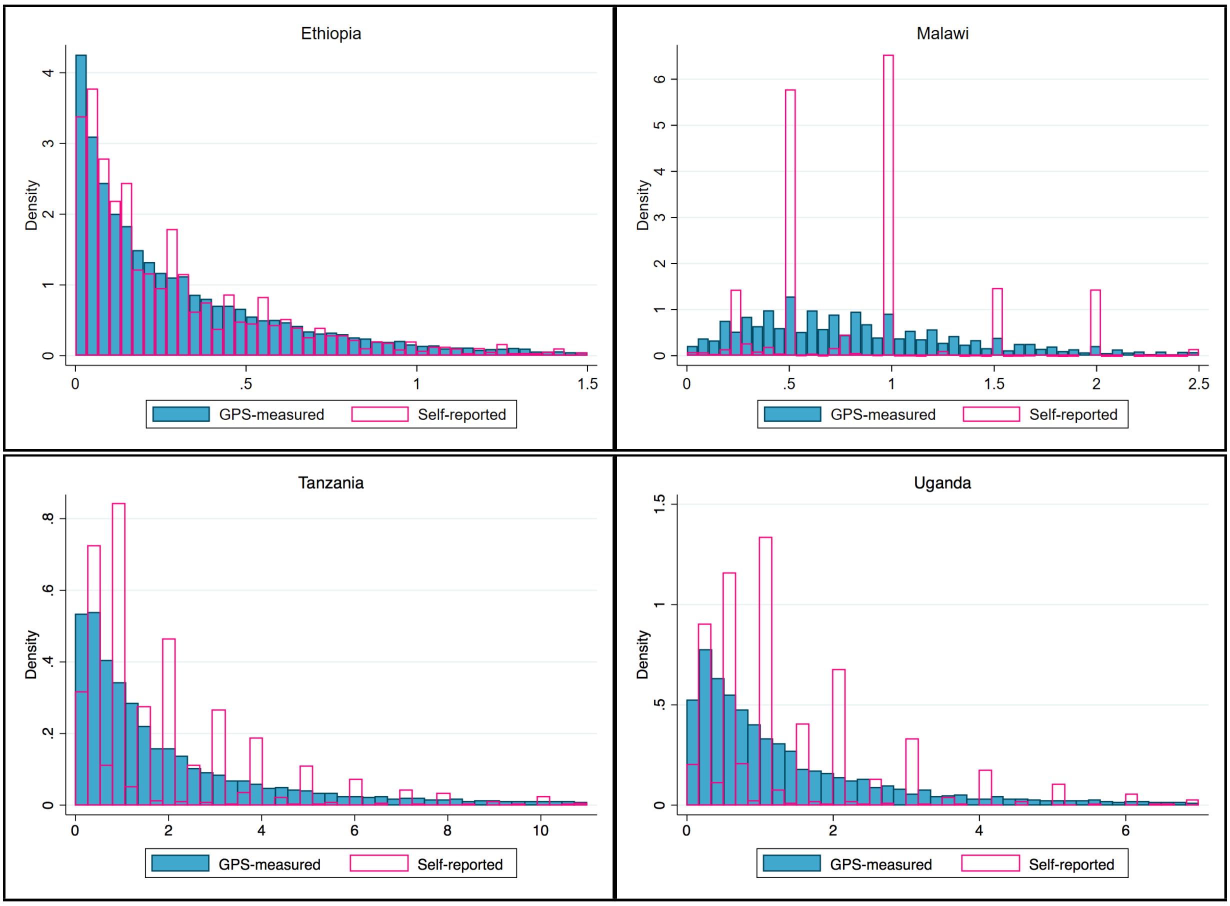
Because the plot size distribution is skewed, these means exceed the median plot/parcel sizes of 0.22, 0.78, 0.94, and 1.26 acres, in Ethiopia, Malawi, Uganda and Tanzania, respectively. In Ethiopia, about 95% percent of plots are under 1 acre, and almost all plots are under 5 acres. In Malawi these figures are about 75 and 99 percent, respectively. In Uganda, they are 52 and 92, and in Tanzania, they are 42 and 85, respectively. So, all four countries' agriculture is smallholder-dominated with cultivation on very small plots of land.

Most other household and plot characteristics are similar across countries. Household heads are slightly more likely to be male in Ethiopia, and are notably less likely to be literate in Malawi. Household sizes are largest, on average, in Uganda, which also has the most diversified cropping system. Ethiopia and Malawi, which are heavily reliant on teff and maize, respectively, are most likely to have mono-cropped plots. Irrigation is almost non-existent in all countries. Ethiopia's farmers are least likely to list their plot as having good soil quality, while Uganda's farmers are most likely to do so.

The differences between average self-reported and average GPS-measured plot size in Table 2 are fairly substantial: 7.5, 13.4, 14.01, and 26.6 percent of the mean GPS-measured plot size in Ethiopia, Malawi, Tanzania and Uganda, respectively. Part of this discrepancy is driven by a tendency of self-reports to bunch around integers and simple fractions (e.g., 0.5, 1, 1.5, 2 acres) that serve as natural focal points. Figure 1 depicts histograms that illustrate this bunching, which varies in magnitude across countries but is omnipresent.

More importantly, the bias in mean, self-reported plot size varies substantially over the distribution of true, GPS-measured plot size. In Table 3 we calculate bias as the difference between quartile-specific mean self-reported plot size and quartile-specific mean GPS-measured plot size. Relative bias is given as quartile-specific (mean) bias as a percent of (mean) GPS-measured plot size. The totals in the bottom row report the same calculation for the entire sample.

Figure (1) Distribution of self-reported and GPS plot sizes



Consistent with prior studies (Carletto, Savastano and Zezza, 2013; Carletto, Gourlay and Winters, 2015), measurement error in self-reported plot size declines with true, objectively measured plot size, in both absolute and percentage terms. Relative bias is by far the largest for the smallest plots, the size of which tend to be drastically over-estimated by farmers. Bias and relative bias are low – close to zero – in the third quartile, for all countries. Notably, bias is positive for all quartiles except for the last; only the largest plots are generally under-estimated. However, the extent of under-estimation on these large plots is so great that it drives country-average self-reported plot size to be lower than country-average GPS-measured plot size, as seen in Table 2 and at the bottom of Table 3. This is notable since bias actually goes in the other direction for the vast majority of plots.

Table (3) Discrepancies between Self-reported and GPS-based measures

Plot Size	Ethiopia		Malawi		Tanzania		Uganda	
Quartile (GPS)	Bias (SR-GPS)	Relative bias (%)	Bias (SR-GPS)	Relative bias (%)	Bias (SR-GPS)	Relative bias (%)	Bias (SR-GPS)	Relative bias (%)
0-25%	0.04	136.54	0.26	95.43	0.34	130.11	0.24	103.15
25-50%	0.02	17.50	0.14	22.56	0.23	27.14	0.19	28.55
50-75%	0.00	0.63	0.04	4.18	0.01	0.80	0.12	8.73
75-100%	-0.18	-14.78	-1.01	-38.75	-2.03	-23.04	-2.60	-34.90
Total	-0.03	-6.36	-0.14	-12.71	-0.36	-12.14	-0.51	-21.09

Notes: GPS denotes acres measured using handheld GPS units; SR denotes self-reported acres. SR-GPS provides quartile-specific mean differences. Relative differences are as a percent of (mean) GPS-measured plot size.

In Table 4 we report agricultural intensification rates, considering four key inputs and using both self-reported and GPS-based plot sizes to calculate intensity. For all countries, we compute these conditional intensification rates only for those plots on which these inputs were applied. The sample sizes in Table 4 are therefore smaller than those in Table 2. Additionally, a few small differences exist across countries due to the way the data were collected. In Uganda and Tanzania, data on improved seed use exists only for purchased seed, and is expressed in monetary value terms. Since most improved seed is hybrid, which loses vigor if one uses retained seed from a first crop, the understatement of improved seed use by using purchased improved seed is likely low. For Ethiopia we use improved seed application in kilograms. In Malawi, we cannot identify improved and traditional seed variety for the initial survey rounds, so our analysis for improved seed only uses the last survey round. Additionally, pesticide use is given only as a binary indicator in Ethiopia, so although we describe it here, we omit it from the later intensification regressions because those would only describe change at the extensive margin, and thereby miss the Boserupian intensification of interest. For Malawi, pesticide use is given in kilograms or liters. For Tanzania and Uganda, it is measured in monetary value of agrochemical applications per hectare.

Given the log-normal distribution of input intensity, Table 4 reports both mean and median application rates. The median and average application rates for most countries are comparable to those previously reported (e.g., Sheahan and Barrett, 2017). For Malawi, Tanzania, and Uganda, median input rates are similar across GPS and self-reported measurement, while mean input rates are substantially higher under GPS measurement. This reflects the under-estimation of input intensity on small plots, which are reported by farmers as larger than they truly are. In Ethiopia, the same holds true for fertilizer, while mean input intensity actually appears higher under GPS-measurement for labor intensity and improved seed intensity. Again, however, median input use is comparable across the two measurement methods for Ethiopian inputs. These results, and those of Table 3, suggest that while mean input use rates are biased when generated with self-reported land size, this is generally driven by bias in land size for the smallest and largest plots. Median input use is therefore more reliable if generated with self-reported land measurement – it is less biased, and also better captures the central tendency of a skewed distribution.

Table (4) Input use rates (for those plots that received the input)

	Input applied		Mean input intensity		Median input intensity	
	# plots	% plots	(Self-report)	(GPS)	(Self-report)	(GPS)
Ethiopia						
Labor (days/acre)	33,442	92	93.84	109.22	27.27	27.05
Fertilizer (kg/acre)	9,649	27	165.43	117.17	56.81	50.40
Improved seed (kg/acre)	13,275	36	75.81	64.54	19.65	17.75
Malawi						
Labor (days/acre)	48,914	91	247.80	276.66	165.00	177.78
Fertilizer (kg/acre)	32,866	61	78.72	91.76	50.00	50.01
Improved Seed (kg/acre)	4,976	9	16.22	17.74	10.00	9.21
Pest/herbicide (kg/acre)	1,121	2	20.74	26.40	0.80	0.71
Tanzania						
Labor (days/acre)	11,649	0.84	49.14	69.12	30.00	32.67
Fertilizer (kg/acre)	1,455	0.11	214.60	263.64	40.00	42.86
Improved Seed (value/acre)	1,552	0.11	15.85	19.66	8.00	9.13
Pest/herbicide (value/acre)	1,228	0.09	14.38	16.28	6.25	6.67
Uganda						
Labor (days/acre)	16,506	0.84	193.56	227.02	102.00	106.67
Fertilizer (kg/acre)	243	0.01	27.57	28.54	7.78	7.69
Improved Seed (value/acre)	1,345	0.07	16.99	20.05	8.33	8.57
Pest/herbicide (value/acre)	914	0.05	18.55	30.17	7.50	7.45

Notes: For Tanzania and Uganda improved seed and pesticide/herbicide application is given in terms of value purchased per acre. All other inputs are in kg/acre, except for labor which is listed in days/acre.

5 Empirical Results and Discussion

Following the empirical strategy outlined in section 3, we first estimate the reduced form relationship between measurement error in farmer-reported plot size, as reported in Table 3, and plot size as measured by GPS and as reported by farmers. If measurement error is correlated with GPS-measured plot size, it behaves non-classically. Furthermore, the size and direction of these relationships have implications for the size and direction of bias in the estimated intensification parameter. We next estimate the intensification parameter of interest according to both GPS-measured and self-reported plot size using the standard specification, as well as with our preferred, more general specification. Last, we test the farmer misperceptions hypothesis and estimate the share of observed plot size measurement error attributable to misperceptions on which farmers act, versus simple misreporting in response to survey questions.

Table 5 reports OLS estimates of equation 15, the linear relationship between measurement error in plot size and true plot size, in odd columns. Even columns report estimates of equation 16, the linear relationship between measurement error and self-reported plot size. As described in section 4, measurement error is defined as the difference between log-transformed self-reported and GPS-measured plot size. All columns include the household and plot characteristics in Table 2 as well as household, round, and crop fixed effects.[14] Appendix Table A2 reports the unconditional relationships. For all countries we easily reject the classical measurement error null

[14]Note that household fixed effects also control for community-level factors, such as availability of uncultivated land, that might impact intensification behaviors.

hypothesis (that π=0) in favor of the NCME alternate, specifically that measurement error is negatively associated with true plot size, e.g., experiences regression to the mean. This is consistent with previous studies that find that farmers generally over-estimate the size of small plots and under-estimate the size of large plots (Carletto et al., 2013; 2015; Bevis and Barrett, 2018; Desiere and Joliffe, 2018; Abay et al., 2018). The magnitudes of these precisely estimated relationships range from -0.3 to -0.5 suggesting that measurement error is quite large, as well as reasonably linear across the plot size distribution, as reflected in appendix Figure A1. We likewise show that measurement error is positively, precisely, and statistically significantly correlated with self-reported plot size in all countries. Per section 2, these relationships imply downward bias in the intensification parameter estimates generated using self-reported area measures unless measurement error exclusively represents farmer misperceptions of plot size, and downward bias again when using GPS-measured area, unless measurement error is purely misreporting.

Table (5) Measurement error relationship with GPS-measured and self-reported plot size

	Ethiopia		Malawi		Tanzania		Uganda	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln (GPS plot size):	-0.304***		-0.490***		-0.396***		-0.426***	
	(0.010)		(0.010)		(0.0127)		(0.0167)	
ln (SR plot size):		0.141***		0.223***		0.166***		0.411***
		(0.009)		(0.015)		(0.0176)		(0.0206)
Observations	35483	35483	53416	53416	9197	9197	16582	16582
R^2	0.234	0.055	0.388	0.067	0.339	0.0881	0.304	0.286

Notes: Standard errors, clustered at household level, are given in parentheses. All regressions include household, year and crop fixed effects, plot and household controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Having established the predicted directions of bias under each form of plot area measurement, we report OLS estimates for equations 17 and 18 in Table 6. These equations reflect the standard regression specifications in the literature but are only unbiased under strong assumptions about the measurement error mechanism at play (Table 1). We use the same controls as in Table 5. Each input is a separate column. Each country is a separate vertically stacked panel within the table. For each country and input, the intensification parameter is first estimated using self-reported plot size ($\widehat{\beta}^o$ from equation 17), and then estimated in a separate regression using GPS-measured plot size ($\widehat{\beta}^*$ from equation 18). In the last row within each country panel, we estimate the difference between the two estimates, ($\widehat{\beta}^o - \widehat{\beta}^*$), purely to check whether the choice of plot size measure matters to the resulting point estimates. As these estimates come from non-nested regressions, we estimate equations 17-19 via seemingly unrelated regressions (SUR) with shared fixed effects and use the resulting covariance matrix to test the null hypothesis that the two parameters are equal (Blackwell, 2005).

Regardless of land area measurement methods, the estimates in Table 6 consistently show a strong inverse relationship between plot size and input use intensity for all countries and for all inputs, consistent with the Boserupian hypothesis and with many previous studies (Pender and Gebremedhin, 2007; Headey, Dereje and Taffesse, 2014; Josephson et al., 2014; Ricker-Gilbert, Jumbe and Chamberlin, 2014; Sheahan and Barrett, 2017).[15] Intensification parameters range from -0.4 to -0.8 across inputs,

[15]This inverse relationship holds if one allows for non-linear relationships, as given in Table A3.

Table (6) Intensification parameter estimates with and without measurement error

	(1) Labor	(2) Seeds	(3) Fertilizer	(4) Pesticides
Ethiopia				
ln (SR plot size): $\widehat{\beta}^o$	-0.805***	-0.469***	-0.475***	
	(0.005)	(0.013)	(0.017)	
Observations	*32750*	*12927*	*9430*	
R^2	*0.726*	*0.389*	*0.248*	
ln (GPS plot size): $\widehat{\beta}^*$	-0.800***	-0.473***	-0.444***	
	(0.004)	(0.012)	(0.017)	
R^2	*0.778*	*0.420*	*0.237*	
$\widehat{\beta}^o - \widehat{\beta}^*$	-0.005	0.003	0.031***	
Malawi				
ln (SR plot size): $\widehat{\beta}^o$	-0.546***	-0.464***	-0.565***	-0.607***
	(0.012)	(0.063)	(0.021)	(0.084)
Observations	*48791*	*4970*	*32795*	*1117*
R^2	*0.335*	*0.355*	*0.282*	*0.515*
ln (GPS plot size): $\widehat{\beta}^*$	-0.622***	-0.562***	-0.614***	-0.585***
	(0.009)	(0.042)	(0.017)	(0.096)
R^2	0.509	0.433	0.419	0.528
$\widehat{\beta}^o - \widehat{\beta}^*$	0.076***	0.098***	0.050***	-0.021*
Tanzania				
ln (SR plot size): $\widehat{\beta}^o$	-0.542***	-0.604***	-0.439***	-0.671***
	(0.0142)	(0.0464)	(0.0571)	(0.0650)
Observations	*9175*	*1487*	*1305*	*949*
R^2	*0.340*	*0.373*	*0.218*	*0.389*
ln (GPS plot size): $\widehat{\beta}^*$	-0.593***	-0.635***	-0.496***	-0.791***
	(0.0109)	(0.0371)	(0.0503)	(0.0474)
R^2	*0.484*	*0.477*	*0.314*	*0.503*
$\widehat{\beta}^o - \widehat{\beta}^*$	0.050***	0.031*	0.057	0.120**
Uganda				
ln (SR plot size): $\widehat{\beta}^o$	-0.770***	-0.788***	-0.579***	-0.642***
	(0.0151)	(0.0800)	(0.171)	(0.0783)
Observations	*16164*	*1335*	*241*	*906*
R^2	*0.350*	*0.263*	*0.555*	*0.262*
ln (GPS plot size): $\widehat{\beta}^*$	-0.734***	-0.784***	-0.432**	-0.706***
	(0.0148)	(0.0837)	(0.189)	(0.0752)
R^2	*0.390*	*0.316*	*0.597*	*0.309*
$\widehat{\beta}^o - \widehat{\beta}^*$	-0.036***	-0.004***	-0.147***	0.064***

Notes: Standard errors are clustered at the household level. The bottom row for each country presents the difference between the OLS parameter estimates $\widehat{\beta}^o$ and $\widehat{\beta}^*$. Significance of this difference is obtained via seemingly unrelated regression (SUR) with shared fixed effects (Blackwell, 2005). All regressions include household, year and crop fixed effects, plot and household controls. For all rows, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

countries, and plot size measurement methods. Boserupian intensification appears a robust finding, although all of these estimates will be downwardly biased if measurement error represents a mixture of misreporting and misperception.

The value of the difference between the two parameter estimates ($\widehat{\beta}^o - \widehat{\beta}^*$) varies by country, and generally falls in the range of 10-20 percent of either estimate. Most of those differences are significant at the one percent level, so choice of measure does matter. But unless measurement error reflects only pure misperceptions or pure misreporting, both estimates are biased.

Table 7 reports OLS estimates of equation 19, now controlling for measurement error in plot size v alongside GPS-measured plot size, X^*. This is our preferred specification because, as shown in section 2, the coefficient estimate, $\widehat{\gamma}$, on GPS-measured plot size offers an unbiased estimate of the intensification parameter of interest, the parameter shaping farmer decisions, no matter the data generating process behind measurement error in plot size. That parameter estimate is consistently negative, statistically significant, and of relatively large magnitude, falling in the range [-0.81, -0.36]. Again, the Boserupian intensification hypothesis finds strong support in these data. But these point estimates are fairly consistently statistically significantly smaller in magnitude than those reported in Table 6, when we imposed the assumption that measurement error was either entirely misreporting or exclusively misperceptions. This can be seen in the hypothesis test results reported at the bottom of each country-specific panel in Table 7. Because measurement error is negatively correlated with true plot size and positively correlated with farmer self-reported plot size in these data, the conventional regression specifications suffer omitted variable bias that downwardly biases both types of estimates, exaggerating the magnitude of the farmer input intensification as plot size changes. The results in Table 7 are precisely what the theory in section 2 predicts.

What about the misperceptions hypothesis? The parameter estimates in Table 7 consistently indicate that plot size measurement error is positively and statistically significantly associated with input intensity (i.e., $\widehat{\varphi} > 0$), conditional on GPS-measured plot size. Consistent with this, the null hypothesis $(\widehat{\gamma} - \widehat{\beta^*}) = 0$ is rejected at the one percent level for almost all inputs and countries. Thus, the misperceptions hypothesis finds strong support in all four countries, and for most inputs. The fact that farmers who over-estimate plot size apply higher levels of inputs per acre suggests that the discrepancy between self-reported and GPS- measured plot size arises, at least in part, from farmer misperceptions rather than just from survey reporting error.[16]

The input-country variation in $\widehat{\theta}$ estimates, and the precision of the $\widehat{\theta}$ estimate, are a function of the extent of use of the input. Bootstrapped confidence intervals censored at the 0 and/or 1 bounds may reflect sub-samples for which omitted variable bias is compounded by the non-linear nature of our estimation, or for which our structural assumptions do not fully hold. Additionally, tighter confidence bounds in Ethiopia and Malawi may reflect not only greater sample sizes but also the fact that analysis is conducted at the plot level, rather than the parcel level. If the intensification decision is at the crop-specific, plot level, aggregating data across plots within a land parcel will almost surely lead to less precisely estimated parameters.

[16]This result also holds when we break measurement error into over-estimates and under-estimates, allowing for prospective asymmetric behavior depending on the sign of measurement error (appendix Table A4). The misperception effect is reasonably symmetric. Overestimation (underestimation) of plot size leads to higher (lower) input intensity. For most country-input combinations, one cannot reject the null of equal coefficients on under- and over-estimation.

Table (7) Input intensity as a function of true plot size and measurement error

	(1) Labor	(2) Seeds	(3) Fertilizer	(4) Pesticides
Ethiopia				
ln (GPS plot size): $\widehat{\gamma}$	-0.782***	-0.422***	-0.388***	
	(0.005)	(0.012)	(0.017)	
Measurement error: $\widehat{\varphi}$	0.057***	0.203***	0.224***	
	(0.008)	(0.019)	(0.024)	
R^2	*0.779*	*0.432*	*0.258*	
$\widehat{\gamma}-\widehat{\beta}^{*}$	0.018***	0.051***	0.056***	
$\widehat{\gamma}-\widehat{\beta}^{o}$	0.023***	0.048***	0.087***	
$\widehat{\theta}$	0.261	0.352	0.367	
95% CI	[0.293, 0.545]	[0.237, 0.424]	[0.308, 0.426]	
Malawi				
ln (GPS plot size): $\widehat{\gamma}$	-0.493***	-0.466***	-0.495***	-0.543***
	(0.010)	(0.062)	(0.019)	(0.092)
Measurement error: $\widehat{\varphi}$	0.263***	0.193*	0.239***	0.150*
	(0.015)	(0.099)	(0.023)	(0.079)
R^2	*0.539*	*0.436*	*0.444*	*0.536*
$\widehat{\gamma}-\widehat{\beta}^{*}$	0.130***	0.096***	0.119***	0.043***
$\widehat{\gamma}-\widehat{\beta}^{o}$	0.054***	-0.002	0.069***	0.064**
$\widehat{\theta}$	0.518	0.361	0.473	0.327
95% CI	[0.609, 1]	[0, 0.778]	[0, 1]	[0, 0.685]
Tanzania				
ln (GPS plot size): $\widehat{\gamma}$	-0.499***	-0.581***	-0.361***	-0.664***
	(0.0126)	(0.0441)	(0.0499)	(0.0640)
Measurement error: $\widehat{\varphi}$	0.237***	0.163**	0.283***	0.299***
	(0.0203)	(0.0642)	(0.0741)	(0.0892)
R^2	*0.505*	*0.483*	*0.338*	*0.521*
$\widehat{\gamma}-\widehat{\beta}^{*}$	0.094***	0.054***	0.135***	0.126***
$\widehat{\gamma}-\widehat{\beta}^{o}$	0.044***	0.023***	0.078***	0.007*
$\widehat{\theta}$	0.473	0.390	0.443	0.889
95% CI	[0.264, 0.779]	[0, 0.993]	[0, 0.764]	[0.438, 1]
Uganda				
ln (GPS plot size): $\widehat{\gamma}$	-0.687***	-0.721***	-0.427**	-0.611***
	(0.0163)	(0.0895)	(0.203)	(0.0836)
Measurement error: $\widehat{\varphi}$	0.111***	0.135	0.0261	0.294***
	(0.0177)	(0.0964)	(0.224)	(0.0985)
R^2	*0.394*	*0.319*	*0.598*	*0.322*
$\widehat{\gamma}-\widehat{\beta}^{*}$	0.047***	0.063***	0.006***	0.095***
$\widehat{\gamma}-\widehat{\beta}^{o}$	0.083***	0.067***	0.153***	0.031***
$\widehat{\theta}$	0.354	0.485	0.046	0.754
95% CI	[0, 0.906]	[0, 1]	[0,1]	[0.041,1]

Notes: $\widehat{\gamma}$ and $\widehat{\varphi}$ are estimated by OLS of equation 22. Standard errors are clustered at the household level. Significance of $\widehat{\gamma}-\widehat{\beta}^{*}$ and $\widehat{\gamma}-\widehat{\beta}^{o}$ are obtained via SUR with shared fixed effects (Blackwell, 2005). $\widehat{\theta}$ is estimated via equation 20 and confidence intervals are bootstrapped, then values censored at the 0 and 1 lower and upper bounds, respectively. All regressions include household, year and crop fixed effects, plot and household controls. For all rows, * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Estimated values of $\widehat{\theta}$, and the bootstrapped confidence intervals around them, suggest that measurement error reflects both farmer misreporting and farmer misperceptions, with the precise mixture varying by input-country combination. All of the $\widehat{\theta}$ point estimates lie between 0 and 1. For 4 of 15 estimates, including all of the Ethiopia estimates, the 95 percent confidence interval encompasses neither 0 nor 1, strongly suggesting hybrid measurement error in those cases. And in 7 of 15 regressions, the 95 percent confidence interval around the $\widehat{\theta}$ estimate lies strictly above 0, providing strong evidence rejecting the maintained hypothesis underpinning conventional measurement error corrections, that measurement error exclusively reflects misreporting. But the pure misperceptions hypothesis likewise finds at best weak support, and in only a few cases. More broadly, there is no general pattern across all input-country combinations. The takeaway empirical finding is thus that measurement error arises from either or both mechanisms, but one cannot safely assume either misreporting or misperceptions dominate, much less fully explain measurement error. On the whole, the $\widehat{\theta}$ parameter estimates and the confidence intervals around them suggest that measurement error reflects both misreporting and misperceptions. This underscores the importance of this more general specification to unbiased estimation of the intensification parameter of interest in the presence of measurement error.

It remains possible that the intensification parameter estimate remains biased by measurement error in input application levels that we cannot check in these data. If input application is measured with error, and that error is correlated with the error in self-reported plot size, then this could spuriously drive the misperception effect evident in Table 7. Abay et al. (2018) show that the correlation between measurement errors in two variables can bias – even aggravate bias in – parameter estimates when one corrects for measurement error in only one variable. Omitted variable bias could also drive these results: if v is not conditionally exogenous to w in equation 18, we cannot identify φ.

One way to address both concerns indirectly is to re-estimate equation 19 using only binary indicator variables for input use. This robustness check addresses the concern about correlated measurement error under the quite plausible assumption that farmers are far less likely to misreport *which* inputs they use than to misreport the *amount* of input used. This also addresses any concern about omitted variable bias that might arise if the decision process differs between the intensive and extensive margins of input use. Note, however, that we do not expect an inverse relationship between plot size and the extensive margin of input use, since the Boserupian hypothesis regards input intensity at the intensive margin, not discrete input use at the extensive margin. We therefore re-estimate equation 19 at the extensive margin of input use purely as a robustness check to examine the coefficient on plot size measurement error. Rejecting the null hypothesis that plot size measurement error is unrelated to (binary) input use seems a reasonable, indirect way of establishing that our prior findings are unlikely due solely to unobserved, non-classical measurement error in input levels. It also suggests that omitted variable bias is unlikely to drive the effect.

Appendix Table A5 replicates Table 7, now looking at the relationship at the extensive margin only, where measurement error in the binary indicator variable for input use is almost surely negligible. Note that we omit labor since very few cultivated plots employed no labor; we consider just improved seed, fertilizer and pesticide use. As shown in Table A5, we find qualitatively identical results: positive and statistically significant coefficient estimates on the plot size measurement error term in virtually all country-input combinations. That pattern is also reasonably symmetric across both over- and under-estimation, as shown in appendix Table A6. Thus, it does not appear that measurement error in input application levels, prospectively correlated with measurement error in self-reported plot size, explains our results. More generally, taken as a whole, the consistent story told by alternate specifications of our estimations in Tables A3-A6 suggest that omitted variables bias is unlikely to drive our estimates, though we cannot rule the possibility out entirely. We interpret the consistency of all robustness checks as supporting our core findings in Table 7,that farmers intensify input application on smaller plots and that farmer misperceptions account for part of observed plot size measurement error and of the Boserupian intensification pattern observed throughout the data. Farmers systematically misestimate their plot sizes and allocate inputs in part accordingly to those misperceptions.

6 Conclusion

This paper explores the possibility that survey measurement error may reflect not only misreporting but also or instead respondents' accurate reports of mistaken beliefs they hold and on which they act, i.e., misperceptions. We show how the mechanism generating measurement error in survey data matters to appropriate regression specifications and to inference about behavioral parameters of interest. We identify a regression specification that permits unbiased estimation of the behavioral parameter of interest under far less restrictive assumptions about measurement error mechanisms. We also introduce an estimable parameter that reflects the average share of measurement error attributable to respondent misperceptions rather than to misreporting, and explain how one can recover that weighting parameter and directly test the misperceptions hypothesis in data. These concepts and the method we employ would seem to have broad application for empirical research using self-reported survey data subject to measurement error generated by unknown mechanisms. More accurate, objective measures made possible through improved survey methods and technological advances generate data that are clearly superior for describing objective conditions. But so long as we allow for human actors to hold mistaken beliefs and for those beliefs to affect behaviors of interest, these more accurate, objective measures may not eliminate – indeed, they could aggravate – bias in inference about behavioral parameters of interest. The possibility that behavioral phenomena might underpin at least some component of measurement error should cause us pause before applying traditional econometric corrections or replacing self-reported data with more objective measures.

We apply these techniques to revisit the Boserupian input intensification hypothesis in sub-Saharan African agriculture, considering the implications of widespread measurement error in farmer self-reports of plot size. In particular, we consider the possibility that measurement error might arise not just due to farmer misreporting, but perhaps due as well or instead to farmer misperceptions that lead to input allocation based on erroneous farmer beliefs about true plot size. Given the importance of the intensification hypothesis in the face of rising population densities in rural Africa, and recent findings that apparent allocative inefficiency among African farmers might account for a large share of the region's productivity deficit compared to the rest of the world, it seems important to sort out what measurement errors truly represent and how one should interpret, or correct econometrically, standard hypothesis tests.

We show that measurement error is pervasive in farmer self-reports of plot size in nationally representative longitudinal survey data from Ethiopia, Malawi, Tanzania and Uganda. That measurement error is non-classical, reflecting strong focal-point-bunching and regression to the mean patterns. We also find, using the new test we introduce, that part of that measurement error represents farmer misperceptions, not merely misreporting. The evidence in support of the Boserupian intensification hypothesis – that input intensity is greater on smaller plots than on larger ones – is overwhelming and consistent across inputs and countries. But part of the observed intensification seems to reflect farmers acting on their misperceptions of plot size, amplifying observed input intensification beyond that which would exist if farmers allocated inputs based on fully accurate perceptions of plot sizes. Our results therefore speak to the evolving literature documenting pervasive factor misallocations in SSA agriculture (e.g., Gollin and Udry, 2019; Restuccia and Santaeulalia-Llopis, 2017). If farmer misperceptions of plot size crowd in input use, it may contribute to factor misallocation. This raises a natural hypothesis amenable to experimentation: if farmers are provided with accurate information on plot size, do they update previously-mistaken beliefs and do they adjust input application – and prospectively other – choices?

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Appendix A Full Derivations From Section 2

In this portion of the appendix, we repeat the equations reported in the main text – keeping the same equation numbers for ease of comparison – and provide the algebraic derivations of the results featured in the text. The two main equations of interest are:

$$Y - X = \beta^o X + \varepsilon \tag{1}$$

$$Y - X^* = \beta^* X^* + \epsilon \tag{2}$$

And measurement error follows:

$$X = X^* + v \tag{3}$$

We assume ϵ is orthogonal to X^*, and ε is orthogonal to both X^* and v.

Scenario 1: Misreporting

In the main text, we explain the misreporting scenario as it might arise from a global regression-to-mean or a focal-point-bunching process.[17] We assume:

$$v = \alpha X^* + \psi \tag{4}$$

Where ψ is assumed to be orthogonal to X^* and to ϵ. It follows that:

$$X = X^* + v = X^* + \alpha X^* + \psi = (1+\alpha)X^* + \psi \tag{5}$$

Since the data generating process follows equation 2, we can rearrange equation 1 as:

$$Y - X = Y - X^* - v = (\beta^* X^* + \epsilon) - (\alpha X^* + \psi) = \beta^* (X - v) - \alpha X^* + \epsilon - \psi$$
$$= \beta^* X - \beta^* v - [\alpha X^* + \psi] + \epsilon$$
$$= \beta^* X - \beta^* v - v + \epsilon$$
$$= \beta^* X - (\beta^* + 1)v + \epsilon$$

Therefore, estimating equation 1 will generate the biased coefficient estimate:

$$\widehat{\beta^o} = \frac{Cov([Y-X], X)}{Var(X)} = \frac{Cov([\beta^* X - (\beta^*+1)v + \epsilon], X)}{Var(X)} = \frac{Cov([\beta^* X - (\beta^*+1)v], X)}{Var(X)}$$

$$\Rightarrow \widehat{\beta^o} = \beta^* - (\beta^*+1)\frac{Cov\,(v, X)}{Var\,(X)} \tag{6}$$

And this can be further specified in terms of the data generating process parameters:

$$\Rightarrow \widehat{\beta^o} = \beta^* - (\beta^*+1)\frac{Cov\big(\alpha X^* + \psi, (1+\alpha)X^* + \psi\big)}{Var\big((1+\alpha)X^* + \psi\big)}$$

[17] Focal point bunching reflects local regression-to-the-mean around focal points but is necessarily non-linear and non-monotone so not directly reflected in equation 4.

$$= \beta^* - (\beta^* + 1)\left[\frac{\alpha(1+\alpha)\sigma^2_{X^*}}{(1+\alpha)^2\sigma^2_{X^*} + \sigma^2_\psi} + \frac{\sigma^2_\psi}{(1+\alpha)^2\sigma^2_{X^*} + \sigma^2_\psi}\right]$$

Alternatively, $\widehat{\beta^o}$ can be derived as a function of $Cov\,(v, X^*)$ rather than of $Cov\,(v, X)$:

$$\widehat{\beta^o} = \frac{Cov\big([Y - X], X\big)}{Var\big(X\big)} = \frac{Cov\big([(\beta^* + 1)X^* + \epsilon - (X^* + v)], X^* + v\big)}{Var\big(X^* + v\big)}$$

$$= \frac{Cov\big([\beta^* X^* + \epsilon - v], X^* + v\big)}{Var\big(X^* + v\big)} = \frac{Cov\big([\beta^* X^* - v], X^* + v\big)}{Var\big(X^* + v\big)}$$

$$\Rightarrow \widehat{\beta^o} = \frac{\beta^* \sigma^2_{x^*} + (\beta^* - 1)\,Cov\,(v, X^*) - \sigma^2_v}{\sigma^2_{x^*} + \sigma^2_v + 2Cov\,(v, X^*)} = \beta^* \frac{Cov\,(X^*, X^* + v)}{Var\,(X^* + v)} - \frac{Cov\,(v, X^* + v)}{Var\,(X^* + v)}$$

An alternative mechanism behind misreporting might arise from optimal prediction error (Hyslop and Imbens, 2001). For instance, if seeding density or fertilizer application rate recommendations are common knowledge, then farmers may report the plot size that corresponds to these recommendations, given the exact quantity of seed or fertilizer they know they recently purchased and fully applied to the plot. As we show, this process generates a similar form of non-classical measurement error, and implies a similar form of bias in the intensification parameter of interest. That is, distinct data generating processes driven by pure misreporting result in similar outcomes.

To illustrate this, let us now assume a different data misreporting process, one in which NCME reflects a relationship between measurement error v and input quantity Y:

$$v = \delta\, Y + \omega$$

where ω is assumed uncorrelated with X^* and with ϵ from equation 2. Combining equations 2 and 7 yields $v = \delta\,(\beta^* + 1)\, X^* + \delta\epsilon + \omega$. By defining $\kappa \equiv \delta\,(\beta^* + 1)$ we can define the functional form of measurement error as

$$v = \kappa X^* + \delta\epsilon + \omega$$

If $\kappa < 0$, measurement error again drives regression to the mean, just as occurs if $\alpha < 0$ under scenario 1. Although the precise mechanisms behind the NCME differ, a common reduced form relationship exists. Combining expressions results in

$$X = (1 + \kappa)X^* + \delta\epsilon + \omega$$

These equations are remarkably similar to those from the misreporting case in the main text, despite the difference in the data generating process underlying the measurement error. Measurement error remains a reduced form function of true plot size, but now with a second term that reflects the component of input levels not derived from plot size, ϵ , reflecting that farmer beliefs about plot size are formed based on observations of Y. This leads to an extra term in the variance of self-reported plot size.

Under this form of NCME, OLS estimation of equation 2 (i.e., using GPS-measured plot size), will again result in an unbiased estimate of β^*. Since again the data generating process follows equation 2, we can derive:

$$
\begin{aligned}
&Y - X = Y - X^* - v \\
&= (\beta^* X^* + \epsilon) - (\kappa X^* + \delta\epsilon + \omega) \\
&= (\beta^* - \kappa)X^* + (1 - \delta)\epsilon - \omega \\
&= \beta^* (X - v) - \kappa X^* + \epsilon - \delta\epsilon - \omega \\
&= \beta^* X - \beta^* v - [\kappa X^* + \delta\epsilon + \omega] + \epsilon \\
&= \beta^* X - \beta^* v - v + \epsilon \\
&= \beta^* X - (\beta^* + 1)v + \epsilon
\end{aligned}
$$

Therefore estimating equation 1 using self-reported plot size will yield the biased coefficient estimate

$$
\widehat{\beta^o} = \frac{Cov\big([Y - X], X\big)}{Var(X)} = \frac{Cov\big([\beta^* X - (\beta^* + 1)v + \epsilon], X\big)}{Var(X)} = \frac{Cov\big([\beta^* X - (\beta^* + 1)v], X\big)}{Var(X)}
$$

$$
\Rightarrow \widehat{\beta^o} = \beta^* - (\beta^* + 1)\frac{Cov\,(v, X)}{Var\,(X)}
$$

And this can be further specified in terms of the data generating process parameters:

$$
\Rightarrow \widehat{\beta^o} = \beta^* - (\beta^* + 1)\frac{Cov\big(\kappa X^* + \delta\epsilon + \omega, (1+\kappa)X^* + \delta\epsilon + \omega\big)}{Var\big((1+\kappa)X^* + \psi\big)}
$$

$$
= \beta^* - (\beta^* + 1)\left[\frac{\kappa(1+\kappa)\sigma^2_{X^*}}{(1+\kappa)^2\sigma^2_{X^*} + \sigma^2_\omega} + \frac{\delta^2\sigma^2_\epsilon}{(1+\kappa)^2\sigma^2_{X^*} + \sigma^2_\omega} + \frac{\sigma^2_\omega}{(1+\kappa)^2\sigma^2_{X^*} + \sigma^2_\omega}\right]
$$

The general form of bias here is identical to that of equation 6, though the structural parameters within the brackets differ under this new form of measurement error. Under classical measurement error, where $\delta = \kappa = 0$ the first two terms of the bracketed expression again disappear. The last term again reflects division bias arising through the dependent variable, and remains even under classical measurement error.

Scenario 2: Misperceptions

Now we assume that the data generating process follows equation 1. Combining equations 1 and 3 leads to:

$$
Y - X = \beta^o X + \varepsilon = \beta^o(X^* + v) + \varepsilon = \beta^o X^* + \beta^o v + \varepsilon \tag{7}
$$

From equation 7 we can define $Y = \beta^o X^* + \beta^o v + \varepsilon + X$. The outcome variable in equation 2 can therefore be reformulated as:

$$
\begin{aligned}
&Y - X^* = [\beta^o X^* + \beta^o v + \varepsilon + X] - X^* \\
&= [\beta^o X^* + \beta^o v + \varepsilon + (X^* + v)] - X^*
\end{aligned}
$$

$$= \beta^o X^* + (\beta^o + 1)v + \varepsilon$$

Estimating equation 2 therefore yields:

$$\widehat{\beta^*} = \frac{Cov\big([Y - X^*], X^*\big)}{Var(X^*)} = \frac{Cov\big([\beta^o X^* + (\beta^o + 1)v + \varepsilon], X^*\big)}{Var(X^*)}$$

$$\Rightarrow \widehat{\beta^*} = \beta^o + (\beta^o + 1)\, \frac{Cov\,(v, X^*)}{Var\,(X^*)} \tag{8}$$

Equation 7 can also be recast so as not to use the self-reported plot size data, to yield:

$$Y - X = \beta^o X^* + \beta^o v + \varepsilon$$

$$\Rightarrow Y - (X^* + v) = \beta^o (X^* + v) + \varepsilon$$

$$\Rightarrow Y - X^* = \beta^o X^* + (\beta^o + 1)v + \varepsilon \tag{9}$$

Scenario 3: Misreporting and Misperception

In this scenario $\widetilde{X}$ is a weighted combination of X and X^* whereby $0 \leq \theta \leq 1$:

$$\widetilde{X} = \theta X + (1 - \theta)\, X^* = \theta v + X^* \tag{10}$$

Therefore a new data-generating process is found:

$$Y - \widetilde{X} = \beta^{\triangle} \widetilde{X} + u \tag{11}$$

where we assume u is orthogonal to both X^* and v. Equation 10 implies $X^* = \widetilde{X} - \theta v$, and equation 11 implies that $Y = (\beta^{\triangle} + 1)\widetilde{X} + u$. By combining equations 3 and 10 we find that:

$$\widetilde{X} = \theta v + X^* = \theta v + (X - v) = (\theta - 1)\, v + X = X - (1 - \theta)\, v$$

or alternatively, $X = \widetilde{X} + (1 - \theta)\, v$. Therefore, equation 1 can be rearranged to take the form

$$Y - X = \Big[\big(\beta^{\triangle} + 1\big)\, \widetilde{X} + u\Big] - \Big[\widetilde{X} + (1 - \theta)\, v\Big]$$

$$= \beta^{\triangle} \widetilde{X} - (1 - \theta)\, v + u$$

$$= \beta^{\triangle}\, (X - (1 - \theta)\, v) - (1 - \theta)\, v + u$$

$$= \beta^{\triangle} X - (\beta^{\triangle} + 1)\, (1 - \theta)\, v + u$$

And so estimating equation 1 will lead to:

$$\widehat{\beta^o} = \frac{Cov([Y - X], X)}{Var(X)} = \frac{Cov([\beta^{\triangle} X - \big(\beta^{\triangle} + 1\big)\, (1 - \theta)\, v + u], X)}{Var(X)}$$

$$\Rightarrow \widehat{\beta^o} = \beta^{\triangle} - \big(\beta^{\triangle} + 1\big)\, (1 - \theta)\, \frac{Cov\,(v, X)}{Var\,(X)} \tag{12}$$

Estimating equation 2 will therefore lead to:

$$\widehat{\beta^*} = \frac{Cov\big([Y - X^*], X^*\big)}{Var(X^*)} = \frac{Cov\big([\beta^{\triangle} X^* + (\beta^{\triangle} + 1)\theta v + u], X^*\big)}{Var(X^*)}$$

$$= \frac{Cov\big([\beta^{\triangle} X^* + (\beta^{\triangle} + 1)\theta v], X^*\big)}{Var(X^*)}$$

$$\Rightarrow \widehat{\beta^*} = \beta^{\triangle} + (\beta^{\triangle} + 1)\theta \frac{Cov\,(v, X^*)}{Var\,(X^*)} \tag{13}$$

Equation 2 may also be manipulated to show:

$$Y - X^* = [(\beta^{\triangle} + 1)\widetilde{X} + u] - [\widetilde{X} - \theta v]$$

$$= \beta^{\triangle}\widetilde{X} + u + \theta v$$

$$= \beta^{\triangle}\,(\theta v + X^*) + u + \theta v$$

$$= \beta^{\triangle} X^* + (\beta^{\triangle} + 1)\theta v + u \tag{14}$$

Appendix B Additional Results

Table (A1) Parcel-level Attrition Due to Missing GPS Measurement

	Retained	Dropped	T-stat
Ethiopia			
Head is male (binary)	0.847	0.872	-0.025***
Age of household head (years)	47.354	45.348	2.006***
Head is literate (binary)	0.422	0.452	-0.030**
Household size (# members)	5.652	5.807	-0.156***
Acres per person (log acres/members)	-0.921	-1.086	0.165***
Self-reported parcel size (log acres)	-1.759	-1.53	-0.228***
Pure stand cropping (binary)	0.622	0.789	-0.167***
Irrigated plot (binary)	0.043	0.028	0.014***
Soil quality perceived as good (binary)	0.312	0.167	0.145***
Steep slope (binary)	0.115	0.077	0.038***
Plot is owned (binary)	0.873	0.735	0.137***
Number of crops grown (# crops)	1.517	1.158	0.360***
Plot grows barley	0.052	0.056	-0.004
Plot grows maize	0.135	0.132	0.003
Plot grows sorghum	0.115	0.102	0.013*
Plot grows teff	0.098	0.115	-0.017**
Plot grows wheat	0.061	0.078	-0.017***
Plot grows horse beans	0.028	0.042	-0.014***
Plot grows chat	0.04	0.013	0.027***
Plot grows coffee	0.066	0.045	0.021***
Plot grows enset	0.034	0.018	0.016***
Malawi			
Head is male (binary)	0.739	0.783	-0.044***
Age of household head (years)	44.179	40.144	4.034***
Head is literate (binary)	0.13	0.29	-0.160***
Household size (# members)	4.811	4.311	0.500***
Acres per person (log acres/members)	-0.517	-0.658	0.141***
Self-reported parcel size (log acres)	-0.249	-0.123	-0.126***
Pure stand cropping (binary)	0.598	0.813	-0.215***
Irrigated plot (binary)	0.006	0.145	-0.139***
Soil quality perceived as good (binary)	0.477	0.507	-0.030***
Steep slope (binary)	0.108	0.137	-0.029***
Plot is owned (binary)	0.851	0.699	0.152***
Number of crops grown (# crops)	1.978	1.192	0.786***
Plot grows maize (local)	0.209	0.049	0.160***
Plot grows maize (hybrid)	0.191	0.071	0.120***
Plot grows maize (hybrid recycled)	0.038	0.015	0.022***
Plot grows groundnuts	0.045	0.011	0.034***
Plot grows sorghum	0.038	0.005	0.033***
Plot grows beans	0.048	0.02	0.028***
Plot grows soya beans	0.031	0.012	0.020***
Plot grows pigeon pea	0.121	0.018	0.103***
Plot grows nkhwani	0.09	0.028	0.061***

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Parcel-level Attrition Due to Missing GPS Measurement

	Retained	Dropped	T-stat
Tanzania			
Head is male (binary)	0.77	0.79	-2.59***
Age of household head (years)	49.18	47.95	5.86***
Head is literate (binary)	0.70	0.73	-3.90***
Household size (# members)	5.95	5.77	4.17***
Acres per person (log acres/members)	-0.22	-0.34	7.55***
Self-reported parcel size (log acres)	0.27	0.25	1.58
Pure stand cropping (binary)	0.47	0.54	-8.82***
Irrigated plot (binary)	0.02	0.02	0.20
Soil quality perceived as good (binary)	0.40	0.39	2.02**
Steep slope (binary)	0.03	0.03	-0.73
Plot is owned (binary)	0.86	0.75	22.31***
Number of crops grown (# crops)	1.69	1.51	12.59***
Plot grows beans	0.19	0.15	6.27***
Plot grows maize	0.67	0.52	17.81***
Plot grows sunflower	0.05	0.04	3.18***
Plot grows sorghum	0.08	0.07	1.46
Plot grows cowpeas	0.05	0.04	3.47***
Plot grows millet	0.02	0.02	1.01
Plot grows groundnuts	0.11	0.08	4.82***
Plot grows rice	0.12	0.22	-15.95***
Plot grows simsim	0.02	0.03	-2.63***
Plot grows sweet potato	0.08	0.06	6.17***
Plot grows pigeon peas	0.06	0.04	3.86***
Plot grows cotton	0.04	0.02	4.17***
Plot grows bambara nuts	0.02	0.02	-0.12
Uganda			
Head is male (binary)	0.72	0.72	-0.55
Age of household head (years)	47.20	45.73	10.25***
Head is literate (binary)	0.65	0.68	-6.33***
Household size (# members)	7.02	7.21	-5.91***
Acres per person (log acres/members)	-1.04	-1.07	2.91***
Self-reported parcel size (log acres)	-0.03	-0.04	1.33
Pure stand cropping (binary)	0.44	0.58	-24.38***
Irrigated plot (binary)	0.02	0.01	5.97***
Soil quality perceived as good (binary)	0.60	0.65	-9.54***
Steep slope (binary)	0.11	0.13	-8.13***
Plot is owned (binary)	0.89	0.64	59.91***
Number of crops grown (# crops)	2.48	1.93	31.82***
Plot grows maize	0.33	0.30	5.86***
Plot grows millet	0.06	0.06	0.33
Plot grows sorghum	0.08	0.07	3.76***
Plot grows beans	0.36	0.33	6.78***
Plot grows groundnuts	0.10	0.10	-0.35
Plot grows sweet potato	0.22	0.18	9.65***
Plot grows cassava	0.36	0.32	9.00***
Plot grows matooke	0.36	0.19	36.48***
Plot grows beer banana	0.06	0.02	18.19***
Plot grows sweet banana	0.03	0.02	10.44***
Plot grows coffee	0.14	0.06	27.09***
Plot left fallow	0.04	0.02	7.67***

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table (A2) Measurement error relationships with GPS-measured and self-reported plot size, unconditional regressions

	Ethiopia		Malawi		Tanzania		Uganda	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln (GPS plot size):	-0.293***		-0.465***		-0.334***		-0.299***	
	(0.010)		(0.009)		(0.00705)		(0.0121)	
ln (SR plot size):		0.104***		0.170***		-0.0293***		0.0603***
		(0.008)		(0.014)		(0.00849)		(0.0103)
Observations	36304	36304	53475	53475	13847	13847	19738	19738
R^2	0.229	0.023	0.377	0.032	0.354	0.00176	0.260	0.00788

Notes: Standard errors, clustered at the household level, are given in parentheses. No controls or fixed effects are included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table (A3) Measurement error and input use intensity, including non-linear terms

	(1) Labor	(2) Seeds	(3) Fertilizer	(4) Pesticides
Ethiopia				
ln (GPS plot size)	-0.827***	-0.280***	-0.306***	
	(0.008)	(0.017)	(0.032)	
ln (GPS plot size)^2	-0.010***	0.040***	0.027***	
	(0.002)	(0.004)	(0.009)	
Measurement error	**0.069*** **	**0.150*** **	**0.211*** **	
	(0.008)	(0.018)	(0.024)	
Observations	32750	12927	9430	
R^2	0.780	0.442	0.261	
Malawi				
ln (GPS plot size)	-0.511***	-0.527***	-0.537***	-0.418***
	(0.012)	(0.064)	(0.020)	(0.124)
ln (GPS plot size)^2	-0.014***	-0.069***	-0.033***	0.090
	(0.005)	(0.025)	(0.009)	(0.070)
Measurement error	**0.268*** **	**0.250** **	**0.247*** **	**0.162** **
	(0.015)	(0.103)	(0.023)	(0.078)
Observations	48791	4970	32795	1117
R^2	0.540	0.440	0.447	0.547
Tanzania				
ln (GPS plot size)	-0.488***	-0.577***	-0.363***	-0.664***
	(0.0124)	(0.0447)	(0.0498)	(0.0638)
ln (GPS plot size)^2	-0.0297***	-0.00857	-0.0689***	0.00487
	(0.00447)	(0.0142)	(0.0160)	(0.0165)
Measurement error	**0.266*** **	**0.172*** **	**0.344*** **	**0.300*** **
	(0.0194)	(0.0660)	(0.0762)	(0.0875)
Observations	9175	1487	1305	949
R^2	0.511	0.484	0.368	0.521
Uganda				
ln (GPS plot size)	-0.699***	-0.730***	-0.450**	-0.604***
	(0.0160)	(0.0909)	(0.203)	(0.0835)
ln (GPS plot size)^2	-0.0193***	0.0269	0.206	-0.0186
	(0.00479)	(0.0308)	(0.135)	(0.0334)
Measurement error	**0.113*** **	**0.131**	**-0.187**	**0.305*** **
	(0.0173)	(0.0955)	(0.258)	(0.0973)
Observations	16164	1335	241	906
R^2	0.395	0.320	0.614	0.322

Notes: This table provides relationships between alternative indicators of measurement error and input use intensity. We construct relative overestimations and underestimations in self-reported plot sizes, for those over and under-estimated plot sizes. All regressions include household and year fixed effects, plot and household controls, and plot dummies. For all rows, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table (A4) Input intensity and measurement error decomposed into over-/under-estimate

	(1) Labor	(2) Seeds	(3) Fertilizer	(4) Pesticides
Ethiopia				
ln (GPS-measured plot size)	-0.788***	-0.410***	-0.382***	
	(0.005)	(0.012)	(0.017)	
Overestimation (% area)	**0.023****	**0.267*****	**0.278*****	
	(0.011)	(0.029)	(0.043)	
Underestimation (% area)	**-0.113*****	**-0.123*****	**-0.176*****	
	(0.012)	(0.030)	(0.032)	
Observations	32734	12923	9430	
R^2	0.779	0.433	0.258	
Malawi				
ln (GPS-measured plot size)	-0.489***	-0.501***	-0.498***	-0.520***
	(0.011)	(0.062)	(0.019)	(0.118)
Overestimation (% area)	**0.269*****	**0.050**	**0.241*****	**0.237**
	(0.019)	(0.129)	(0.031)	(0.243)
Underestimation (% area)	**-0.250*****	**-0.334****	**-0.225*****	**-0.075**
	(0.024)	(0.157)	(0.040)	(0.142)
Observations	46570	4812	31317	1076
R^2	0.539	0.446	0.451	0.536
Tanzania				
ln (GPS-measured plot size)	-0.510***	-0.599***	-0.373***	-0.677***
	(0.0132)	(0.0468)	(0.0520)	(0.0683)
Overestimation (% area)	**0.200*****	**0.0105**	**0.161**	**0.310****
	(0.0289)	(0.0922)	(0.103)	(0.146)
Underestimation (% area)	**-0.270*****	**-0.290*****	**-0.401*****	**-0.244****
	(0.0301)	(0.103)	(0.106)	(0.110)
Observations	8937	1458	1265	930
R^2	0.509	0.491	0.345	0.536
Uganda				
ln (GPS-measured plot size)	-0.692***	-0.699***	-0.357*	-0.580***
	(0.0167)	(0.0924)	(0.197)	(0.0848)
Overestimation (% area)	**0.0727*****	**0.345****	**0.705**	**0.266**
	(0.0270)	(0.158)	(0.683)	(0.173)
Underestimation (% area)	**-0.151*****	**0.00883**	**0.297**	**-0.427*****
	(0.0264)	(0.165)	(0.387)	(0.147)
Observations	15760	1302	227	874
R^2	0.397	0.323	0.616	0.335

Notes: This table provides relationships between alternative indicators of measurement error and input use intensity. We construct relative overestimations and underestimations in self-reported plot sizes, for those over and under-estimated plot sizes. All regressions include household and year fixed effects, plot and household controls, and plot dummies. For all rows, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table (A5) Measurement error and the extensive margin of input use

	(1) Improved seed (dummy)	(2) Fertilizer (dummy)	(3) Pesticide (dummy)
Ethiopia			
ln (GPS-measured plot size)	0.044***	0.053***	0.021***
	(0.002)	(0.002)	(0.001)
Measurement error	**0.008****	**0.019*****	**0.005****
	(0.004)	(0.003)	(0.002)
Observations	35397	35483	35483
R^2	0.582	0.201	0.170
Malawi			
ln (GPS-measured plot size)	-0.022	0.188***	0.015***
	(0.017)	(0.006)	(0.002)
Measurement error	**-0.031**	**0.092*****	**0.006****
	(0.023)	(0.008)	(0.003)
Observations	10370	53416	53416
R^2	0.401	0.267	0.034
Tanzania			
ln (GPS-measured plot size)	0.0162***	0.0236***	0.0252***
	(0.00565)	(0.00491)	(0.00460)
Measurement error	**0.0146***	**0.00980**	**0.0129****
	(0.00756)	(0.00693)	(0.00561)
Observations	7987	9197	9197
R^2	0.0974	0.0354	0.0943
Uganda			
ln (GPS-measured plot size)	0.0117***	0.00485**	0.0124***
	(0.00416)	(0.00204)	(0.00354)
Measurement error	**0.00889****	**0.00226**	**0.00730****
	(0.00429)	(0.00195)	(0.00367)
Observations	16582	16571	16571
R^2	0.0479	0.0131	0.0349

Notes: This table provides relationships between measurement error and the extensive margin of input use. We rely on binary (indicator) variables showing whether a farmer has applied a specific input in a specific plot. All regressions include household and year fixed effects, plot and household controls, and plot dummies. For all rows, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table (A6) Measurement error by direction and the extensive margin of input use

	(1) Improved seed (dummy)	(2) Fertilizer (dummy)	(3) Pesticide (dummy)
Ethiopia			
ln (GPS-measured plot size)	0.042***	0.055***	0.021***
	(0.002)	(0.002)	(0.001)
Overestimation (% area)	**-0.001**	**0.030*** **	**0.006** **
	(0.006)	(0.004)	(0.003)
Underestimation (% area)	**-0.024*** **	**-0.002**	**-0.004**
	(0.006)	(0.006)	(0.004)
Observations	35379	35465	35465
R^2	0.582	0.202	0.170
Malawi			
ln (GPS-measured plot size)	-0.015	0.190***	0.016***
	(0.018)	(0.007)	(0.003)
Overestimation (% area)	**0.015**	**0.093*** **	**0.008* **
	(0.034)	(0.012)	(0.004)
Underestimation (% area)	**0.089** **	**-0.087*** **	**-0.005**
	(0.036)	(0.014)	(0.005)
Observations	9975	51010	51010
R^2	0.399	0.268	0.034
Tanzania			
ln (GPS-measured plot size)	0.0135**	0.0219***	0.0256***
	(0.00584)	(0.00491)	(0.00487)
Overestimation (% area)	**0.0133**	**0.0107**	**0.0120**
	(0.0108)	(0.0101)	(0.00805)
Underestimation (% area)	**-0.0102**	**-0.00323**	**-0.0155* **
	(0.0121)	(0.0110)	(0.00867)
Observations	7778	8959	8959
R^2	0.0973	0.0348	0.0938
Uganda			
ln (GPS-measured plot size)	0.0113***	0.00466**	0.0106***
	(0.00428)	(0.00200)	(0.00351)
Overestimation (% area)	**0.00721**	**-0.00104**	**-0.00442**
	(0.00641)	(0.00242)	(0.00551)
Underestimation (% area)	**-0.00901**	**-0.00513**	**-0.0166*** **
	(0.00658)	(0.00367)	(0.00524)
Observations	16175	16164	16164
R^2	0.0480	0.0134	0.0352

Notes: This table provides relationships between alternative indicators of measurement error (overestimations and underestimations) and extensive margin of input use decisions. All regressions include household and year fixed effects, plot and household controls, and plot dummies. For all rows, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure (A1) Linearity of the Relationship Between Measurement Error and Log GPS Plot Size