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Inverse productivity: land quality, labor markets, and measurement error

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Abstract

This paper examines the role that land quality and imperfect markets play in generating the inverse productivity relationship in the International Crop Research Institute for the Semi Arid Tropics (ICRISAT) data. Differences in land quality largely explain the ‘‘Inverse Productivity’’ (IP) relationship in the random effects profit regression, but not in labor demand regressions. Controlling for labor and land market failures and differences in soil quality eliminates the IP relationship for male labor, but not female labor in the random effects estimates. The inverse relationship is much stronger in fixed effects than random effects estimates, suggesting that the farm size variable may be subject to measurement error, a view supported by the results of instrumental variables estimation.

JEL classification: O13; J43; Q12
Keywords: Inverse productivity; Land quality; Labor markets; Measurement error; India

1. Introduction to the problem

A recurring puzzle in empirical work on developing country agriculture is the ‘‘Inverse Productivity’’ (IP) relationship,[1] which is summarized in the following straightforward empirical model. In the basic IP relationship, the dependent variable, Y_i, is either output or

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[1] I follow the convention of referring to output per acre of land farmed as productivity, although it clearly ignores differences in the intensity of input use, especially labor use. Indeed, as I discuss below, labor use per acre is an important source of differences in output per acre across farmers, and is used as another way of defining and explaining the inverse productivity relationship.

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profits. Let X_i denote a vector of control variables, and let the total area farmed be denoted by A_i. A reduced-form empirical relationship to be estimated is given by:[2]

$$\ln Y_i = \alpha + X_i\beta + \gamma \ln A_i + u_i \tag{1}$$

where α, β, and γ are parameters to be estimated, and u_i is a random error term. Since γ measures the elasticity of output or profits with respect to area, estimates of γ less than 1 suggest that total output rises less quickly than total area farmed, and the inverse productivity hypothesis holds. The IP relationship is also observed in labor demand relations in many developing countries, e.g. where Y_i is household labor demand, and labor demand rises less quickly than area farmed. Benjamin (1995) verifies that the IP relationship exists for both output and labor demand for rice farmers in Java, and Barrett (1996) verifies the same phenomenon for Madagascar. Numerous other studies have identified the inverse productivity in other countries.[3]

A simple and direct explanation of the inverse relationship could lie in the production function. If small farmers are technically more efficient than large farmers, this would generate the observed relationship. However, Carter (1984) finds that smaller farmers in India would produce 15% less output than larger farmers given the same inputs (Carter, 1984, p. 141) and numerous studies have shown that it is impossible to reject constant returns to scale for agricultural production in both the Indian context and elsewhere.[4] In fact, increasing returns to scale seems intuitively more likely for the small farms characteristic of much of the developing world.

Two alternative explanations of the IP relationship have been suggested in the literature on developing country agriculture, with important implications for policy. Sen (1975), and more recently Benjamin (1995), have suggested that the IP relationship may be traced to unobserved differences in land quality which are not adequately controlled for in regression analysis. If the IP relationship arises because small farmers on average farm land of higher quality, then public policies designed to redistribute land to small landholders will not raise (and may lower) agricultural output and rural incomes. Alternatively, market failures, especially labor-market failures, are often argued to be a primary cause of the IP relationship in labor: Farmers who cannot sell their labor in the wage–labor market apply it to their own fields. If imperfect labor markets are responsible, then a policy of land redistribution will improve efficiency, raise agricultural output and lower inequality. Of course, the two explanations are not mutually exclusive. It is possible that the distribution of land quality across households arises as a response to labor market imperfections.

This paper tests competing explanations of the inverse relationship using the widely used and important ICRISAT data set on the semi-arid tropics of India. Given the widespread use of ICRISAT data, explaining the source of the IP relationship should be of wide interest. Moreover, the ICRISAT data offer a number of advantages over previous

[2] No structural relationship is implied in the estimation, although the connection to Cobb-Douglas is clear. In practice, applied studies often begin with the reduced form suggested here without relating those results back to a structural economic model.

[3] See Berry and Cline (1979) for a summary of previous research through the mid-1970s.

[4] A number of these studies are summarized in Berry and Cline. Carter (1984) also fails to reject constant returns to scale in India.

studies in testing competing explanations of the IP relationship. In particular, a rich set of variables measuring land quality allow for better tests of the omitted land quality hypothesis, and data on labor market activities permit more careful testing of the imperfect labor market hypothesis. The panel nature of the data allows estimation of both random and fixed effects, which proves useful in looking at the role of measurement error.

The structure of the paper is as follows. In Section 2, I consider agricultural production in the semi-arid tropics, discuss the severity of the IP relationship, and suggest that the empirical results are consistent with measurement error in the farm size variable. I then go on to test competing explanations, in light the possibility of measurement error using a straightforward approach. To test the role of omitted land quality, I add measures of household level land quality to the profit and labor demand regressions to see how much of the inverse relationship is explained away (Section 3). To test the role of imperfect labor markets, I include village level measures of labor and land market characteristics to the regressions (Section 4). In Section 5, I test the possibility that measurement error in the farm size variable may contribute to the estimated IP relationship, exacerbated in practice by fixed effects estimation. Section 6 concludes.

2. Inverse productivity in the semi-arid tropics of India

I examine the IP relationship in Indian agriculture using data collected by the International Crop Research Institute for the Semi-Arid Tropics (ICRISAT) on rural households in three distinct agro-climatic regions of India between 1975 and 1985. Data are available from three of the study villages for all 10 years, from three villages for 4 years, and from two other villages for 4 years, for a total of eight villages.[5] Table 1 presents sample means for the variables used in the paper, based on a sample of 1060 households drawn from across the eight villages.

Households were surveyed and the quantity of inputs to, and outputs from, crop production at 2-week intervals throughout the year were recorded. Agricultural production in the semi-arid tropics is characterized by two main growing seasons. The rainy (kharif) season begins with the onset of the monsoon when soils are water-rich and germination is easy. The post-rainy (rabi) season, which is less important in overall agriculture, begins after the monsoon, drawing on moisture stored in the soil after rainy season crops have been grown. Weather is a major source of the uncertainty surrounding the household's production environment and crop yields are highly susceptible to variations in the timing and duration of the monsoon.

Agriculture in the semi-arid tropics of India is characterized by a great deal of heterogeneity in production. The crop mix varies distinctly from village to village and across soil types, reflecting in part differing agro-climatic conditions. This requires more care in measuring "output" than in the case of monoculture. I choose revenues net of the costs of hired and family labor and variable inputs as the measure of output. Family labor is valued at the prevailing village-level wage and all other inputs are evaluated at village

[5] I did not include data from the final two villages—Rampura and Rampura Kalan—because these data are generally deemed to be less reliable than other villages.

Table 1
Sample means

Log total cropped area	2.18 acres
Log real household profits	7.51 rupees
Log hours of male labor	6.50 h
Log hours of female labor	6.54 h
Average value of land	2607 rupees/acre
Share of irrigated land	0.16%
Share, type 1 land	0.09%
Share, type 2 land	0.40%
Share, type 3 land	0.21%
Share, type 5 land	0.19%
Share, type 6 land	0.02%
Real wage, male, period 1	0.80 rupees/h
Real wage, male, period 2	0.83 rupees/h
Real wage, female, period 1	0.46 rupees/h
Real wage, female, period 2	0.51 rupees/h
Real fertilizer price	2.83 rupees/kg
Real price of sorghum	1.70 rupees/kg
Real price of fodder	27.3 rupees/quintal

prices as well. This assumes that farmers are profit maximizing and that the choice of crop mix and the allocation of labor across crops is optimal. One direction for future research would be to reexamine the inverse productivity hypothesis with weaker assumptions on farmer behavior.

I did not include the implicit rental value of farmland in my costs of production. If interest lies in measuring the returns to farming *net of differences in land quality*, then rental cost of land should not be included in profit calculations. To the extent that land quality differences, separate from the differences in managerial ability or effort, lead to higher yields and, ceteris paribus, higher returns per acre to farming, we would expect these differences to be capitalized into land prices. Indeed, I will later use data on the value of land per acre to examine the hypothesis that land-quality differences account for the inverse productivity relationship.

I amended Eq. (1) to take account the panel nature of the ICRISAT data. Allowing for the simplest form of household level heterogeneity, I assume that the constant term in Eq. (2) varies by household:

$$\ln Y_{it} = \alpha_i + X_{it}\beta + \gamma \ln A_{it} + u_{it} \tag{2}$$

for $i = 1 \ldots N$ households. The intercept term α_i is allowed to vary across individuals, but not the slope coefficients β. The proper econometric model for the α_i is an issue of some concern. If the intercept terms α_i are viewed as parametric shifts in the intercept term of the regression line, then treating them as fixed for a given household in the sample is appropriate, giving rise to the fixed effects estimator. If, on the other hand, the sampled cross-sectional observations are viewed as drawn from a larger random population, the α_i become random variables themselves, which must be estimated, giving rise to the random effects model. The choice of fixed effects or random effects is important since using random effects will yield biased coefficient estimates if there are fixed effects that are correlated with variables in Eq. (2). The issue is complicated considerably when any of the right-hand side variables in Eq. (2) are subject to measurement error, as discussed below.

Obviously the measure of total area farmed, A_{it}, is a crucial variable in these regressions and must be constructed with some care in the ICRISAT data to account for the two distinct cropping periods. I calculate total area as the sum of acreage planted in the kharif and rabi seasons, counting twice those acres planted in both seasons, but not adjusting for intercropping within a season on a given plot, following Benjamin (1995). Accounting for both kharif and rabi production should work to ameliorate the inverse relationship where it exists.[6]

Variations in total area farmed are of interest themselves, given its role in the IP phenomenon. Fig. 1 summarizes the sources of variation in total cropped area in the three main study villages by household. Total cropped area (in both the kharif and rabi seasons) is plotted on the vertical axis by household, so that a household is represented by a "column" of scatterplots.[7] As can be seen, there is substantial variation across time within households in total area farmed, and the greatest within-household variation is in Shirapur. In fact, inter-household differences in average area explain only about three-fourths of the total variation in area farmed in Shirapur, with the remaining one-fourth explained by within household differences.[8]

Some of the variation within household may reflect changes in land rented or sharecropped in. There is substantial variation in the measured share of planted acreage that is sharecropped or leased in across villages (Fig. 2) and within households across time as well (Fig. 3). Sharecropping and renting together account for about one-fourth of land farmed in the ICRISAT villages (Shaban, 1987). Part of the variation within households may also reflect the impact of the monsoon on planting decisions. While it may be difficult to add area in light of a strong monsoon, farmers do respond to bad monsoons by fallowing land (Walker and Ryan, 1990, p. 34). Area planted is more likely to respond to rainfall shocks in Aurepalle than the other study villages, since rainfall is more erratic (Walker and Ryan, 1990, p. 36).

The ability to plant during the rabi season represents a margin of adjustment in total cropped area, although this is somewhat constrained by soil type.[9] Sharecropping or leasing might also represent an important margin of adjustment, with farm households sharecropping if weather appears favorable. Coefficient estimates based on Eq. (2) (e.g. that condition on A_{it} ignoring its endogeneity) will be biased, where the bias will

[6] In the Java data, Benjamin found that using total available land, as opposed to total cropped area, resulted in a slightly more severe inverse relationship.

[7] Most of the variation in Aurepalle and Kanzara occurs in area farmed during the kharif season. In contrast, area planted to rabi production varies quite a lot within the household in Shirapur, since rabi production is more important there. Kharif production accounts for only 40% of gross cropped area in Shirapur in an average season (Walker and Ryan, 1990, p. 34).

[8] For both Aurepalle and Kanzara the within-household variation in area is much less important, accounting for only about 10% of the total.

[9] It is worth noting differences in kharif and rabi season production across the villages. Among the three main villages, kharif production is far more important than rabi production in Aurepalle and Kanzara, accounting for 90% of gross cropped area. In contrast, most of the production in Shirapur occurs in the rabi season.

depend on the correlation between planted acreage and the random component u_{it}, but this would seem likely to work against the observed IP relationship. If farmers expand area farmed in response to productivity shocks, then the coefficient on total area farmed

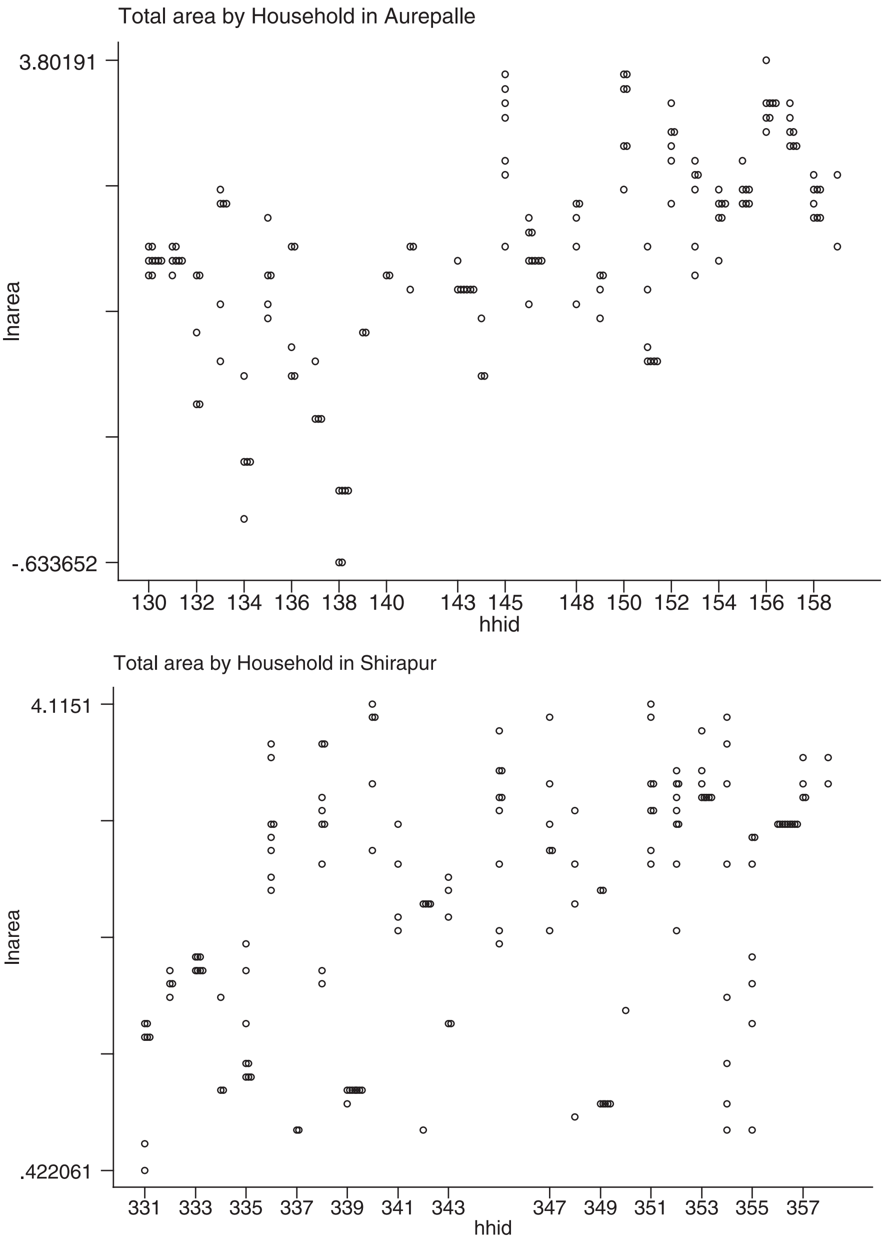

Fig. 1. Variation in total area farmed, by Household.

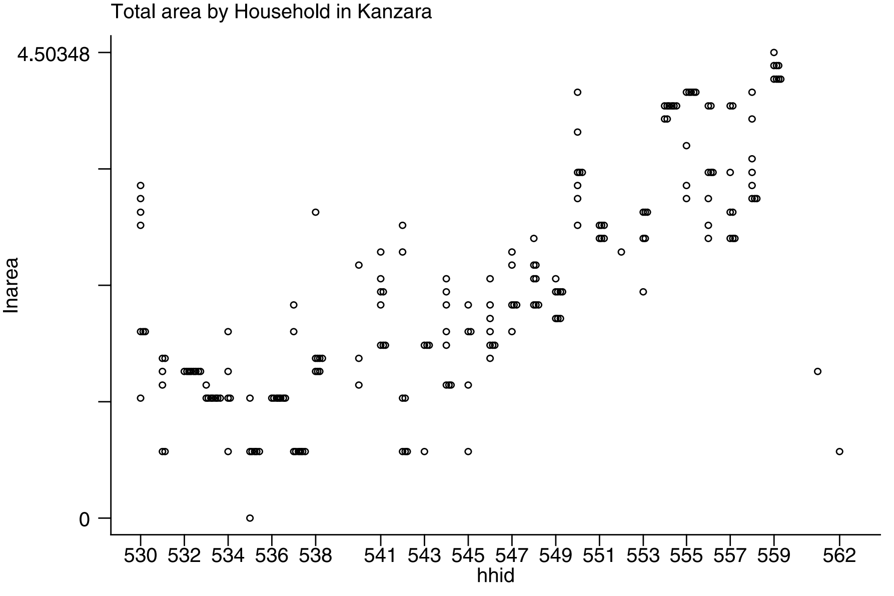

Fig. 1 (*continued*).

from Eq. (2) would be biased upward. For endogeneity to contribute to the inverse productivity result, farmers would have to plant fewer acres in response to a positive productivity shock.

I first estimate a simple profit equation conditional on exogenous weather shocks, real village-level wages, the real price of fertilizer, and the real price of fodder and sorghum, two crops which are found in all villages. Wages are measured for two different periods during the crop production year. Period 1 is the planting period and that part of the crop production cycle before weather uncertainty is resolved. Period 2 is the harvest period after uncertainty about yields has been resolved. All wages are measured as village averages, by gender, converted to real dollars using the village-level consumer price index. The sample includes farmers with total cultivation ranging from about 0.4 to over 100 acres, so there is a wide dispersion of holdings.[10]

Results in column (1) of Table 2 show random effects estimates. The parameter γ is estimated to be 0.89 and is significantly less than 1 at the 1% level.[11] Both the rainfall coefficients and the coefficients on wages and prices are jointly statistically significant at the 1% level. While the rainfall variables are jointly significant at the 1% level, only the date of the monsoon onset is individually significant at the 5% level and negative, suggesting that a delay in the monsoon onset lowers farmer profits. The first period male

[10] Results in Table 2 are from a slightly larger sample of farmers than those in the remainder of the paper. To facilitate use of instrumental variables, some observations were dropped from the estimation. Several regressions were run to compare results between these two samples, and the results did not differ significantly.

[11] Note that the appropriate significance level for the coefficient is in relation to one, and the *t*-statistics reported in the table is for a null-hypothesis that $\gamma = 1$.

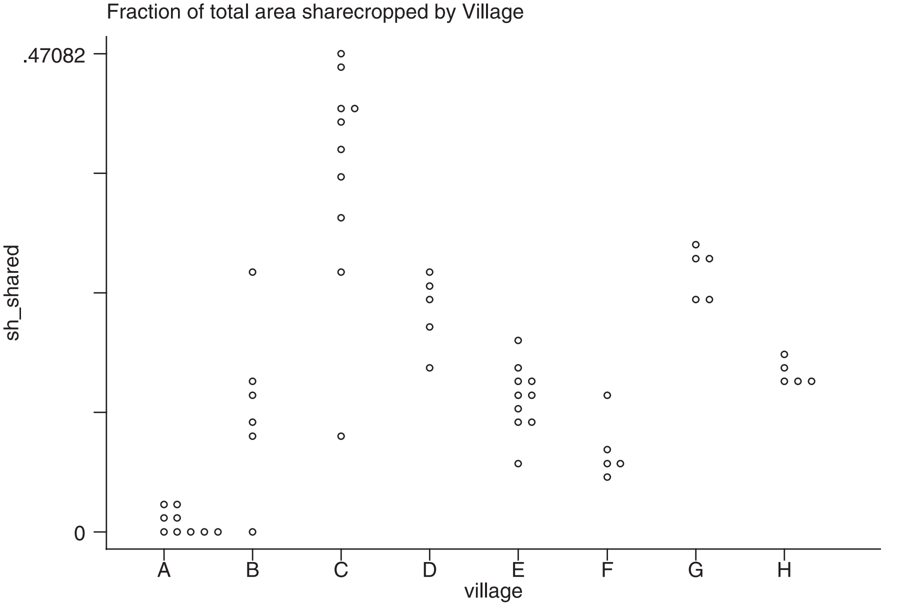

Fig. 2. Fraction of total area sharecropped, by Village.

wage is negative and significant at the 10% level. The coefficient on fertilizer price and second period male wages is estimated to be positive and statistically significant, at odds with economic theory. Other price and wage variables are not significantly different from 0.

I also estimated the model using fixed effects, for comparison. Column 2 of Table 2 shows fixed effects estimates. The coefficient γ is estimated to be 0.62 and is significantly less than 1 at the 1% level. The inverse relationship with profits is far more severe with fixed effects than random effects estimates. Other coefficients are similar to those obtained from random effects estimation. The coefficients on the rainfall shocks are jointly significant at the 5% level, as are the set of coefficients on wages and prices.

Given the substantial difference in the severity of the IP relationship in the fixed effects and random effects models, the role of fixed effects in the profit regression here bears consideration. The argument for using fixed effects hinges on the presence of unmeasured household specific differences are correlated with variables on the right-hand side of Eq. (2). In the case of the profit regressions, farmer productivity would seem to be one such factor. If there is unobserved heterogeneity in farmer productivity, then its impact on the random effects estimates in column (1) would depend on its correlation with the farm size variable. If smaller farmers cultivate land that is on average of higher quality, then the random effects estimate of γ would be biased downward, and controlling for fixed effects should yield an estimate of γ closer to 1 in the profit regression. Likewise, if differences in productivity arise from the effect of labor market imperfections, smaller farmers would be more constrained than larger ones, and controlling for these differences with fixed effects should lead to an estimate of γ closer to 1. The fact that the coefficient on farm size is

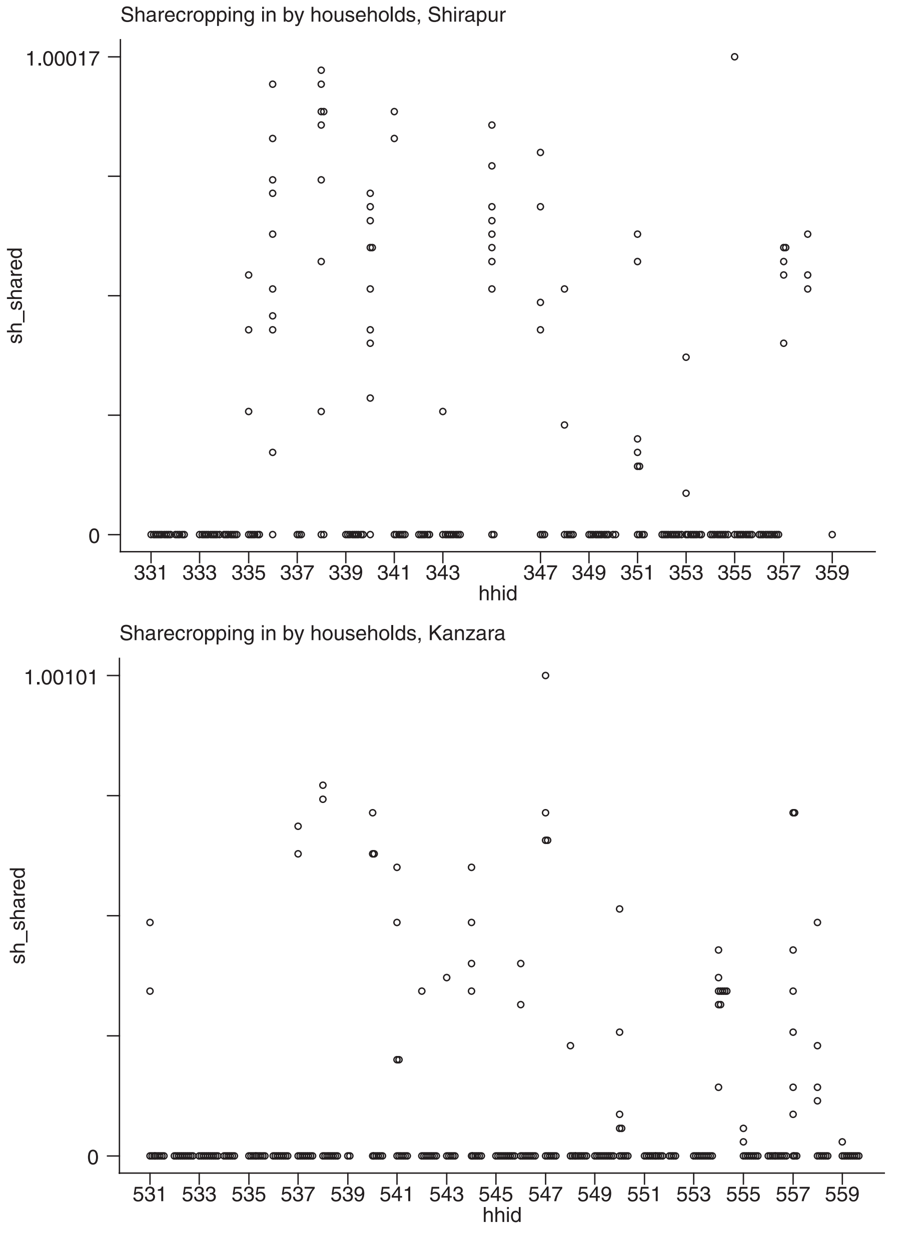

Fig. 3. Variation in sharecropping, by Household.

substantially less in the fixed effects estimates than in the random effects estimates suggests that something else is going on in the data.

One possible culprit that might exacerbate the IP relationship in the fixed effects estimates is measurement error. Even if area is measured accurately on average, year-to-

Table 2
The inverse productivity relationship in ICRISAT data[a] dependent variable: log of household profits

	(1) Random effects	(2) Fixed effects	(3) Random effects[b]	(4) Fixed effects[b]
Log total cropped area[c]	0.89***	0.62***	0.97	0.71***
	(2.57)	(4.53)	(0.72)	(3.48)
Monsoon onset	− 0.01***	− 0.01**	− 0.01***	− 0.01*
	(− 2.95)	(− 2.05)	(− 3.96)	(− 1.83)
Monsoon end	− 0.001	− 0.000	0.00	0.00
	(− 0.67)	(− 0.12)	(0.17)	(0.45)
Frequency of days with rain	− 0.79	− 0.87	− 0.81	− 0.61
	(− 1.59)	(− 1.54)	(− 1.62)	(− 1.06)
Total rainfall	0.000	0.000	0.00	− 0.00
	(1.34)	(0.26)	(1.45)	(− 0.48)
Real fertilizer price	0.22**	0.24	0.32***	0.35**
	(2.24)	(1.44)	(3.05)	(2.02)
Real sorghum price	− 0.04	− 0.009	0.05	0.00
	(− 0.30)	(− 0.04)	(0.04)	(0.01)
Real fodder price	− 0.004	− 0.01**	− 0.01***	− 0.02***
	(− 1.04)	(− 2.00)	(− 3.34)	(− 3.05)
Real wage, male, period 1	− 0.82*	− 0.69	− 0.76	− 1.25
	(− 1.65)	(− 0.92)	(− 1.54)	(− 1.59)
Real wage, male, period 2	1.33***	0.67	1.52***	0.40
	(2.65)	(0.74)	(2.72)	(0.64)
Real wage, female, period 1	0.55	0.72	0.59	1.77*
	(0.98)	(0.78)	(1.07)	(1.75)
Real wage, female, period 2	− 0.06	0.91	− 1.33*	0.45
	(− 0.10)	(1.03)	(− 1.77)	(0.63)
Share of irrigated land	***	***	1.14***	0.71**
			(6.61)	(2.31)
Average value of cropland	***	***	19.43***	18.22***
			(5.78)	(3.54)
Test for joint significance of wages and prices (*p*-value)	4.82	5.28	39.11	5.49
	(0.00)	(0.00)	(0.00)	(0.00)
Test for joint significance of rainfall (*p*-value)	3.34	3.50	16.99	3.99
	(0.01)	(0.01)	(0.00)	(0.00)
Test for joint significance of land quality (*p*-value)			1.92	3.91
			(0.00)	(0.00)

[a] Values in parentheses are *t*-statistics. Standard errors corrected for heteroskedasticity using Huber/White correction in fixed effects estimates.

[b] Includes variables for the share of different soil types not reported in tables.

[c] Reported *t*-statistics is for a test of the null hypothesis that $\gamma = 1$.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

year variations may be measured noisily. As is well known, when exogenous variables are subject to measurement error, fixed effects estimation will exacerbate the measurement error bias, resulting in coefficient estimates that are subject to greater bias than the random effects estimates.[12] In the extreme case, if the true variable is constant for a given household, but is measured with error, then fixed effects estimation would be a regression on pure noise. I consider the possible role of measurement error in the IP relationship more fully below.

The presence of measurement error greatly complicates the question of whether fixed or random effects is the appropriate model. Testing for the presence of fixed effects is problematic, since the Hausman test assumes that the fixed effects estimator is consistent, which is clearly not the case if measurement error is present. In the case of measurement error, rejection of random effects in favor of fixed effects based on the Hausman test implies that random effects estimates are different from fixed effects estimates, but not necessarily superior. In fact, the fixed effects estimates here would be subject to greater bias than the random effects, not less. Therefore, I estimate the model using both fixed and random effects estimators to examine the role that land quality and market imperfections may play in explaining the IP relationship. Comparison of the results from fixed effects and random effects models may also yield insight into the role that measurement error plays in generating the IP relationship as well.

3. Inverse productivity and land quality

Benjamin (1995), following a suggestion by Sen (1975), tests the hypothesis that unobserved variations in land quality explain the inverse productivity relationship in Java rice production. If high-quality land is subdivided more often than low-quality land, resulting in smaller plots of higher quality, yields per acre will be greater for smaller farmers. Failure to control for household level differences in land quality introduces omitted variable bias into the coefficient on farm size (and perhaps others), since farm size is correlated with omitted land quality. Since Benjamin lacks information on land quality at the farm or plot level, he uses instrumental variables (IV) estimation. If the instruments used for farm size are valid, (esp. uncorrelated with omitted land quality), then the IV estimates should be free from omitted variable bias. Benjamin argues that his findings are consistent with omitted land quality, but stops short of arguing that land quality is the culprit. In fact, he formally rejects a structural model of omitted land quality.[13]

One concern in Benjamin's analysis is the quality of the instruments he uses for farm size. In order for the IV estimates to address the question of bias arising from missing measures of land quality, they must be correlated with farm size but uncorrelated with unobserved variations in land quality and land productivity. An important improvement offered by this study is that the availability of household-level variables measuring land

[12] See Hsiao (1986), p. 63ff for a thorough discussion of the consistency of different estimators for panel data in the presence of measurement error.

[13] Bhalla and Roy (1988) find differences in soil quality across households within the same district partially explain the inverse productivity relationship they observed in the Fertilizer Demand Survey data from India. However, they failed to control for household fixed effects, which may have biased their estimates to the extent there are unobserved household characteristics that are correlated with land quality in their data.

quality allows for more direct estimation of the importance of land quality than was possible in Benjamin's study.

Because the ICRISAT data contain straightforward measures of land quality, including soil type, the presence of irrigation at the plot level, and the value of land by plot, it is possible to test directly the hypothesis that variations in land quality explain the IP relationship. If the inverse productivity relationship arises from differences in land quality, then including variables that measure land quality in the profit regressions should ameliorate the inverse productivity relationship. Likewise, differences in land quality may account for the differences in labor use, so including land quality would ameliorate the inverse relationship in labor demand equations as well.

The ICRISAT data includes information on soil type at the plot level. Each plot was assigned to one of nine different soil types. The distribution of types is not uniform across the ICRISAT villages. For example, over 70% of the soil in Aurepalle are classified as shallow red (measured over all years of the sample), while most of the land in Shirapur is deep black, medium black, or shallow black, respectively. On the other hand, over 80% of the soil in Kanzara are classified as medium black. So there are substantial differences in land type across villages. Moreover, the composition of plots changes from year to year, based on changes in sharecropping arrangements and (less frequently) sales or purchases of land.

Indeed, sharecropping and leasing provide important mechanisms for allowing land quality to change substantially from year to year. Sharecropping rates are quite high for several of the study villages. For example, land cultivated by sharecroppers accounted for 35.5% of gross cropped area in Shirapur. Only in Aurepalle was sharecropping rare, accounting for less than 1% of land in cultivation, with fixed rents accounting for another 3.1% (Walker and Ryan, 1990, p. 172).

An additional measure of soil quality is the per acre value of each plot. The value is measured by a village authority who is active in the local economy, so it avoids problems associated with self-reporting of land values by survey respondents. Moreover, the per acre value of farmland is probably the best measure of the inherent productivity of the land. If land values are determined in a present-value model, then the value represents the sum of expected future returns from farming, appropriately discounted, net of the value of all inputs. To the extent that these returns reflect the opportunity cost of inputs, the land value should provide a good measure of net returns. Nonetheless, this variable probably fails to measure differences in land quality perfectly, since there is room for error given the "opinion-based" nature of the variable, and the fact that it may be influenced by factors unrelated to its productivity in agriculture.

Columns 3 and 4 of Table 2 report household random effects and fixed effects estimates (respectively) of the household-level profits regressions when I controlled for variations in land quality. The dependent variable is the log of total profits, so that a coefficient estimate less than 1 on the log of total cropped area is consistent with the IP relationship. I control for four different soil categories as well as the share of the household's total cropped area that is irrigated and the value per acre of land. In the random effects regression, the inverse relationship between land productivity and cropped area is greatly reduced. The estimate of γ rises to 0.97 and is not significantly different from 1 at even the 10% level. Moreover, the variables measuring household-level variations in land quality are highly statistically significant. A chi-squared test for joint significance has a *p*-value less than 0.01, and coefficients on the share of household's cultivated land that is irrigated and the average value of cropland are positive and statistically significant at the 1% level as well. Variables measuring rainfall shocks continue to be important in explaining profits, with the monsoon onset significant at the 1% level and the frequency of rainfall days is significant at the 10% level. The coefficient on fertilizer price is significant and positive while the price of fodder is significant and negative, at odds with theory. Among the wage variables, the coefficients on first period male wage and second period female wage are significant and negative, as economic theory would suggest. This is consistent with the observation that second period tasks dominate in female labor. The second period male wage is positive and statistically significant.

The estimates based on household fixed effects are similar in most respect to the random effects estimates. A striking difference is the severity of the inverse relationship: The estimate of γ is 0.71 and is significantly less than 1 at the 1% level. In the fixed effects estimates, the inverse relationship is barely mitigated by controlling for household land quality. The coefficients on land value and the share of irrigated land are positive (as expected) and statistically significant at the 1% level, and the variables measuring soil type and land quality are jointly statistically significant at the 1% level.

While including land quality in the profits regression essentially eliminates the inverse relationship in the random effects estimates, the relationship is stubbornly persistent in the fixed effects estimates. This is further evidence that the estimated IP relationship itself may reflect in part the effects of measurement error, which is exacerbated in the fixed effects estimates. The role of land quality in explaining the IP relationship in the random effects estimates is still not complete, however. An important dimension of the observed IP relationship has been in labor demand equations. If controlling for variations in land quality eliminates the IP relationship in labor demand, then there is strong evidence that land quality may be the culprit in the observed IP relationship. In fact, Benjamin finds that the inverse relationship is more severe for labor demand than for either the quantity of physical output or household level profits in his study of rural Java.

I test for the presence of an inverse relationship in farm labor demand, and the role that land quality may play in explaining it. I constructed the sum of hours of family and hired labor using the detailed input and output data in Schedule Y of the ICRISAT data set. I separated labor by gender to reflect differences in gender roles in the agricultural production. Moreover, because labor use is likely to respond differently by gender to the weather shocks, which are an important part of the production environment, goodness of fit to the data may be improved by looking at the relationships differently. The dependent variable in labor demand regressions is the natural logarithm of total labor hours.

Regression results for male and female labor demand conditional on soil quality are summarized in Table 3; columns (1) and (2) are fixed effects estimates for male and female labor demand, respectively, and columns (3) and (4) are random effects estimates. I report both fixed effects and random effects estimates since testing for fixed effects in the presence of possible measurement error is problematic and, moreover, differences in the two sets of estimates may provide useful information.

The most important finding here is that the estimate of γ was less than 1 for both males and females in both fixed effects and random effects estimation. In the random effects

Table 3
Labor demand and the inverse relationship[a] (household fixed effects, White standard errors) dependent variable: log total hours, by gender

	Fixed effects		Random effects	
	(1) Male	(2) Female	(3) Male	(4) Female
Log total cropped area[b]	0.81***	0.80***	0.92***	0.86***
	(− 4.00)	(− 3.68)	(− 4.08)	(− 5.03)
Monsoon onset	0.01***	− 0.00	0.01***	0.00
	(5.31)	(− 1.19)	(7.11)	(0.25)
Monsoon end	0.00	− 0.00	0.00	− 0.00*
	(0.91)	(− 0.63)	(0.97)	(− 1.63)
Frequency of days with rainfall	1.03***	0.44	0.81***	− 0.06
	(5.09)	(1.75)	(3.84)	(− 0.22)
Total rainfall	− 0.06***	0.02	− 0.00***	0.03*
	(− 4.68)	(1.00)	(− 5.85)	(1.64)
Real price of fertilizer	− 0.09	− 0.01	− 0.03	0.23***
	(− 1.48)	(− 0.20)	(− 0.60)	(3.53)
Real price of sorghum	0.30***	0.26***	0.26***	0.02
	(4.06)	(2.67)	(4.33)	(0.28)
Real price of fodder	− 0.00	− 0.00	− 0.01***	− 0.01***
	(− 0.16)	(− 1.19)	(− 4.37)	(− 3.69)
Real wage, male, period 1	− 0.17	0.05	− 0.58***	− 1.10***
	(− 0.64)	(0.14)	(− 2.69)	(− 3.63)
Real wage, male, period 2	− 0.32	0.16	− 0.71***	0.62*
	(− 1.10)	(0.41)	(− 2.94)	(1.86)
Real wage, female, period 1	0.94***	0.25	2.03***	1.88***
	(2.62)	(0.51)	(8.00)	(5.19)
Real wage, female, period 2	− 0.04	− 0.41	0.14	− 1.13**
	(− 0.12)	(− 0.81)	(0.42)	(− 2.51)
Share of irrigated land	1.12***	1.09***	1.38***	1.34***
	(8.14)	(6.59)	(17.72)	(12.10)
Average value of cropland	0.89	5.58**	5.39***	9.23***
	(0.46)	(2.19)	(3.53)	(4.27)
Test for joint significance of rainfall (p-value)	11.47	3.98	68.79	7.27
	(0.00)	(0.00)	(0.00)	(0.12)
Test for joint significance of prices/wages (p-value)	4.00	2.04	99.00	59.11
	(0.00)	(0.04)	(0.00)	(0.00)
Test for joint significance of land quality (p-value)	10.54	12.61	564.4	321.7
	(0.00)	(0.00)	(0.00)	(0.00)

[a] Includes variables for the share of different soil types not reported in tables; values in parentheses are t-statistics. Standard errors corrected for heteroskedasticity using Huber/White correction in fixed effects estimates.

[b] Reported t-statistics is for a test of the null hypothesis that $\gamma = 1$.

* Statistically significant at the 10% level.

** Statistically significant at the 5% level.

*** Statistically significant at the 1% level.

estimates (which are less likely to be biased by measurement error) of male labor demand, γ was estimated to be 0.92 and was significantly less than 1 at the 1% level. The variables measuring household-level land quality were jointly significant at the 1% level and the share of irrigated land and average land value were both positive, suggesting that land and labor are complements in production. Rainfall shocks were jointly significant in explaining male labor use at the 1% level. Both first and second period male wages were negative and statistically significant at the 1% level as suggested by theory, and the female wage in the first period was positive and significant, suggesting that female and male labor are substitutes in the first period. The coefficient on monsoon onset was significant and positive, suggesting that a delay in the monsoon onset raises male labor use.

In random effects estimate of female labor demand, γ was estimated to be 0.86 and was significantly less than 1 at the 1% level. Variables measuring land quality were jointly significant at the 1% level and the coefficients on irrigated land and the value of cropland are significant and positive. Surprisingly, rainfall variables were not jointly significant. The coefficient on first-period female wage was positive and significant, at odds with the predictions of economic theory, while the second period female wage was negative and significant. The coefficient on fertilizer price was significant and positive, which seems surprising since fertilizer use should raise the demand for harvest labor (and perhaps labor for weeding), which is a female task, e.g. fertilizer and female labor should be complements.

In the fixed effects regressions, estimates of γ were sharply lower: 0.81 for males and 0.80 for females and were significantly less than 1 at the 1% level. Variables measuring land quality were jointly significant, as were the rainfall shocks and the wages. The fact that the fixed effects estimates of γ were farther below 1 than the random effects estimates is further indication that measurement error may explain the IP relationship.

4. The inverse relationship and imperfect markets

While land quality variables explain most (but not all) of the IP relationship in profits, it fails to explain the IP relationship in labor demand regressions. Moreover, the perverse own-wage coefficient on first-period wage and fertilizer price in the female labor demand regression suggests that markets for planting period female labor may not be clear in the villages. In the classical model of labor-market dualism, farmers with small plots are unable to sell labor in the spot labor market, so they over-allocate labor to their own plots, driving the marginal (revenue) product of own-farm labor below the market wage rate. Carter (1984) finds that very small households over-allocate labor and other variable inputs to own-farm production in India, which explains a large part of the inverse productivity relationship. Labor market failure is not adequate by itself to generate the IP relationship, however. In a simple model of farm size, Feder (1985) shows that farm size should adjust until the efficient distribution of land across farmers is achieved. Imperfections in the markets for other productive inputs are necessary to generate the observed relationship. If, for example, land markets are not working well to reallocate land across farm households, then imperfections in the labor market would combine with imperfections in the land market to generate the observed IP relationship. In a recent paper, Benjamin and Brandt (1997) found that for villages in rural China,"...where markets were more active, especially land rental markets, excess returns to land were diminished and inequality was lowest".

More sophisticated models of market failure might also explain the over-allocation of labor by small farmers. Principal-agent problems in labor supervision could drive a wedge between the productivity of own-farm labor and hired labor, causing farmers to over-allocate own-farm labor (Eswaran and Kotwol, 1985; Taslim, 1989). Binswanger and Rosenzweig (1986) posit that imperfect information in a search-theoretic model of the labor market could result in misallocation of labor, in which net-labor-supply households fail to supply labor and net-labor-demand households fail to buy labor. Each result would tend to reinforce the inverse productivity relationship, assuming that net-labor-supply (demand) is negatively (positively) correlated with farm size.

The semi-arid tropics in India are characterized by a rich assortment of labor and land market transactions, with considerable participation by most farm households in some form of the village wage labor market or land rental market. As much as one-half of the women and 40% of the men participate in the labor market, e.g. they work outside their family production activities, suggesting that hired labor is more important in women's labor than in men's.[14] Seventy percent of the families earn some income from the labor market (Kochar, 1999, p. 51). Land markets are similarly important, especially the sharecropping of land in and out, and sharecropping varies substantially across villages and within villages over time, as discussed above.

Of course, if markets do not clear, then prices and wages will not adequately control for the intensity of input use and the inverse relationship will persist even if prices and wages are included in the labor demand regressions. A measure of labor-market slack that adequately controls for involuntary unemployment in the wage labor market would ameliorate, if not eliminate the inverse relationship. The ICRISAT data allows for construction of such a measure.

I used detailed information contained in Schedule K of the ICRISAT data on the labor market activities of sample households. From 1979 to 1984, information was collected on the number of days individual members of the households worked own-farm and off-farm in both agricultural production, and a number of types of nonagricultural activities. In addition, information on the number of days in the sample period during which workers looked for work but were unable to obtain it was also collected. I calculated total labor supply by adding up days worked in all activities, except own-farm production work. To the extent that the imperfect labor markets view is correct, then including own-farm activities in the measure of total labor supply would bias the measure of involuntary unemployment. I calculated a gender-specific unemployment rate for both planting period and harvest period activities by village, where the numerator is days unemployed and the denominator is days in the labor market (excluding own-farm work days).[15]

[14] If female production tasks tend to be more centered around "peak" production times, then demand for hired female labor will be greater.

[15] Since this detailed information is only available from 1979 onward, the sample available is somewhat smaller than for the other regressions, with only 504 households included in the data set.

I also controlled for the degree of activity in the village land market. I constructed a variable measuring the average share of land within the village that was either sharecropped in or rented. For most villages, Aurepalle being an exception, sharecropping is far more important than renting. Benjamin and Brandt found that such a measure was a useful indicator of how well land markets worked in rural China early in the 20th century.

Estimates of labor demand conditional on both land quality and measures of labor and land market imperfections are reported in Table 4. The random effects estimates of labor demand for male and female labor (which are less susceptible to measurement error bias if it is present in the data) are presented in columns (3) and (4). For male labor, column 3, the estimate of γ is exactly equal to 1. The male unemployment rate is positive (as expected) and significant for the harvest period only. In contrast, the point estimate of second period male wage is positive, at odds with economic theory. One explanation is that the market for male labor in the second period may not be clearing. Another explanation is that wages are positively related to unobserved productivity and may be biased upward (as discussed in Benjamin). The first period unemployment rate for males is not significant, while the first period male wage is significant and negative so the market for male labor appears clear in the first period. The second period male unemployment rate is positive and highly significant, indicating that fewer off-farm opportunities lead households to use more male labor own-farm in the second period. The share of land in the village that is sharecropped or leased is not significant in determining male labor demand. The variables measuring land quality are statistically significant and have the expected sign.

Random effects estimates for female labor demand are given in column 4. First, while the estimated γ is not (statistically) significantly different from 1, it is rather far away at 0.94. Most surprising, though, the female unemployment rate in the first and second periods are both negative, although they are not significant at the 10% level, suggesting that higher unemployment rates in the village lead to less female labor use own-farm. The second period female wage is negative and significant, as expected and the first period female wage was not significantly different from 0. This indicates that the labor market appears to work in allocating female labor in the second period, but does not appear to do so in the first period. The estimated coefficient on the share of land sharecropped or rented in the village is negative and statistically significant at the 1% level, suggesting that the more active the village land market, the less female labor used in own-farm production. The first period wage is not significantly different from 0 at any reasonable level. Land quality variables are significant and have the expected sign.

The differential responses of male and female labor to the village labor and land market variables are worth examining more closely. First, the level of activity in the village land market is quite significant in explaining own-farm female labor use, but not male labor use. This suggests that households may respond to market failure in the female labor market by attempting to increase own-farm production. At the same time, neither first nor second period female unemployment is statistically significant in female labor demand regressions. Female labor use appears to respond at the extensive, rather than the intensive margin. In contrast, labor market failure seems to bear more heavily on male labor use, with the effect being especially large in the second period. This is consistent with

Table 4
Labor demand and profits, conditional on labor and land market variables[a]

	Fixed effects		Random effects		Fixed effects	Random effects
	(1) Male	(2) Female	(3) Male	(4) Female	(5) Profits	(6) Profits
Log total cropped area[b]	0.83***	0.83**	1.00	0.94	0.62***	1.00
	(−3.23)	(−2.24)	(− 0.18)	(− 1.54)	(− 2.94)	(0.17)
Monsoon onset	− 0.01	− 0.01**	− 0.00	− 0.01**	− 0.02	− 0.01
	(− 1.51)	(− 2.20)	(− 1.32)	(− 2.13)	(− 1.48)	(− 0.75)
Monsoon end	− 0.00	0.00	− 0.00**	− 0.00*	− 0.00	0.00
	(− 0.79)	(1.50)	(− 2.49)	(− 1.85)	(− 0.01)	(0.81)
Frequency of days with rainfall	1.51**	1.49***	0.47	1.41*	9.97***	5.91***
	(1.96)	(2.56)	(0.88)	(1.81)	(5.20)	(4.35)
Total rainfall	0.05**	0.08***	0.00	0.00**	− 0.08	− 0.00
	(2.38)	(2.77)	(1.45)	(2.46)	(− 1.29)	(− 1.18)
Real price of fertilizer	0.50*	0.56*	0.32**	− 0.14	− 1.16*	− 1.20*
	(1.83)	(1.64)	(2.22)	(− 0.69)	(− 1.77)	(− 1.90)
Real price of sorghum	− 0.60***	− 0.08**	− 0.54**	0.01	3.47***	1.75***
	(− 2.97)	(− 0.26)	(− 2.34)	(0.20)	(4.81)	(3.01)
Real price of fodder	0.04**	0.07***	0.01*	0.02**	0.07*	0.01
	(2.39)	(3.03)	(1.85)	(2.23)	(1.66)	(0.51)
Real wage, male, period 1	− 0.48	0.90	− 1.15**	− 1.70**	− 2.19	− 0.49
	(− 0.70)	(0.89)	(− 2.20)	(− 2.15)	(− 1.13)	(− 0.39)
Real wage, male, period 2	2.02***	1.28*	1.64***	2.41***	1.77	1.35
	(4.27)	(1.79)	(3.17)	(3.18)	(1.12)	(1.04)
Real wage, female, period 1	2.68***	0.12	2.65***	0.83	5.65***	1.86
	(4.28)	(0.13)	(4.27)	(0.94)	(3.03)	(1.18)
Real wage, female, period 2	− 4.95***	− 6.22***	− 2.54***	− 3.45***	− 7.21*	− 1.91
	(− 2.95)	(− 2.68)	(− 2.90)	(− 2.72)	(− 1.77)	(− 0.87)
Unemployment rate, male, period 1	− 2.28	− 6.57***	0.98	− 1.91	− 17.05***	− 9.85***
	(− 1.25)	(− 2.60)	(1.07)	(− 1.40)	(− 4.07)	(− 4.30)
Unemployment rate, male, period 2	4.70***	3.51*	3.47***	1.52	4.65	3.67
	(3.88)	(1.74)	(3.51)	(1.07)	(1.48)	(1.44)
Unemployment rate, female, period 1	0.02	− 0.36	− 1.38**	− 1.41	4.10**	5.03***
	(0.02)	(− 0.40)	(− 2.21)	(− 1.55)	(2.20)	(3.12)
Unemployment rate, female, period 2	− 1.28	1.30	− 1.91**	− 1.49	− 4.26*	− 1.80
	(− 1.24)	(1.00)	(− 2.41)	(− 1.29)	(− 1.92)	(− 0.88)
Village share of land sharecropped in	− 1.14**	− 5.17	− 0.11	− 4.57***	− 22.89***	− 6.94***
	(− 0.37)	(− 1.35)	(− 0.15)	(− 4.02)	(− 3.26)	(− 3.70)
Share of irrigated land	1.15***	1.33***	1.39***	1.40***	0.27	0.96***
	(7.23)	(5.55)	(13.70)	(9.25)	(0.70)	(3.91)
Average value of cropland	2.42	− 1.20	5.45***	6.64**	8.91	18.30***
	(0.951)	(− 0.31)	(2.95)	(2.33)	(1.59)	(4.09)

Table 4 (*continued*)

	Fixed effects		Random effects		Fixed effects	Random effects
	(1) Male	(2) Female	(3) Male	(4) Female	(5) Profits	(6) Profits
Test for joint significance of rainfall (p-value)	3.77 (0.00)	3.25 (0.01)	9.42 (0.05)	18.20 (0.00)	7.72 (0.01)	20.62 (0.00)
Test for joint significance of prices/wages (p-value)	5.00 (0.00)	2.83 (0.01)	27.91 (0.00)	23.6 (0.00)	4.57 (0.00)	39.21 (0.00)
Test for joint significance unemployment rates and village sharecropping (p-value)	4.56 (0.00)	5.48 (0.00)	19.1 (0.00)	27.5 (0.00)	4.43 (0.00)	34.55 (0.01)
Test for joint significance of land quality (p-value)	8.75 (0.00)	6.32 (0.00)	318.1 (0.00)	183.3 (0.00)	1.23 (0.29)	93.46 (0.00)

[a] Includes variables for the share of different soil types not reported in tables; values in parentheses are t-statistics. Standard errors corrected for heteroskedasticity using Huber/White correction.

[b] Reported t-statistics is for a test of the null hypothesis that $\gamma = 1$.

* Statistically significant at the 10% level.

** Statistically significant at the 5% level.

*** Statistically significant at the 1% level.

households, increasing the intensity of farming activity in response to male labor market failure.

While the fixed effects estimates are reported in columns (1) and (2), they are not discussed in detail here, since they are likely to be more susceptible to measurement error bias than the random effects. In fact, the estimate of γ is 0.83 in both the male and female labor demand regressions, well below the random effects estimates. This relationship between fixed effects and random effects estimates is consistent with exacerbated measurement error contributing to the IP relationship in labor demand for the ICRISAT households.

I also estimated household profit equations conditional on both land quality and labor- and land-market imperfections. First, the estimate of γ is sharply lower in the fixed effects estimates than the random effects estimates, consistent with the presence of measurement error in the data. Therefore, the random effects estimates are less likely to be subject to measurement error bias, and they are highlighted here. The most significant finding in the random effects estimates is that the γ is now exactly 1 in the profits regression, so that the IP relationship is completely explained away by the combination of land quality and market imperfections. Beyond that, household profits are positively related to output prices and negatively to the price of fertilizer. The coefficient on the share of land sharecropped or rented in the village is statistically significant and negative. The village unemployment rates and the degree of sharecropping or leasing are jointly statistically significant. The interpretation of the coefficients on village unemployment rates requires some care. Since family labor is valued at the prevailing wage, then to the extent that labor is over-applied to own-farm production when unemployment is high, it will reduce household profits as suggested by the negative (and significant) coefficient on first period male labor. Land quality variables are significant and have the expected sign. These results suggest that there may be important interactions between labor and land market imperfections and the distribution of land quality across households.

5. Does measurement error contribute to the inverse productivity relationship?

Labor market imperfections and differences in land quality appear to play a substantial role in explaining the IP relationship for both profits and labor in the ICRISAT data. In the random effects estimates of profit and male labor demand regressions the IP relationship is eliminated by controlling for differences in land quality and labor and land market imperfections; for female labor the estimate of γ is not statistically different from 1. In the fixed effects estimates, however, the IP relationship is stubbornly persistent. One explanation that accounts for the persistence of the IP relationship in the fixed effects (but not random effects) estimates is the possibility of measurement error in the variable measuring farm size.[16] Suppose that the observed variable A_{it}* is measured with error, so that:

$$A_{it}^{*} = A_{it} + \eta_{it} \tag{3}$$

Then the estimable relationship is given by:

$$\ln Y_{it} = \alpha_i + X_{it}\beta + \gamma \ln A_{it}^{*} + e_{it} \tag{4}$$

where the error term is $e_{it} = u_{it} - \gamma\eta_{it}$. In this case, the least squares estimator for α, β, and γ will be biased, since the error term e_{it} is correlated with the regressor A_{it}* through the random variable η_{it}. Either random or fixed effects estimates will be biased (and inconsistent), although the bias is exacerbated when fixed effects are used. This is consistent with the pattern of coefficient estimates encountered above, in which the fixed effects estimates of γ were always further below 1 than the random effects estimates. This is the first indication that measurement error may play a role in explaining the IP relationship. If the fixed effects themselves are meant to pick up the effect of omitted factors like land quality that are negatively correlated with farm size, they should be closer

[16] I am grateful to an anonymous referee for bringing this point to my attention.

to 1 than the random effects estimates. That they are further from 1 is supportive of the measurement error explanation.

Of course, under the assumption of constant returns to scale, the elasticity of profits and labor demands with respect to area farmed (γ) should all be one.[17] Since area farmed is the same variable in the profits and labor regressions, it must be mismeasured to the same extent in both. If measurement error were the only source of the IP relationship, then estimates of γ would be the same in profit and labor demand regressions. In the random effects estimates, γ remains below 1 in the female labor demand regression, but not the profit regression or male labor demand regressions. A plausible explanation for this might be more severe labor market imperfections for female than male labor, which is not adequately controlled by the unemployment rates. However, in the fixed effects estimates, estimates of γ are further from 1 in the profit regression than the labor demand regression. This is inconsistent with measurement error being the only source of the IP relationship, although measurement error may still play a role, as indeed a comparison of fixed and random effects estimates suggests.

One method for achieving unbiased estimates of γ is to use an instrumental variable estimator to get rid of measurement error bias. Estimating the equations again using instrumental variables and comparing the estimated coefficients with those above can help in determining whether measurement error plays a role. The Hausman test may be used to see whether estimates generated using instrumental variables are different than those generated using least squares.[18] In particular, if there is no measurement error present in A_{it}, both the fixed effects estimator and an instrumental variables estimator (also controlling for fixed effects) will be unbiased, but the fixed effects estimator will be efficient, in the sense of having a "smaller" variance–covariance matrix. If A_{it} is measured with error, the fixed effects estimator is biased, while an instrumental variable estimator will be unbiased.

In order to estimate the equations using instrumental variables one needs instruments for log total area that are both correlated with area, but uncorrelated with the error term in the equation being estimated and that do not belong in the structural equation of interest. That is, instruments should be orthogonal to u_{it}. Finding instruments that are correlated with farm size is straightforward; finding instruments that are uncorrelated with u_{it} poses more of a problem. In principal, lagged values of log area would be one possibility, but in fact these fail the orthogonality condition based on tests of the over-identifying restrictions. Moreover, there is no reason to think that instruments which are uncorrelated with the error term in the profit equation are necessarily uncorrelated with the error term in the labor demand equation, or vice versa, adding another layer of complexity. Other possible instruments for total area farmed are dummy variables for sharecropping or renting in land, and double-cropping by the household. These suffer from the criticism that they may be endogenous in the

[17] For a derivation, see Benjamin (1995), p. 73.

[18] The Hausman's test-statistics for a test of the null hypothesis H_o: $\gamma_{fe} = \gamma_{iv}$ is given by $(\gamma_{fe} - \gamma_{iv})/\sigma \sim N(0, 1)$, where σ is the appropriate diagonal element of the matrix $(\Omega_{iv}^{-1} - \Omega_{fe}^{-1})^{-1}$. Therefore, a z-test of the null hypothesis H_o: $\gamma_{fe} = \gamma_{iv}$ would serve as a test for measurement error.

current period (that is, correlated with the error term), although it is possible to test this by testing the over-identifying restrictions. Thus, these form the basis for the IV estimation that follows.

Results of the instrumental estimations are reported in Table 5. Since measurement error is most likely to contribute significantly to the IP relationship in the fixed effects model, the focus is on fixed effects estimates. If IV estimation eliminates the inverse relationship in the fixed effects model, then this suggests that in fact measurement error may be contributing to the observed relationship. The first-stage regression explaining area farmed is reported in column (1). Area farmed is positively correlated with total rainfall. Larger farms have on average less irrigation and are worth less than smaller farms. Both instruments used, dummy variables for sharecropping/leasing and double cropping, are statistically significant at the 1% level. The first-stage regression explained about 40% of the within-household variation in area farmed.

The instrumental variables, fixed effects estimates for the profit regression are given in column (2). I condition on variables measuring land quality and labor and land market imperfections, so the results in column (2) of Table 5 are comparable to those reported in column (5) of Table 4. The most striking feature of these results is that the estimate of γ is numerically equivalent to 1—the inverse productivity relationship in profits completely disappears. Coefficient estimates from the IV regression are otherwise quite similar to those obtained with fixed effects, with the exception of coefficients on the share of irrigated land and the average value of land. Both these variables have stronger effects in the IV regression than the fixed effects regression, suggesting a downward bias in the fixed effects estimates. Test of the over-identifying restrictions suggests that the null hypothesis that the over-identifying restrictions hold cannot be rejected at any reasonable level of significance. The Hausman's test-statistics for measurement error has a p-value of only 0.12, but the Hausman test (Hausman, 1978) is well known to have low power against identifying a false null.[19]

Instrumental variables estimates of female labor demand equations (using household fixed effects) are reported in column (4) of Table 5; these results may be compared with those in column (2) of Table 4. The inverse relationship in female labor demand is completely eliminated in the IV estimates. While the estimated γ is 1.12, it is not statistically different from 1. Coefficient estimates for wages and prices and labor market variables are in line with those obtained using fixed effects. One notable difference in the results is that the amount of land sharecropped or leased in the village is far less important in explaining female labor demand in these regressions, and is not statistically significant. The Hausman's test-statistics for measurement error has a p-value of 0.05, indicating statistical support for the presence of measurement error, e.g. I can reject a null hypothesis of no measurement error in this case.

Surprisingly, the IV fixed effects estimates of male labor demand did not offer any improvement in the IP relationship (column 3, Table 5). The estimate of γ remained unchanged at 0.83, and other coefficient estimates differed very little from

[19] See, for example, Nakamura and Nakamura (1985).

Table 5
Instrumental variables estimates of profit and labor demand equations[a] household fixed effects

	(1) Log total area	(2) Profits	(3) Male	(4) Female
Log total cropped area[b]		1.00	0.83*	1.12
		(−0.01)	(−1.77)	(0.86)
Monsoon onset	−0.01	−0.02	−0.01*	−0.02*
	(−1.32)	(−1.06)	(−1.85)	(−1.82)
Monsoon end	−0.00	0.00	−0.00	0.00
	(−1.30)	(0.13)	(−0.67)	(1.53)
Frequency of days with rainfall	−0.60	9.92***	1.51*	2.70**
	(−0.65)	(4.33)	(1.73)	(2.13)
Total rainfall	0.00*	−0.00*	0.00**	0.00*
	(1.82)	(−1.71)	(2.16)	(1.88)
Real price of fertilizer	0.04*	−1.24**	0.50**	0.50
	(0.18)	(−2.00)	(2.10)	(1.46)
Real price of sorghum	−0.27	3.59***	−0.61**	0.03
	(−0.88)	(4.60)	(−2.07)	(0.06)
Real price of fodder	0.01	0.06	0.04**	0.06**
	(0.40)	(1.37)	(2.38)	(2.59)
Real wage, male, period 1	−0.47	−2.22	−0.48	0.87
	(−0.55)	(−1.05)	(−0.60)	(0.74)
Real wage, male, period 2	1.00*	1.45*	2.02***	1.03
	(1.62)	(0.92)	(3.37)	(1.18)
Real wage, female, period 1	1.04	5.29***	2.68***	−0.16
	(1.32)	(2.69)	(3.57)	(−0.14)
Real wage, female, period 2	−1.18	−6.24	−4.97***	−5.46**
	(−0.66)	(−1.39)	(−2.90)	(−2.20)
Unemployment rate, male, period 1	−0.18*	−16.10***	−2.30	−5.81**
	(−0.09)	(−3.21)	(1.20)	(−2.09)
Unemployment rate, male, period 2	2.18*	3.74	4.72***	2.80
	(1.74)	(1.17)	(3.87)	(1.58)
Unemployment rate, female, period 1	0.28	3.63*	0.02	−0.73
	(0.35)	(1.82)	(0.03)	(−0.66)
Unemployment rate, female, period 2	−2.01**	−3.66	−1.29	1.78
	(−2.10)	(−1.52)	(−1.40)	(1.33)
Village share of land sharecropped/leased in	0.42	−22.67***	−1.14	−4.99
	(0.14)	(−2.94)	(−0.39)	(−1.17)
Share of irrigated land	−0.70***	0.53	1.14***	1.53
	(−5.53)	(1.54)	(8.29)	(7.64)
Average value of cropland	−4.13	11.42*	2.38	0.78
	(−1.54)	(1.67)	(0.91)	(0.21)
Dummy for household sharecropping in	0.48***			
	(9.09)			
Dummy for double-cropping	0.25***			
	(3.88)			
Test for joint significance of rainfall (p-value)	2.30	24.97	8.93	9.22
	(0.06)	(0.00)	(0.06)	(0.06)
Test for joint significance of prices/wages (p-value)	4.07	38.04	27.44	13.06
	(0.00)	(0.00)	(0.00)	(0.07)

(continued on next page)

Table 5 (*continued*)

	(1) Log total area	(2) Profits	(3) Male	(4) Female
Test for joint significance unemployment rates and village sharecropping (p-value)	4.44 (0.00)	13.83 (0.02)	20.97 (0.00)	22.83 (0.00)
Test for joint significance of land quality (p-value)	14.75 (0.00)	9.26 (0.23)	80.83 (0.00)	66.29 (0.00)
P-value, test for over-identifying restrictions		0.94	0.65	0.05*
Hausman test for difference between IV and FE estimate		1.54 (0.12)	***	2.07 (0.05)

[a] Includes variables for the share of different soil types not reported in tables; values in parentheses are t-statistics. Standard errors corrected for heteroskedasticity using Huber/White correction in fixed effects estimates.

[b] Reported t-statistics is for a test of the null hypothesis that γ=1.

[*] Significant at the 10% level.

[**] Significant at the 5% level.

[***] Significant at the 1% level.

the fixed effects estimates. A possible explanation is that the instruments used for area farmed are not uncorrelated with the error term in the male labor equation.

6. Conclusion

This paper finds that while land quality and market failures may explain most of the inverse relationship, especially in random effects estimates, measurement error in the farm size variable likely plays a role as well, especially in fixed effects estimates. The severity of the IP relationship is far more pronounced in fixed effects than random effects profit regressions. In random effects estimates, differences in land quality explain most of the inverse relationship between farm size and profits, but fail to explain the greater intensity of labor use by smaller farmers. Controlling for imperfections in village labor and land markets (along with differences in household land quality) wipes out the IP relationship in male labor demand, but not in female labor. This suggests that there may be important interactions between labor and land market imperfections and the allocation of land quality across households.

The nagging persistence of the inverse productivity puzzle in the fixed effects estimates suggests that measurement error may play a role in the IP relationship. The empirical results here, in which the IP relationship is always more severe in fixed than random effects, are consistent with the well-known tendency of fixed effects to exacerbate measurement error problems. When instrumental variables estimation is used to correct for measurement error, the estimated coefficient on area farmed is exactly 1 in the (fixed effects) profits regression, and not statistically different from 1 in the (fixed effects) female labor demand model.

These results suggest an important caveat for empirical research related to farm size debate and more generally to applied work in developing countries. Given the tendency of farm size to change little over time, applied researchers should use caution in applying fixed effects models to estimate the relationship between farm size and productivity.

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