Incentives, Supervision, and Sharecropper Productivity

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Abstract

Though sharecropping has long fascinated economists, the determinants of this contractual form are still poorly understood and the debate over the extent of moral hazard is far from settled. We address both issues by emphasizing the role of landlord supervision. When tenant effort is observable, but at a cost to the landlord, otherwise identical share-tenants can receive different levels of supervision and have different productivity. Unique data on monitoring frequency collected from share-tenants in rural Pakistan confirms that, controlling for selection, ‘supervised’ tenants are significantly more productive than ‘unsupervised’ ones. Also, landlords’ decisions regarding the intensity of supervision and the type of incentive contract to offer depend importantly on the cost of supervising tenants.

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1 Introduction

Sharecropping, among the archetypical incentive problems, has fascinated economists since Adam Smith (see, e.g., Laffont and Martimort, 2001). Despite the continued high prevalence of sharecropping in much of the developing world and a vast theoretical literature seeking to explain it, there have only been scattered attempts to link observed contractual forms in agriculture to specific models of the principal-agent relationship (recently, see Ackerberg and Botticini, 2002; Laffont and Matoussi, 1995; Pandey, 2004).

Most empirical attention has instead focused on the sharecropper-productivity debate; the question of whether tenant effort is indeed prohibitively costly to monitor, thereby leading to moral hazard (cf., Stiglitz, 1974), or whether underprovision of tenant effort is obviated by landlord supervision (as argued by, e.g., Johnson, 1950). While Shaban’s (1987) landmark study appeared to vindicate the moral hazard view, evidence for the ‘superiority’ of owner-cultivation or fixed rental over sharecropping is, on the whole, inconclusive (Otsuka, et al., 1992; Binswanger, et al., 1995).\(^1\)

In this paper, we argue that heterogeneity in supervision costs has important implications for both of these strands of the empirical literature on land tenancy. To show this, we develop a simple model in which tenant effort is observable and enforceable, but at a cost to the landlord. Tenants vary in their ex-ante wealth, landlords in their cost of supervision, and equilibrium in the tenancy market is achieved through a process of matching along these two dimensions. The model yields two insights: First, otherwise identical share-tenants can receive different levels of supervision and, consequently, have different productivities. Second, a landlord’s decisions regarding the intensity of supervision and the type of incentive contract to offer are driven (in a fairly restricted way) by his cost of supervision.

Our empirical work, which uses a large and detailed micro-level data set from rural Pakistan, begins by estimating the average yield differential between sharecropped and owner-cultivated plots cultivated by the same household. We are able to state with high confidence that this differential is small, a conclusion supported by evidence on family labor allocation. Next, using unique information on monitoring frequency collected directly from tenants, we demonstrate that yields on plots cultivated by ‘unsupervised’ tenants

\(^1\)In Laffont and Mattoussi’s (1995) study of tenancy in a Tunisian village, for example, sharecropper productivity (evaluated at mean tenure duration) is higher than that of owner/renters. On the other hand, recent evidence from a tenancy reform in West Bengal (Banerjee, et al., 2002) shows that a modest reallocation of property rights in favor of share-tenants, falling far short of outright ownership, led to a dramatic yield increase, much greater than any found in farm-household level data.
are significantly lower than yields on plots cultivated by ‘supervised’ tenants. There are simply not enough of the former type to generate large overall productivity differentials between sharecropped and owner-cultivated plots. Thus, accounting for variation in landlord supervision can help resolve the sharecropper-productivity debate.

The final step in our analysis is to build and estimate an econometric model of contractual choice, tightly linked to the theory, that takes into account both heterogeneity in landlord supervision costs and in tenant wealth. Consistent with the predictions of our theoretical model, we find that contracts involving high supervision combined with low-powered incentives are preferred by landlords residing in close proximity to their leased plots—i.e., by those with low supervision costs. This finding complements recent work showing that the nature of the share-contract is, in certain contexts, shaped by risk-sharing considerations (Pandey, 2004). Our contribution is to highlight the role of supervision costs in determining the form of share-tenancy.

The results also have important practical implications. To the extent that sharecropping exerts a serious drag on agricultural productivity, tenancy or land reform may be a rare example of ‘win-win’ policy. Redistributing property rights over land from wealthy landlords to poor tenants, in other words, could improve both equity and efficiency. Our analysis, however, provides a rather sobering assessment of the potential overall productivity gains from such policies in a setting where share-tenancy, though widespread, has been hitherto unregulated.

The remainder of the paper is organized as follows. Section 2 describes the baseline analysis of productivity differentials between sharecropped and owner-cultivated plots, as well as the data and context of our study. Section 3 presents the tenancy model with landlord supervision. The implications of the model for productivity differentials are explored in section 4, and for contractual choice in section 5. Section 6 summarizes the findings.

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2As discussed later, the econometric model is also attentive to landlord-tenant matching along these two dimensions (see Ackerberg and Botticini, 2002, for a more general discussion of the empirical issues involved).
2 Is Sharecropping Less Productive?

2.1 Data

Our empirical analysis draws upon agricultural production data collected in two separate household surveys from rural Pakistan. The first is round 14 of the IFPRI panel survey, fielded in 1993 in four districts and 52 villages. The second is the nationally representative Pakistan Rural Household Survey (PRHS) of 2001, which collected data from about 2,800 households sampled across 17 districts and 150 villages. All of the households in IFPRI-93 were purposively included in PRHS-01. Overall, about 60% of the households surveyed in 2001 were farm households and, as in 1993, a considerable fraction of these operated multiple plots. Because much of the plot-level agricultural production and tenancy data are comparable across the two surveys, part of the empirical work uses both data sets together. Our analysis of landlord supervision in section 4, however, relies solely on the 2001 survey, since the relevant questions were not asked in 1993.

2.2 Context

As a consequence of Pakistan’s extreme land ownership inequality, the fraction of tenanted land is high (more than a third), and about two-thirds of this land is under sharecropping. Despite numerous tenancy laws on the books (regarding eviction and the sharing of output and costs), contracts are \textit{de facto} unconstrained, as enforcement is practically nonexistent.

Sharecropping is the predominant form of tenancy in Sindh province, where the land ownership distribution is particularly skewed. According to reports by surveyed tenants, the median landlord owns 28 acres, whereas nearly 80% of the share-tenants are landless farmers. Large landlords in this region often employ labor supervisors (\textit{kamdars}) to manage their many tenants. In the Punjab, the second of the major provinces of Pakistan, tenancies are split more evenly between share and fixed rent contracts. Landlords in Punjab are also typically much smaller than in Sindh, with median holdings of only 7 acres, and are more likely to be resident in the same village as their tenants.

Although nearly three-quarters of share-tenants in our data report a 50-50 output sharing rule, this probably overstates the degree of uniformity in these contracts. For example, it is common for tenants to borrow from their landlords, often by taking an advance for their share of the input costs, which is typically repaid at harvest time. Output and cost shares alone thus do not fully characterize the terms of the share-contract.
2.3 Empirical Strategy

Our regression model for yields realized by cultivator \( c \) on plot \( i \) is

\[
y_{ci} = \gamma s_{ci} + \omega^i x_{ci} + \nu_c + \eta_{ci} \tag{1}
\]

where \( s_{ci} \) is an indicator of whether the plot is sharecropped and \( x_{ci} \) is a vector of exogenous plot characteristics. Thus, \( \gamma \) estimates the average yield differential between sharecropped and owner-cultivated (or rented) plots.\(^3\) The error component \( \nu_c \) captures unobserved factors common to a given cultivator that determine productivity; e.g., access to credit, farming knowledge, average land quality, and ownership of non-marketed assets more generally. The error component \( \eta_{ci} \) reflects plot-specific unobservables, such as soil fertility, that are not contained in \( x_{ci} \).

Since, in general, the decision to enter into a sharecropping contract depends upon the cultivator’s unobserved productivity, \( E[\nu_c | s_{ci} = 1] \neq 0 \) and OLS estimates of \( \gamma \) are subject to selection bias. All of the major theories of share-tenancy proposed thus far in the literature imply that \( E[\nu_c | s_{ci} = 1] < 0 \), i.e., that sharecroppers have lower unobserved productivity than owner-cultivators or fixed renters.\(^4\) This means that selection bias will, if anything, lead to an overstatement of the disincentive effects of share-tenancy. Our strategy for correcting this selectivity bias is essentially the same as that of Sha-ban (1987) and Bell (1977). In particular, we use household fixed effects to purge \( \nu_c \).

This procedure requires a sufficient number of owner-cum-sharecropper (OCS) households, owner-cultivators (or renters) that also cultivate at least one sharecropped plot.

Note, finally, that our household fixed effects estimator (as well as the one used by Shaban) is not robust to correlation between \( s_{ci} \) and \( \eta_{ci} \), as would arise, most plausibly, when there is adverse selection in the leasing market. Under adverse selection, sharecropped

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\(^3\)Tests for moral hazard based on gross productivity data have power only to the extent that labor effort does not have very close contractible substitutes, which is highly plausible. Even so, it might seem preferable to perform the test using net rather than gross productivity. In practice, however, there are several purchased inputs, each measured with considerable noise, so that net productivity tends to be less precisely measured than gross productivity.

\(^4\)Models with limited liability (e.g., Shetty, 1998; Basu, 1992; Mookherjee, 1997; Laffont and Matteossi, 1995) imply that wealthier cultivators are less likely to be share-tenants, but wealthier cultivators also tend to have more capital and other unobserved inputs. If (following Stiglitz, 1974) share-tenants are more risk averse, then they are also likely to be less productive than owner-cultivators because, firstly, risk aversion is negatively related to wealth and, secondly, because greater risk aversion militates against the use of riskier but more productive techniques. Under double-sided moral hazard (Eswaran and Kotwal, 1985), both tenant and landlord supply a noncontractible input. In the landlord’s case, this can be farming know-how, which the tenant is assumed to lack. Likewise, in Hallagan’s (1978) screening model, less able cultivators endogenously select into sharecropping.
land tends to be of lower quality than owner-cultivated land; i.e., $E[\eta_{ci} | s_{ci} = 1] < 0.5$. Thus, just as in the previous paragraph, ignoring this form of selection bias when it is present would lead us to understate the productivity of share-tenancy vis à vis owner-cultivation. Importantly, this means that a failure to find a negative $\gamma$ using a household fixed effects estimator cannot be due to adverse selection, since adverse selection can only make $\gamma$ appear more negative. In other words, our estimate of the disincentive effects of share-tenancy is (at worst) an upper bound.6

2.4 Productivity results

We focus on production of the five major crops: wheat, rice, cotton, sugarcane, and maize. Wheat is the principal crop cultivated in the rabi season (November-May), whereas rice, cotton, and maize are grown in the kharif season (May-November). Sugarcane is grown year-round, but is usually planted in kharif. Land devoted to fodder and a number of minor crops is excluded from consideration, as these outputs are difficult to measure accurately. In the IFPRI-93 sample, the five major crops account for 66% of cultivated area (71% of sharecropped area); the corresponding figure for the nationally representative PRHS-01 sample is 80% (83%). Yield is defined as the value of output from these five crops evaluated at median prices for that year divided by area planted to these crops and is kept in levels (rather than logged) to avoid problems with zeros.7 However, for ease of presentation of the yield results in Table 2, the dependent variable is scaled (see Appendix) so that $\gamma$ is interpretable as a percentage deviation relative to owner-cultivators/fixed renters.

Recall that the IFPRI-93 and PRHS-01 samples partly overlap to form a panel. For our purposes, this overlap is not large given the criterion that households cultivate at least one of the five major crops in both survey rounds. In our final sample, 16% of the households appear both in 1993 and 2001, contributing 29% of the plots. With

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5 While it is true that plots taken on fixed rent are also subject to adverse selection, only 5% of our estimation sample consists of renter-cum-sharecroppers. So, for the most part, we are comparing sharecropped to owner-cultivated plots.

6 Arcand et al. (2007) provide sophisticated tests for sharecropper moral hazard treating $s_{ci}$ as endogenous conditional on the household fixed effect; they use plot owner characteristics as instruments for contract type. However, these instruments are shown to be quite weak in their data. As the same holds in our data, we eschew an IV strategy on the grounds that it would render the estimates too imprecise to be informative.

7 Six percent of the plots had zero output. In addition, a small number of observations (27 in all) had to be dropped because of probable coding errors that generated excessive yields. Including these observations has a negligible impact on the results, but does raise the standard errors on the coefficient of interest by about 6%. 

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such panel data, there are two ways to implement the household fixed effects procedure described in section 2.4: either group plot-level observations by household irrespective of survey year (household basis), or group plots of a given household separately by survey year (household-year basis). The latter specification is less restrictive in that it allows for nonstationarity in household endowments (i.e., in $\nu_e$). Since, in using the household basis, it is likely that the same plot will appear twice in a given household group, we correct the standard errors in this case for clustering on household. Table 1 breaks down the number of plots available, by survey year. With respect to the household-year basis, the yield sample consists of 1,718 plots belonging to households with multiple plots. Of these, 403 belong to owner-cum-sharecropper households, and the rest to pure sharecroppers or owner-cultivator households. Including this latter group of households increases the precision of the estimates when we control for plot characteristics. Using the household basis boosts the multiple-plot household sample to 1,993 and the number of plots among these cultivated by owner-cum-sharecroppers to 771.

Our regressions control for the most important plot characteristics: area, value, location, irrigation, and soil type. Table A.1 in the Appendix describes these variables. We also control for the crop composition on a plot (see Appendix) – i.e., the fraction of area planted to each of the five major crops – and allow these composition effects to vary by survey year so as to capture any changes in relative crop prices. While, in theory, crop choice may be specified in the tenancy contract, and hence endogenous, in Pakistan, share-tenants generally have autonomy over crop choice and grow basically the same mix of crops as owner-cultivators. Indeed, for all five of the major crops in our yield measure, there is no significant difference in the proportion of area cultivated between sharecropped and other plots, once we control for tehsil (there are 23 tehsils or subdistricts in our sample). These regression results, using 2088 plots from PRHS-01, are summarized in the top panel of Appendix Table A.2.

The first three specifications in Table 2 use the less restrictive household-year basis

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8 For example, in the eight years between the two rounds of the survey, a household’s farm assets or financial position may have changed.

9 Because only households were followed and not individual plots, one cannot say for sure how many of the same plots were captured in PRHS-01. However, it is likely that most of the owner-cultivated plots and many of the leased plots belonging to panel households are in fact the same across survey rounds.

10 Owner-cum-sharecropper households are over-represented in the IFPRI survey by geographical accident; these households tend to be concentrated in central Punjab.

11 Based on the raw means in Appendix Table A.1, it appears that less cotton and more rice is planted on sharecropped land as compared to owner cultivated land. However, this is an artifact of the greater prevalence of sharecropping in rice-growing Sindh province vis-a-vis cotton-growing Punjab.
for the fixed effects. Without any covariates, we find that yields are about 3% lower on sharecropped plots. Sequentially adding controls for plot characteristics and crop composition in columns (2) and (3), respectively, lowers this estimate of $\gamma$ only slightly, along with the standard error. For the sake of brevity, we do not report the individual coefficients on the plot characteristics and crop composition variables. However, they are highly jointly significant in all specifications as indicated in Table 2. Results are virtually identical when we use the household basis for the fixed effects and there is no evident gain in precision from doing so. All in all, then, there is no significant productivity difference between sharecropped and owner-cultivated plots.

How informative is our failure to reject the null hypothesis of equal productivity? Andrews’ (1989) inverse power (IP) function allows us to quantify the set of alternatives against which our test has power. Based on yield specification (3) in Table 2, we would be equally likely as not to reject the null if the true yield differential were 6.5%; this figure demarcates the region of low power. On the other hand, if the true yield differential were 13%, we would be 95% certain of rejecting the null. Thus, our test has high power against yield differentials exceeding 13%.

Although 13% is a respectable number, we may do better with the more restrictive village fixed effects specification. This estimator, the results of which are reported in column (5) of Table 2, is not robust to the selection problem outlined above, but may be more efficient than household fixed effects, in part because it exploits a much larger 2,807 plot sample (see Table 1). In addition to village fixed effects, we allow for household random effects to deal with the correlation across plots within multi-plot households. As it happens, the village fixed effects estimates of $\gamma$ are less negative than their household fixed effects counterparts, which would be contrary to expectations except for the fact that this difference is not remotely significant; the two estimates are within a standard error of each other. Selection bias, therefore, does not appear to be a serious problem.\footnote{Again using the IP function, we can be 95% certain that the household fixed effect estimate of the yield differential is within 11 percentage points of the village fixed effect (household random effect) estimate. In other words, we would be very likely to detect moderate selection bias if it existed.}

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With an almost 40% lower standard error, however, the village fixed effects estimator of $\gamma$ is much more precise than the ones based on household fixed effects. As a consequence, we can now be 95% certain that the true yield differential is no greater than 8%.
2.5 Labor results

A complementary test for moral hazard is to compare the cultivator’s family labor input on sharecropped versus owned plots. While such a test has power only to the extent that the quantity of family labor captures the relevant tenant effort, it does have an important antecedent in the literature to which we can draw comparisons (see next subsection).

IFPRI-93 (but not PRHS-01) provides information on plot-level labor inputs. These data are collected for each type of worker (adult male and female, and male and female child), but since the vast majority of farm labor is supplied by adult men in this sample, we sum the days of all worker types. Farm labor inputs are also disaggregated by task (plowing/irrigation, sowing, weeding, harvesting, and threshing), which we initially combine. On average, families supply a total of 16 days annually for each acre cultivated.

For the family labor input regressions in the lower panels of Table 2, we use essentially the same approach as we did with yields.\textsuperscript{13} Based on the household fixed effects estimates, we find that total family labor per acre is about 6% lower on sharecropped plots than on owner-cultivated plots, but this difference is not significant. Unlike the case of yields, the estimated magnitude of the labor differential falls considerably after adding the controls. The village fixed effects estimate is somewhat more precise than that based on household fixed effects, but in this case a Hausman test rejects the former specification (\(p\)-value = 0.006). Conservatively, then, our test of the null of zero moral hazard has high power against family labor use differentials on the order of 20%.

A relatively more powerful test of moral hazard can be constructed by focusing on those tasks that are difficult to contract. Data from the farm wage employment section of PRHS-01 indicate that harvesting effort is much easier to monitor than effort in land preparation, weeding, and so forth. Two-thirds of all annual paid labor days in agriculture are for harvesting, compared to just 7% for weeding and 4% for tilling, and harvesting jobs are much more likely to pay piece rates, which rely on directly observing output and hence, indirectly, effort. Thus, using a family labor regression that excludes days spent harvesting and threshing (almost half of all family labor devoted to the average plot), the null hypothesis of zero moral hazard should be more likely to be rejected if it is indeed false. These regressions are estimated on exactly the same sample as for total

\textsuperscript{13}We do, however, take logs of labor hours instead of levels, as the former gives higher within \(R^2\)s in the fixed effect regressions and thus considerably lower standard errors. The standard errors of the percentage changes are calculated using the approximation formula given by van Garderen and Shah (2002).
family labor (see Table 1), and the results are reported in the bottom panel of Table 2. Surprisingly, the differential between sharecropped and owner-cultivated plots in terms of noncontractible family labor is only 2% (column (3)). This leads us to suspect that even the 6% overall family labor differential may overstate the extent of moral hazard.

2.6 Discussion

To sum up, we find no significant yield shortfall on share-tenanted land vis à vis owner-cultivated/rented land. Using similar procedures and data, Shaban (1987) obtains a significant 16% yield differential in six south Indian villages. We can rule out an effect of such magnitude with virtual certainty in neighboring Pakistan. Shaban also estimates that owner-cum-sharecroppers allocate 21% less male family labor overall (47% less female labor) to their sharecropped plots than they do to their owned plots (ICRISAT labor data are not disaggregated by task). Based on our findings, family labor differentials this large in rural Pakistan are highly improbable.

Otsuka, et al. (1992), faced with similar (though perhaps less conclusive) evidence, echo Johnson (1950) in surmising that supervision and enforcement of share-tenant effort must be broadly effective. Sharecropping, they argue, is adopted as long as monitoring costs are low enough to make it worthwhile relative to fixed rental. They go on to suggest that "significant inefficiency of share-tenancy is expected to arise only when the scope of contract choice is institutionally restricted" (p. 2007). Thus, rather than evidence of the general inefficiency of sharecropping, they view Shaban’s (1987) findings as an aberration arising from India’s legal environment. When land-to-the-tiller legislation effectively penalized long-term tenancy contracts, many landlords with high supervision costs switched from fixed rent to share contracts, since sharecroppers were easier to disguise as permanent farm laborers.

The proposition that sharecropper productivity increases with the degree of supervision has, however, rarely been subjected to empirical testing.\footnote{Ai et al. (1997) is the only other attempt that we are aware of. Using a different modelling approach and plot-level data from a single Tunisian village, they find, paradoxically, that crop yield is a decreasing function of the frequency of landlord visits with the tenant.} The rest of this paper formalizes and carries out such a test.
3 A tenancy model with supervision

3.1 Basic limited liability framework

Consider a constant returns to scale technology (plot size normalized to one), in which the application of tenant effort \( e \in (0, 1) \) yields an output of \( R \) with probability \( e \) and zero output with probability \((1 - e)\). Tenant effort is thus identified with the crop success probability. Both landlord and tenant are risk-neutral and production requires a fixed upfront outlay \( B \) (in units of output) on materials. Consistent with our empirical setting, we assume that \( B \) is shared between the landlord and the tenant in the same ratio as is output, although this assumption is of no theoretical consequence. All tenants face the same outside option which, without loss of generality, we set at 0. Tenants can, however, differ in initial wealth, \( w \), which is observable to the landlord.

In the event that \( R \) is realized, the tenant gets \( \alpha(R - B) - r \), where \( \alpha \) is a share of the net output, and \( r \) is a fixed upfront payment, which can be negative (see the case of the low-wealth tenant below). If the crop fails, the tenant pays an amount \( z \) that depends on his wealth. In particular, the tenant cannot be made to relinquish more than he owns, giving rise to a limited liability constraint (LLC)

\[
z \leq w. \tag{2}
\]

So, the tenant’s payoff is \( e(\alpha(R - B) - r) + (1 - e) \max(-r + \alpha B, -z) - \psi(e) \), where \( \psi(e) \), the disutility of effort, is strictly increasing and convex with \( \psi(0) = \psi'(0) = 0 \), \( \psi''(e) > 0 \) and \( \psi'''(e) > 0 \). The landlord’s return, on the other hand, is \( e((1 - \alpha)R + r + \alpha B) + (1 - e) \min(r + \alpha B, z) - B \).

3.2 No supervision

When the tenant’s effort is unobservable to the landlord and prohibitively costly to monitor, the landlord’s problem takes a standard form (e.g., Mookherjee, 1997). There are two interesting cases to consider. In the first, the tenant’s wealth exceeds a threshold \( \underline{w} \) at which he can just meet all of his contractual obligations, irrespective of the realization of output. Thus, if \( w \geq \underline{w} \), the landlord’s decision is to

\[
\max_{\alpha, r} (1 - \alpha)(eR - B) + r \tag{3}
\]

subject to the tenant’s participation constraint (PC)
\[
\alpha(eR - B) - r - \psi(e) \geq 0
\]  
(4)

and the effort incentive constraint (IC)

\[
\alpha R = \psi'(e).
\]  
(5)

Examination of the first-order conditions shows immediately that the landlord chooses \(\alpha = 1\), a fixed rent (FR) contract, and that the PC must bind at the tenant’s chosen effort \(e^*\). This effort level is first-best since \(R = \psi'(e^*)\). By definition of the wealth threshold, it must also be true that \(w = z = r + B = e^*R - \psi(e^*)\), where the last equality follows from the binding PC.

The second case of interest is \(w < w\). For such low-wealth tenants, the LLC must bind, and it is evident that \(z = w = r + \alpha B\). Notice that if \(w = 0\), we have \(r = -\alpha B\); the landlord essentially allows the tenant to borrow his share of the input costs upfront and repay at harvest. The landlord’s problem is now

\[
\max_{\alpha} (1 - \alpha)eR + w - B \quad s.t. \quad \alpha eR - w - \psi(e) \geq 0
\]  
(6)

and the IC as given above. The solution entails a share (S) contract, with \(\alpha_s < 1\). For certain \(w\), the PC may bind at the optimum, in which case \(\alpha_s = (\psi(e_s) + w)/e_sR\), where \(\alpha_s R = \psi'(e_s)\). The landlord’s return is then \(r_s = e_sR - B - \psi(e_s)\). As \(w\) falls, however, rather than further lowering \(\alpha_s\), the landlord may prefer to relinquish rents to the tenant in order to induce effort, so that the PC may cease to bind. Since the PC has to bind for \(w \geq w\), there must exist some range of \(w < w\) over which the PC is also binding. At any rate, whatever the status of the PC, it is clear that effort under an unmonitored share-contract is strictly less than first-best \((e_s < e^*)\).

### 3.3 Costly supervision

Suppose now that, as an alternative to leaving his tenant unsupervised, the landlord can monitor effort, but at a cost. Once the landlord bears this supervision cost, he can monitor perfectly.\(^{15}\) Let the cost of implementing a given level of effort be represented

\(^{15}\text{Demougin and Fluet (2001) and Allgulin and Ellingsen (2002) examine supervision in a principal-agent framework with limited liability and a probabilistic monitoring technology. Allowing for less than perfect monitoring in the present context would only complicate the exposition without yielding any new empirically relevant insights.}\)
by a function $\theta_0 + \theta_1(e)$, where $\theta_0 > 0$ and $\theta_1'(e) > 0$ and $\theta_1''(e) \geq 0$. Hence, the decision to monitor entails a fixed cost, independent of the effort level of the tenant, such as the employment of a kamdar, or forgoing the opportunity to live in town.

Since a FR contract always dominates monitoring when the LLC does not bind, we assume that the LLC is binding so that the landlord’s problem is

$$
\max_{e, \alpha} (1 - \alpha)eR + w - B - \theta_o - \theta_1(e) \quad \text{s.t.} \quad \alpha eR - w - \psi(e) \geq 0.
$$

(7)

The optimal effort level in the monitored share (MS) contract, $e_m$, is thus

$$
R = \psi'(e_m) + \theta_1'(e_m).
$$

(8)

Tenant effort with monitoring is less than first-best ($e_m < e^*$) because the cost to the landlord of inducing effort is always higher than the tenant’s marginal disutility. Moreover, at $e_m$, the tenant’s PC must be binding, regardless of wealth, as he is now being compensated for his observable effort.\(^{16}\) Consequently, the landlord’s return in the MS contract is $r_m = e_mR - B - \psi(e_m) - \theta_o - \theta_1(e_m)$.

Comparing $r_s$ and $r_m$ tells us what type of contract the landlord offers to different tenants as well as how this choice varies across landlords. Since $\partial r_s/\partial w > 0$ and $\partial r_m/\partial w = 0$, relatively low-wealth tenants will get the MS contract, whereas higher wealth tenants will get either an S contract or, if $w \geq w^*$, a FR contract. Furthermore, since $\partial r_s/\partial \theta_0 = 0$ and $\partial r_m/\partial \theta_0 < 0$, the wealth cutoff for switching to the MS contract is decreasing in supervision cost $\theta_0$. In other words,

**Proposition 1** An increase in $\theta_0$ lowers the tenant wealth threshold below which the landlord supervises, but has no effect on the wealth threshold above which he offers a fixed rent contract.

Does landlord supervision increase the productivity of a share-tenancy, *holding tenant wealth constant*? To answer this question, consider first the case in which the tenant earns no rents under the S contract. If, for any given $w$, there exists a $\theta_0$ such that the landlord chooses to monitor, it must be true that $e_m > e_s$.\(^{17}\) Intuitively, the landlord must get

\(^{16}\)We follow the convention that in case of indifference the agent takes the action favored by the agent. In a model with imperfect monitoring (e.g., Demougin and Fluet, 2001), effort is not fully contractible, so the tenant may earn an incentive rent while being supervised.

\(^{17}\)Proof: $r_m > r_s \Rightarrow (e_m - e_s)R - [\psi(e_m) - \psi(e_s)] > \theta_o + \theta_1(e_m) > 0$. Since $eR - \psi(e)$ attains a maximum at $e^*$ it must be increasing for $e < e^*$. The fact that both $e_m$ and $e_s$ are less than $e^*$ thus proves the result.
more effort out of the tenant when he monitors in order to justify the supervision costs. In the case where the tenant earns rents in the $S$ contract, however, this intuition may break down. In particular, tenants with wealth levels much lower than the threshold at which the PC ceases to bind in the $S$ contract earn relatively high rents. By monitoring, the landlord may be able to reduce the amount he pays his tenant by more than the added cost of supervision, even though the tenant supplies as much or even less effort in the $MS$ contract. Thus, what we have shown is that

**Proposition 2** There exists a range of tenant wealth over which productivity is higher in a share-contract with supervision than in one without supervision.

The question that we take to the data is whether this range of wealth is empirically relevant and hence whether, on average, supervision enhances sharecropper productivity.

### 3.4 Matching equilibrium

Our model also speaks to the manner in which landlords and tenants match with each other, which is relevant to the empirical analysis of contract choice in section 5. To understand matching, consider the total surplus $v$ from the pairing of a tenant with wealth $w$ and a landlord with (fixed) supervision cost $\theta_0$. We take the simplest case in which the tenant’s PC is binding under the $S$ contract. Under this assumption, the landlord gets all the rents, so that $v(\theta_0, w) = \max\{r_m(\theta_0), r_s(w)\}$. Since $\partial r_m/\partial \theta_0 < 0$ and $\partial r_s/\partial w > 0$, matching must be (weakly) positive assortative; landlords with high supervision costs will tend to hire high wealth tenants (since they are comparatively cheaper to incentivize) and low supervision cost landlords will tend to hire low wealth tenants (since they are comparatively cheaper to monitor).

We can now sketch out a simple model of landlord-tenant search and matching. Prior to contracting, potential tenants randomly drawn from the wealth distribution arrive sequentially before landlords who can either accept or reject them. If rejected, the tenant reenters the candidate pool. Given that searching for tenants is costly, landlords will adopt a "reservation wealth" strategy; that is, they will make a hire once they find a

---

\(^{18}\) In particular, we can show (proof omitted for brevity) that $v$ is supermodular; that is, if $\theta_0 > \theta_0'$ and $w > w'$, then $v(\theta_0, w) + v(\theta_0', w') \geq v(\theta_0, w') + v(\theta_0', w)$. Becker (1973) shows that strict supermodularity is a sufficient condition for positive assortative matching.

\(^{19}\) It is reasonable to assume in our setting that tenants also face search costs, thus allowing landlords to make "take-it-or-leave-it" offers at the time of contracting.
tenant whose wealth exceeds a given threshold. Since, by the above argument, landlords with higher supervision costs must have higher reservation wealth, we obtain

**Proposition 3** In equilibrium, tenant wealth and landlord supervision costs are positively, but not perfectly, correlated.

4 Are Supervised Sharecroppers More Productive?

4.1 Quantifying landlord supervision

PRHS-01 asks each share-tenant "during [kharif/rabi season] how many times did the landlord meet with you to discuss or supervise your activities on this plot." In case the landlord employed labor overseers, or *kamdars*, the same question was asked about meetings between the tenant and these individuals as well. Very few share-tenants (less than 4%) report never having had supervisory meetings with their landlord or with a *kamdar* during the year (meetings with *kamdars* occurred in about a quarter of share-tenancies). On one-half of all sharecropped plots, tenants report having had more than 30 meetings per year with their landlord/*kamdar*, and, on half of these plots, tenants claim to have had at least 90 meetings. While many of these conversations may have occurred during non-crucial periods or were not otherwise intended to elicit or enforce effort on the part of the tenant, it is clear that share-tenants in Pakistan are not being left to their own devices (Nabi, 1986, provides similar evidence in a smaller scale survey).20

We certainly do not want to treat supervision intensity as linear in the number of meetings, since there must be diminishing returns beyond a point, and possibly increasing returns at very low numbers of meetings as well. The simplest empirical approach, and the one we adopt here, is to assume a threshold number of annual meetings above which a tenant can be considered ‘supervised’. But, what should this threshold be? This is a question on which we will let the data speak.

To this end, we estimate a version of Hansen’s (1999) threshold regression model for panel data. Let \( m_{ci} \) be the number of meetings that cultivator \( c \) on plot \( i \) had with his landlord (defined only for sharecropped plots). Our modified yield regression is then

\[
y_{ci} = \gamma s_{ci} + \delta s_{ci} I(m_{ci} > k) + \omega x_{ci} + \nu_c + \eta_{ci}
\]  

20 Supervision is most intense in Sindh province (with a median of 48 annual meetings versus 8 for the remaining three provinces taken together), where landlords typically have larger holdings and are more likely to employ *kamdars*. 

14
where $I(\cdot)$ is the indicator function and $k$ is the threshold, which is treated as a parameter to be estimated. For ease of interpretation, we demean the supervision indicator using $E[I(m_{ci} > k)|s_{ci} = 1]$, so that $\gamma$ continues to estimate the mean difference in yields between sharecropped and owner-cultivated/rented plots.

Selection bias could be a problem here as well. Or, to put it another way, supervision could be endogenous. Landlords may choose which tenants to supervise based on ability or other unobserved productive attributes. If, for example, low productivity cultivators are monitored more intensively, then we would find a spurious negative relationship between yields and supervision. Estimation of equation 9 with household fixed effects, as in section 2, can correct for this problem to the extent that the cultivator-specific unobservable is constant across tenanted and owned plots. But, this estimator is not robust to selection on tenant type. In particular, landlords may decide which tenants to supervise based on tenant characteristics that do not influence productivity on the tenant’s own land. This would essentially give rise to an additional error component of the form $s_{ci}\mu_c$. To purge this component, we use a sample of tenant households with multiple sharecropped plots ($s_{ci} = 1 \forall i$) to estimate the following regression by household fixed effects:

$$y_{ci} = \delta I(m_{ci} > k) + \theta x_{ci} + \mu_c + \nu_c + \eta_{ci},$$

where $\mu_c + \nu_c$ is the fixed effect (and $\gamma$ is absorbed in the constant term).

### 4.2 Supervision and Productivity

#### 4.2.1 Main results

Our analysis of supervision and yields is based on a sample of 1,256 plots cultivated by multi-plot households in PRHS-01 (see Table 1). Replicating the household fixed effects specification (3) in Table 2 on this smaller sample, we obtain a yield differential of -4.2% (4.8), which is very similar to, but less precise than, our earlier result. Exploratory regressions (not reported here for the sake of brevity) using various linear splines in $m_{ci}$ suggest that the positive yield impact of additional supervision dies out at or before about 15 landlord-tenant meetings per year. To estimate the monitoring threshold $k$ directly, we search over values of $m_{ci}$ within a reasonable range and find the $\hat{k}$ that minimizes the sum of squared residuals ($\sum \eta_{ci}^2$) from equation 9 (see Hansen, 1999, for details).\footnote{We restrict the search for $k$ between the 10th and 50th percentiles of $m_{ci}$ among the 351 sharecropped plots in this sample. The 50th percentile is 21 annual meetings, which is already fairly intensive supervision.} Although
conventional standard errors on the coefficients in equation 9, which treat $\hat{k}$ as the true value of $k$, are asymptotically valid, the test of the null hypothesis $\delta = 0$ is non-standard, since $k$ is not identified under the null. We thus adopt the bootstrap $F$-test proposed by Hansen (1999).

Household fixed effects regressions, including plot characteristics and crop composition, are reported in Table 3. For baseline specification (1), the estimation algorithm produces an optimal threshold value of 10 meetings. In other words, the definition of supervision that best fits the data is one in which the tenant meets his landlord at least 11 times per year, or about once each month. Accordingly, 65% of the 351 share-tenanted plots in our fixed effects estimation sample are ‘supervised’ by the landlords and/or the landlord’s kamdar. To verify that monitoring decisions are not driven by choice of crop (even though we condition on crop choice throughout Table 3), we also regress the crop composition variables on a dummy for supervision, based on our data-driven definition, using the full sample of 670 sharecropped plots from the PRHS-01 (see lower panel in Appendix Table A.2). At 74%, supervision is somewhat more prevalent on this representative sample, but there is no significant association between supervision and crop mix.

Returning to Table 3, we see that the average yield differential between sharecropped plots and owner-cultivated/rented plots remains at about -4% after including the supervision variable. Supervised tenants, however, achieve 28% higher yields than unsupervised ones, and this difference is just about significant using the bootstrap $F$-test ($p$-value=0.051). Viewed relative to owner/rented cultivated plots (by means of a regression with suitably rescaled coefficients), plots cultivated by supervised tenants realize 3.0% (5.6) higher yields, a trivial difference. By contrast, land cultivated by unsupervised tenants is 17.8% (7.2) less productive than owner/renter cultivated land. Compare this latter figure to the -16% yield differential between sharecropped and owner-cultivated land found by Shaban (1987). Assuming that sharecroppers in Shaban’s Indian sample are largely unsupervised (see subsection 2.6), our results are remarkably consistent with his.

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22 If $\omega = 0$ in equation 9, a more transparent approach would be as follows: Let $\bar{y}_j = MS, S, OC$ be average yield for supervised sharecropped, unsupervised sharecropped, and owner/rented-cultivated plots, respectively. The overall sharecropper-owner yield differential tells us that $0.65\bar{y}_{MS} + 0.35\bar{y}_S = (1 - 0.04)\bar{y}_{OC}$, whereas the supervised-unsupervised sharecropper differential tells us that $\bar{y}_{MS} = (1 + 0.28)\bar{y}_S$. These two equations can be solved for $\bar{y}_{MS}/\bar{y}_{OC}$ and $\bar{y}_S/\bar{y}_{OC}$. Since $\omega \neq 0$, however, the regression-based approach must be used to account for differences in the means of the $x_{ai}$ across tenancy arrangements.
4.2.2 Robustness

To assess robustness to other potentially confounding factors, we fix the value of \( \kappa \) at 10 and add alternative sets of controls. Our first concern is that supervision is picking up characteristics of the tenancy that may have independent effects on yields. Perhaps newer tenants, whose abilities are less familiar to the landlord, are more heavily supervised and are also less productive. Specification (2) in Table 3 thus includes a dummy variable indicating that the share-tenancy has lasted no more than 3 years; it does not attract a significant coefficient. The number of landlord-tenant meetings could also reflect the social relationship between the two, which may have independent productivity effects. Yet, there is no evidence that this is the case, given the insignificant effect of a dummy for whether the landlord and tenant are related (including membership in the same caste/clan). Lastly, wealthier landlords may supervise more and, at the same time, might provide more or better quality inputs to their tenants. In specification (3), we control for the land, tractor, and tubewell ownership of the landlord, which, as before, has a negligible impact on the supervision coefficient. Specification (4) includes both sets of extra control variables together, also with no change in \( \delta \).

Another type of concern is that tenants on certain plots – for example, those that are more fertile – receive more supervision and that these plots give systematically different yields. Although we cannot control for unobserved attributes of the plot, we can check whether the estimates change when we do not control for observed attributes. As emphasized by Altonji, et al. (2005), such tests are only as good as the explanatory power of the covariates. In the present case, the removal of all plot-level controls from specification (1) reduces the within household \( R^2 \) substantially, from 0.091 to 0.017. At the same time, the estimate of \( \delta \) becomes 25.4 (11.1), which is practically indistinguishable from its counterpart in Table 3. Thus, to the extent that selection on observables is an accurate guide to selection on unobservables, our estimates are unlikely to suffer from the latter problem.

Our final set of robustness checks allows for the possibility that supervision is correlated with unobserved tenant characteristics. Table 4 presents estimates of equation 10 on the sub-sample of 113 households that cultivate at least two plots on share contracts (264 plots in all). Thus, instead of comparing yields on sharecropped plots (supervised or unsupervised) with yields on owned/rented plots cultivated by the same households, as in Table 3, here we compare yields on supervised sharecropped plots with yields on unsupervised plots cultivated by the same households. In specification (1), we run the
threshold estimation algorithm with household fixed effects on this new sample to obtain $\hat{k} = 9$, practically the same value as before. In this case, however, the effect of supervision is quite large, raising yields by 73% versus our earlier 28%, and highly significant, with the bootstrap $F$-test $p$-value coming in at 0.006. Specifications (2)-(4) show, once again, that $\delta$ is robust to the inclusion of the additional controls. In sum, our finding that landlord supervision enhances yield on sharecropped plots does not appear to be driven by tenant-specific unobservables. Indeed, 28% may well be a lower bound on the impact of supervision on sharecropper productivity.

5 Supervision Costs and Contractual Form

5.1 Econometric model

We have argued that differences in the form of tenancy, and, ultimately, in tenant productivity, are driven by variation in costs of supervision. To test this proposition directly, we now turn to an analysis of the landlord’s choice among alternative tenancy contracts. Based on the theory in section 3, we build an econometric model of landlord choice across $S$, $MS$ and $FR$ (fixed rent) contracts from the following equations:

\begin{align}
    r_m &= a\theta_0 + \kappa_m tx \\
    r_s &= bw + \kappa_s tx \\
    w &= \sigma_1 tx \\
    w &= c\theta_0 + \xi
\end{align}

The first two equations are linearized versions of the two return functions. The landlord’s return on the $MS$ contract depends negatively on his fixed supervision costs ($a < 0$), but is invariant to tenant wealth, whereas the return on the $S$ contract is increasing in tenant wealth ($b > 0$), but invariant to supervision costs. Each return function, as well as the fixed rental wealth threshold $w$, are also allowed to depend on plot characteristics $x$ (which includes a constant). Equation 11d is the matching function with match error $\xi$ being the sole source of randomness.$^{23}$ Proposition 3 implies that $c > 0$.

$^{23}$See, e.g., Ackerberg and Botticini (2002). These authors also allow for imperfect observability of principal/agent characteristics by the econometrician. In our case, $\theta_0$ can be proxied by a variable $z$ such that $\theta_0 = \lambda z + u$, where $u$ is a measurement error and $\text{sign}(\lambda)$ is known a priori. It is easy to see, after substitution into equations 6a and 6d, that this generalization merely adds another independent source
Putting these equations together, the landlord’s choices are characterized as follows:

\[ MS \iff r_m > r_s \iff \xi < d_1\theta_0 + \sigma_2\theta x \]
\[ S \iff r_m \leq r_s \text{ and } w < w \iff d_1\theta_0 + \sigma_2\theta x \leq \xi < d_2\theta_0 + \sigma_1\theta x \]
\[ FR \iff w \geq w \iff d_2\theta_0 + \sigma_1\theta x \leq \xi \]

where \( d_1 = (\frac{a}{b} - c) \), \( d_2 = -c \), and \( \sigma_2 = (\kappa_m - \kappa_s)/b \). Contract choice probabilities can thus be estimated using an ordered logit model, generalized to allow the slope coefficients to vary with the categorical value of the dependent variable.

There are three testable restrictions: \( d_1 < 0 \), \( d_2 < 0 \), and \( d_1 - d_2 < 0 \). Intuitively, higher supervision costs affect contract choice by directly lowering the landlord’s return to monitoring a tenant as well as by increasing the attractiveness to him of wealthier tenants, who can be given stronger incentives and concomitantly less supervision. The combination of these two effects, however, operates only at the margin between the \( MS \) and \( S \) contract, whereas the matching effect alone operates at the margin between the \( S \) and \( FR \) contract (cf., proposition 1). This fact allows us to identify the ratio of structural determinants of contract choice, i.e., \( \frac{a}{b} = d_1 - d_2 \), subject to the usual normalization in a discrete choice model.

### 5.2 Contract choice results

PRHS-01 not only asks landlords about each of their tenants and the terms of their contract, but also about the number of supervisory meetings that they or their kamdars had with their tenant. Thus, we can use the threshold number of annual landlord-tenant meetings (10) estimated in section 4 to construct an analogous supervision indicator on the landlord side. Based on this definition, out of 609 leased plots in our landlord sample, 29% are given on fixed rent, 25% on ‘unsupervised’ share-contracts, and 46% on ‘supervised’ share-contracts.\(^{24}\)

\(^{24}\)Because Sindh is dominated by very large landowners, with a low sampling probability, our subsample of 432 sharecropped plots on the landlord side is weighted toward NWFP (44%) and away from Sindh (25%). By contrast, 52% of the 351 sharecropped plots in the Table 3 sample are in Sindh, compared to 25% in NWFP. The upshot is that the use of kamdars, largely a Sindh phenomenon, is rarely reported in the landlord sample, and the overall intensity of supervision is somewhat lower than on the tenant side.

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To implement the econometric model, we need a variable that shifts the fixed cost of supervision. A natural candidate for $\theta_0$ is a measure of the accessibility of the plot to the landlord. We use the location of the plot relative to the landlord’s village; 14% of plots are outside the landlord’s village of residence. Although we do not have the actual distance between the plot and the landlord’s house, in most cases a plot in a different village will not be within walking distance, whereas a plot inside the village will be no more than a kilometer or two away.

Table 5 reports the generalized ordered logit estimates. Included in $x$ are the plot characteristics used previously, plus dummies for whether the contract applies to the kharif or rabi seasons only and dummies for the four provinces (Punjab, Sindh, NWFP, and Balochistan). The first two columns under specification (1) report the estimates of the threshold parameters for, respectively, the margin between the $S$ and $MS$ contracts and the margin between the $FR$ and the $S$ contracts. Thus, for example, the coefficient -1.59 in the first column is an estimate of $d_1$ (scaled by the error variance) and the coefficient -0.766 in the second column is the corresponding estimate of $d_2$. As indicated in the table, we can strongly reject all three null hypotheses, $d_1 = 0$, $d_2 = 0$, and $d_1 = d_2$ against their respective one-sided alternatives, $d_1 < 0$, $d_2 < 0$, and $d_1 - d_2 < 0$.

A possible caveat, however, is that only a select group of landlord’s may own plots outside their village. These landlords, in turn, may prefer particular types of contracts for reasons unrelated to supervision costs on the plot. For example, large and wealthy landlords may be more likely to own distant plots and to supervise their share-tenants. To address this problem, specification (2) of the contract choice model controls for landlord characteristics; namely, ownership of land, tractors and tubewells. While these asset variables do significantly affect the choice between $S$ and $MS$ contracts (although not in a consistent direction), their inclusion has little effect on the results of interest. Indeed, our key finding that greater supervision costs lower the returns to tenant monitoring (i.e., $\frac{g}{h} < 0$) is even strengthened.

6 Conclusions

Recent research suggests that tenancy or land reform may be among the elusive ‘win-win’ policies. While redistributing land rights to poor peasants clearly has attractive equity implications, Banerjee, et al.’s (2002) findings imply that such redistributions can lead to large efficiency gains as well. This paper delivers a less sanguine conclusion. Our
evidence shows that, on average, gross productivity of sharecropped land differs little from that of land cultivated by owners and fixed renters. We can be 95% confident that the mean yield differential does not exceed 8%.

To be sure, this average productivity difference conceals considerable heterogeneity. We find that yields on land cultivated by sharecroppers who are monitored by their landlords – the majority of share-tenants in Pakistan – are about equal to that of owner-cultivators, whereas yields on land cultivated by unmonitored sharecroppers are around 18% lower. So, if feasible, a land redistribution targeted toward the latter group would have potentially large productivity effects. The story would be different, however, for an untargeted reform. Taking a weighted average of the yield differentials for the two groups of share-tenants using the estimated proportion of supervised sharecroppers from our representative sample (i.e., 74%), we obtain an overall -2.4% gross productivity differential on sharecropped versus owner-cultivated/rented land. Not surprisingly, this figure is very close to our actual point estimate of the average yield differential in the full (village fixed effects) sample. Thus, in Pakistan, at least, broadly giving higher powered incentives to share-tenants would not have a dramatic impact on agricultural productivity.25

The overall efficiency implications, however, are more difficult to assess. Maintaining share-tenant productivity requires fairly heavy landlord supervision. Since this supervision is costly to the landlord, either in his own time or in money (for hiring labor overseers), redistributing land rights would generate an efficiency gain by eliminating the need to supervise. Nevertheless, it is improbable that the total resources expended on supervising tenants would come close to matching the gross return on supervision uncovered in this paper, although a definitive answer to this question must await further research.

Putting aside the policy question, this paper has also shown how explicit treatment of landlord supervision and recognition of heterogeneity in productivity differentials can cut through some of the confusion in the existing empirical literature on sharecropping. It is at least empirically plausible that Shaban’s (1987) influential finding of a large productivity advantage of owner-cultivation over sharecropping is due to legal constraints on tenancy that effectively created a large number of unsupervised sharecroppers. More broadly, this paper provides evidence on an important element underlying the form of incentive contracts: the principal’s cost of supervision. Further empirical work along these lines

25While this paper focuses on static inefficiency, in Jacoby and Mansuri (2007) we also use data from Pakistan to examine dynamic inefficiency due to land leasing. There we find that non-contractible investment is substantially lower on tenanted land than on owner-cultivated land, but that the implied yield effect is quite small. Thus, taken together, these studies point to limited productivity gains from land or tenancy reform.
would greatly enhance our understanding of real-world contracting problems.
References


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Notes: Number of sharecropped plots in parentheses. The household basis groups plots of a given household together regardless of survey year; household-year basis groups plots of a given household separately by survey year. Family labor data are available only in IFPRI-93.

\(^a\)Owner-cum-sharecropper, but also includes a few renter-cum-sharecroppers.

\(^b\)See Table 2. Multi-plot households used in household fixed effect estimation; all types used in village fixed effects estimation.

\(^c\)See Table 3.

\(^d\)See Table 4.
Table 2: Effect of Sharecropping on Yields and Family Labor

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**Normalized yield**

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Controls

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**Family labor in all tasks**

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**Family labor in all tasks except harvesting/threshing**

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Controls

| p-value controls | — | A | A,B | — | A,B |

Notes: See Table 1 for samples and Appendix Table A.1 for variable definitions and descriptive statistics: A = {plot characteristics}, B = {crop composition}. All specifications use fixed effects at household or village level as indicated. Standard error adjusted for household clustering in column (4). Village fixed effects specifications in column (5) include household random effects. Coefficients and standard errors from logarithmic specifications are converted to percentage changes. No distinction between household and household-year fixed effects since labor data are available only in IFPRI-93.
### Table 3: Landlord Supervision and Yields

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<td></td>
</tr>
<tr>
<td>Log of landholdings</td>
<td>5.78</td>
<td>-</td>
<td>-</td>
<td>0.050</td>
<td>0.071</td>
</tr>
<tr>
<td></td>
<td>(1.64)</td>
<td>(0.040)</td>
<td>(0.043)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Owns a tractor</td>
<td>0.32</td>
<td>-</td>
<td>-</td>
<td>12.7</td>
<td>10.4</td>
</tr>
<tr>
<td></td>
<td>(12.4)</td>
<td>(12.5)</td>
<td>(12.5)</td>
<td>(12.5)</td>
<td></td>
</tr>
<tr>
<td>Owns a tubewell</td>
<td>0.21</td>
<td>-</td>
<td>-</td>
<td>13.6</td>
<td>13.1</td>
</tr>
<tr>
<td></td>
<td>(14.8)</td>
<td>(14.8)</td>
<td>(14.8)</td>
<td>(14.8)</td>
<td></td>
</tr>
<tr>
<td>p-value controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Plot characteristics</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td>Crop composition</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. Dependent variable is major crop yield scaled by average yield on unsupervised sharecropped plots $\times 100$. All regressions include household fixed effects. $N = 1256$ plots (see Table 1).

- Coefficient renormalized to represent percentage change relative to owner-cultivation.
- Set to zero for non-sharecropped plots (means over 351 sharecropped plots).
Table 4: Landlord Supervision and Yields among Multi-plot Sharecroppers

<table>
<thead>
<tr>
<th></th>
<th>Mean (s.d.)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Supervised ($m_{ci} &gt; \hat{k}$)</td>
<td>0.66</td>
<td>73.1</td>
<td>78.1</td>
<td>69.4</td>
<td>74.1</td>
</tr>
<tr>
<td>Tenant characteristics</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Recent ($\leq 3$ yrs.)</td>
<td>0.26</td>
<td>—</td>
<td>19.5</td>
<td>—</td>
<td>21.0</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(18.3)</td>
<td></td>
<td>(18.6)</td>
</tr>
<tr>
<td>Relative of landlord</td>
<td>0.30</td>
<td>—</td>
<td>12.3</td>
<td>—</td>
<td>16.8</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(19.7)</td>
<td></td>
<td>(20.7)</td>
</tr>
<tr>
<td>Landlord characteristics</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log of landholdings</td>
<td>5.87</td>
<td>—</td>
<td>—</td>
<td>0.038</td>
<td>0.067</td>
</tr>
<tr>
<td></td>
<td>(1.71)</td>
<td></td>
<td></td>
<td>(0.081)</td>
<td>(0.085)</td>
</tr>
<tr>
<td>Owns a tractor</td>
<td>0.35</td>
<td>—</td>
<td>—</td>
<td>6.4</td>
<td>2.9</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(20.1)</td>
<td>(20.3)</td>
</tr>
<tr>
<td>Owns a tubewell</td>
<td>0.22</td>
<td>—</td>
<td>—</td>
<td>6.4</td>
<td>7.5</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(31.6)</td>
<td>(31.7)</td>
</tr>
<tr>
<td>p-value controls:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Plot characteristics</td>
<td>0.157</td>
<td>0.116</td>
<td>0.200</td>
<td>0.136</td>
<td></td>
</tr>
<tr>
<td>Crop composition</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. Dependent variable is major crop yield scaled by average yield on unsupervised sharecropped plots $\times 100$. All regressions include household fixed effects. $N = 264$ plots (see Table 1).
Table 5: Generalized Ordered Logit Model of Contract Choice

<table>
<thead>
<tr>
<th></th>
<th>Mean (s.d.)</th>
<th>(1) S - MS</th>
<th>FR - S</th>
<th>(2) S - MS</th>
<th>FR - S</th>
</tr>
</thead>
<tbody>
<tr>
<td>Plot outside landlord's village</td>
<td>0.14 (0.367)</td>
<td>-1.59 (0.382)</td>
<td>-0.766 (0.427)</td>
<td>-1.73 (0.389)</td>
<td>-0.727 (0.427)</td>
</tr>
<tr>
<td>$H_0 : d_i = 0$ vs. $H_1 : d_i &lt; 0$</td>
<td>[0.000]</td>
<td>[0.022]</td>
<td>[0.000]</td>
<td>[0.031]</td>
<td></td>
</tr>
<tr>
<td>$i = 1, 2$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Landlord characteristics**

<table>
<thead>
<tr>
<th></th>
<th>Mean (s.d.)</th>
<th>(1) S - MS</th>
<th>FR - S</th>
<th>(2) S - MS</th>
<th>FR - S</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log of landholdings</td>
<td>3.96 (1.43)</td>
<td>—</td>
<td>—</td>
<td>0.648 (0.242)</td>
<td>0.192 (0.165)</td>
</tr>
<tr>
<td>Owns a tractor</td>
<td>0.11 (0.593)</td>
<td>—</td>
<td>—</td>
<td>-1.33 (0.511)</td>
<td>0.511 (0.367)</td>
</tr>
<tr>
<td>Owns a tubewell</td>
<td>0.10 (0.519)</td>
<td>—</td>
<td>—</td>
<td>1.10 (0.505)</td>
<td>-0.656 (0.367)</td>
</tr>
</tbody>
</table>

$H_0 : d_1 = d_2$ vs. $H_1 : d_1 < d_2$ | [0.012] | [0.002] |

**Other plot characteristics:** $\chi^2_{(8)}$ | [0.042] | [0.014] | [0.103] | [0.049] |

Note: Standard errors, adjusted for village-level clustering, in parentheses; p-values (one-sided for the relevant structural hypotheses tests) in square brackets. Each equation includes all plot characteristics listed in Appendix, dummies for seasonal leases, and province dummies. Sample size is 609 landlord-owned plots.

*a* Index function coefficients for threshold between unmonitored and monitored share contract.

*b* Index function coefficients for threshold between fixed rent and unmonitored share contract.
## Appendix

**Table A.1: Descriptive Statistics**

<table>
<thead>
<tr>
<th></th>
<th>IFPRI - 93</th>
<th></th>
<th>PRHS - 01</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Owned/Rented</td>
<td>Sharecropped</td>
<td>Owned/Rented</td>
<td>Sharecropped</td>
</tr>
<tr>
<td>Plot area (acres)(^a)</td>
<td>10.8</td>
<td>9.9</td>
<td>8.3</td>
<td>7.4</td>
</tr>
<tr>
<td></td>
<td>(15.4)</td>
<td>(8.9)</td>
<td>(14.1)</td>
<td>(8.2)</td>
</tr>
<tr>
<td>log(plot value/acre)</td>
<td>10.6</td>
<td>10.3</td>
<td>9.5</td>
<td>9.1</td>
</tr>
<tr>
<td></td>
<td>(1.1)</td>
<td>(1.1)</td>
<td>(1.0)</td>
<td>(1.0)</td>
</tr>
<tr>
<td>Plot outside village</td>
<td>0.07</td>
<td>0.06</td>
<td>0.07</td>
<td>0.11</td>
</tr>
<tr>
<td>Year-round canal irrigation</td>
<td>0.43</td>
<td>0.58</td>
<td>0.31</td>
<td>0.53</td>
</tr>
<tr>
<td>Seasonal canal irrigation</td>
<td>0.09</td>
<td>0.10</td>
<td>0.23</td>
<td>0.21</td>
</tr>
<tr>
<td>Tubewell access</td>
<td>0.18</td>
<td>0.10</td>
<td>0.57</td>
<td>0.46</td>
</tr>
<tr>
<td>Sandy soil</td>
<td>0.11</td>
<td>0.19</td>
<td>0.18</td>
<td>0.24</td>
</tr>
<tr>
<td>Maira soil</td>
<td>0.24</td>
<td>0.24</td>
<td>0.29</td>
<td>0.20</td>
</tr>
<tr>
<td>Chikni soil</td>
<td>0.03</td>
<td>0.03</td>
<td>0.24</td>
<td>0.36</td>
</tr>
<tr>
<td>Cotton(^b)</td>
<td>0.02</td>
<td>0.00</td>
<td>0.13</td>
<td>0.09</td>
</tr>
<tr>
<td>Rice(^b)</td>
<td>0.19</td>
<td>0.38</td>
<td>0.15</td>
<td>0.30</td>
</tr>
<tr>
<td>Sugarcane(^b)</td>
<td>0.11</td>
<td>0.12</td>
<td>0.07</td>
<td>0.12</td>
</tr>
<tr>
<td>Maize(^b)</td>
<td>0.08</td>
<td>0.05</td>
<td>0.08</td>
<td>0.06</td>
</tr>
<tr>
<td>Normalized yield(^c)</td>
<td>1.00</td>
<td>0.98</td>
<td>1.00</td>
<td>1.02</td>
</tr>
<tr>
<td></td>
<td>(0.75)</td>
<td>(0.66)</td>
<td>(0.71)</td>
<td>(0.66)</td>
</tr>
<tr>
<td>N</td>
<td>440</td>
<td>279</td>
<td>1418</td>
<td>670</td>
</tr>
</tbody>
</table>

Notes: Means (Std. Dev.) for village fixed effects sample (2807 plots). Omitted categories: canal irrigation = none; soil type = clay.

\(^a\)Entered in logs in the regressions.

\(^b\)Fraction of area planted to five major crops (omitted crop is wheat).

\(^c\)Value of five major crops per acre scaled by year-specific mean for owned/rented plots.
Table A.2: Crop Mix and Tenancy

<table>
<thead>
<tr>
<th></th>
<th>Cotton</th>
<th>Rice</th>
<th>Sugarcane</th>
<th>Maize</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sharecropped plot(^a)</td>
<td>-0.006</td>
<td>0.003</td>
<td>0.015</td>
<td>-0.011</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.010)</td>
<td>(0.011)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Supervised sharecropped plot(^b)</td>
<td>0.004</td>
<td>-0.031</td>
<td>0.025</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.024)</td>
<td>(0.027)</td>
<td>(0.010)</td>
</tr>
</tbody>
</table>

Notes: Standard errors in parentheses. Dependent variables are the fraction of area of plot planted to given crop (wheat regressions are dropped because of redundancy). All specifications include tehsil fixed effects.

\(^a\)Uses full PRHS-01 sample of 2088 plots

\(^b\)Uses subsample of 670 sharecropped plots in PRHS-01 (494 are supervised).