MORAL HAZARD: EXPERIMENTAL EVIDENCE FROM TENANCY CONTRACTS*

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Abstract

We report results from a field experiment designed to estimate the effects of tenancy contracts on agricultural input choices, risk-taking, and output. The experiment induced variation in the terms of sharecropping contracts: some tenants paid 50% of output in compensation for land usage; others paid 25%; again others paid 50% of output and received cash, either fixed or stochastic. We find that tenants with higher output share utilized more inputs, cultivated riskier crops, and generated 60% more output relative to control. Cash transfers did not effect farm output. We interpret the increase in output as the incentive effect of sharecropping.

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"For, when the cultivator has to give to his landlord half of the returns to each dose of capital and labour that he applies to the land, it will not be to his interest to apply any doses the total return to which is less than twice enough to reward him." (Marshall, 1890, Book VI, Chapter X.14)

1 Introduction

Agricultural productivity in developing countries is notoriously low (Gollin et al., 2014). Understanding the sources of this productivity shortfall is key for designing policies that increase the incomes of the rural poor and improve food security. A commonly cited explanation for low agricultural output is the prevalence of sharecropping contracts whereby a tenant farmer pays a share of his output to the landowner. It is now a central idea of modern economic thought that such output sharing rules – whether in the form of contracts or taxes – will induce inefficient behaviour by the agent as long as she is not the full residual claimant. This powerful idea dates back to the classical authors Adam Smith and, in particular, Alfred Marshall, who stated it succinctly, precisely to highlight sharecropping contracts as a potential source of low agricultural output.

How important are sharecropping contracts in explaining low agricultural output? How would tenant farmers adjust their behaviour in response to a higher share? How much of that effect is due to the incentive effect conjectured by Alfred Marshall? These questions are empirical in nature, but little robust evidence exists. Recent empirical studies have demonstrated the role of agents' incentives in other contexts; see, for example, Prendergast (1999) and Bandiera et al. (2011) for studies of how to incentivise workers within a firm. In contrast to workers, tenant farmers typically have a wide set of decisions to make: they decide on the level of inputs used in cultivation, including their own effort level; and they often determine the mix of inputs they want to apply, for example, by determining the composition of crops and the planting technology. Such decisions often entail trading off expected returns with the riskiness of production (Ghatak and Pandey, 2000). In this sense, the decisions of tenant farmers are conceptually closer to those of entrepreneurs or corporate executives, analysed in public economics (Domar and Musgrave, 1944; Mossin, 1968) and corporate finance (Jensen and Meckling, 1976).

In this paper, we report results from a field experiment designed to estimate the incentive effects of sharecropping contracts on tenant farmers' input choices, risk-taking behaviour and output. These estimates provide answers to the three questions set out above.

Testing and quantifying the incentive effects of contracts on productive decisions generally poses at least two challenges. First, the outcomes of interest as well as the contractual terms are likely to be determined jointly by unobservable factors. In tenancy contracts, technology adoption and investment choices might be a function of factors such as unobserved productivity or outside options, and contractual terms are plausibly chosen endogenously as a function of the same factors. In fact, an extensive theoretical literature discusses the potential determinants of agricultural tenancy contracts.[1] This body of work implies that a positive correlation between the tenant's share in output and the level of total output might be the consequence of

[1] Sharecropping contracts can be understood as trading off incentive and risk-sharing motives (Stiglitz, 1974),

unobservable factors driving both the adoption of certain contractual terms and agricultural output, rather than evidence of incentive effects. Secondly, even when plausibly exogenous variation in a tenant's share of the output exists, it cannot solely be interpreted as an incentive effect since a higher output share corresponds to higher expected earnings, which might influence input choices independently.

In order to overcome these challenges and to quantify the incentive effects of tenancy contracts on agricultural output, input choices and risk-taking, we collaborated with the NGO BRAC Uganda to induce random variation in real-life tenancy contracts. A major focus of BRAC's work in Uganda is the socio-economic empowerment of young women. As part of this effort, BRAC leased plots of land to women from low socio-economic backgrounds who were interested in becoming farmers (henceforth 'tenants') and provided them with agricultural training and seeds for cultivation. The experiment was conducted with 304 tenants located in 237 villages. In all villages, tenants were contracted for one season under a sharecropping contract that gave them a 50% stake in the output. After signing the contract, tenants were randomized into three groups. In the first group (C), the contract was maintained – i.e. tenants received 50% of output. In the second group (T1), tenants were offered to keep 75% of the output. Tenants in a third group (T2) kept the same crop share as in control (50%) but received an additional fixed payment which was independent of their output level, paid at harvest and announced at the same time as T1 received news of the higher share. Within this third group, half of the tenants (T2A) received it as a risk-free cash transfer while the other half received part of their additional payment as a lottery (T2B).[2] The plots were visited pre-harvest to measure output levels and crop choice; and all tenants were surveyed shortly after the harvest to record their utilisation of inputs, such as labor, fertilizer, irrigation and tools.

The experimental design entails six key elements that allow to estimate the incentive effects of sharecropping contracts. First, by randomly assigning tenants to the control and treatment groups we ensure that tenants in different groups are not systematically different in their (unobservable) characteristics, such as their abilities, time preferences or risk attitudes. Second, the same contract was advertized in all groups to rule out ex-ante selection effects.[3] Our experimental design allows us to rule out such selection effects, since all tenants were hired under the same contract. Further tenants in the treatment groups were offered a change in contract that was unambiguously beneficial to avoid-design induced attrition. Third, for T1 we offer to change the terms of the tenancy agreements, in order to generate exogenous variation in

as incentivizing the landlord's inputs, some of which may be unobservable and therefore non-contractible (Eswaran and Kotwal, 1985), as trading off moral hazard in effort and risk-taking (Ghatak and Pandey, 2000), as screening tenants of different abilities (Hallagan, 1978; Newbery and Stiglitz, 1979) and as the optimal contract under financial constraints (Shetty, 1988; Laffont and Matoussi, 1995; Banerjee et al., 2002). See Binswanger and Rosenzweig (1982) and Otsuka and Hayami (1988) for reviews of the literature on contract choice and the co-existence of different types of tenancy contracts.

[2] The monetary value of the safe and risky transfers were equal in expectation.

[3] Ackerberg and Botticini (2002) show that tenants are matched endogenously to contracts (and plots/crops). There may still be ex-post differences due to differential attrition, which we test for. Randomization also ensures that there are no systematic differences in terms of plot or crop characteristics across the different treatment groups.

the tenant's share of the output.[4] This variation is key for estimating the incentive effect of the sharecropping contract. Forth, any differences in actions of tenants entitled to 75% of their output relative to those who receive 50% may be driven not only by the incentive effect, but also by the fact they have higher expected income. The latter may influence tenants' effort choice and risk-taking through various mechanisms, rendering the direction and the magnitude of the effect unclear.[5] For that reason we implement T2. The comparison of T2 with C allows to test for the presence of an income effect on agricultural productivity. Fifth, the additional income induced by T1 is risky. To test whether tenants' exposure to risk alters their agricultural choices, some tenants within T2 received a risky income transfer while others receive a safe one. Sixth, tenants might have an incentive to misreport the agricultural yield when a share of the output has to be given to the landlord. We therefore conducted pre-harvest plot-surveys to obtain an objective measure of expected yield.

To make precise the theoretical predictions on how these contractual variations may affect tenants' decisions, we present a simple conceptual framework. In the framework, an expected-utility-maximizing risk-averse tenant needs to decide on the level and the risk profile of inputs to use on a plot. In particular, she can choose between a risk-free cultivation technique or a risky but, in expectation, more productive one. Her compensation is in the form of a share s of the realized output and a fixed payment y, which could be positive (a wage), negative (a rent) or zero. The model predicts that an increase in s leads to an increase in the level of inputs the tenant chooses to employ in cultivation (the 'Marshallian inefficiency' effect); but has an ambiguous effect on her risk-taking, the direction of which depends on the shape of her utility function.[6] On the other hand, an increase in y should have no effect on the level of her investment in inputs and a non-negative effect on her risk-taking (positive if the tenant's absolute risk aversion is decreasing with income). In terms of output, the effect of increasing s is positive, as long as the effect on risk-taking does not offset the effect on increasing the level of inputs; while the effect of increasing y depends on how y affects tenants' risk-taking: if higher y leads to greater risk-taking by the tenant, it will lead to greater expected output as well.

We exploit the experimental variation to test these theoretical predictions. In terms of input levels, we find that the tenants in T1 (who received higher s) invested more in capital inputs to cultivate their plots. In particular, they used more fertilizer (120% more than the control group) and they invested more in agricultural tools (29% more relative to the control). There was also a positive, but imprecisely estimated effect on their labor use, coming mainly from an increase in hours of unpaid labor. On the other hand, T2 tenants who received higher y did

[4] We change the tenancy contract such that the tenants are uniformly better off, in order to avoid one potential source of endogenous attrition.

[5] Higher expected income may lower an individual's labor supply through a standard income effect. It may also affect incentives for risk-taking, as we demonstrate in section 2. Moreover, since tenants in T1 receive a better contract than what they had initially agreed on, they may increase their effort due to the presence of an efficiency wage. Finally, higher expected income may increase a tenant's access to credit which may enable him to increase the supply of inputs.

[6] The latter is a standard result in public finance literature that studies the effect of taxation on entrepreneurial risk-taking (Domar and Musgrave, 1944; Mossin, 1968; Stiglitz, 1969; Feldstein, 1969).

not invest more in capital or labor inputs relative to the control group. As such, the effects on the level of inputs used by the tenants are in line with the theoretical predictions.

To test the predictions on tenants' risk-taking, we study the crop-mix they chose to cultivate on their plots. We can think of crops as differently risky assets between which the tenant chooses, conditional on a level of investment. We first determine the riskiness of the crops, both using data from our experiment and from county-level panel data. There is a clear ranking in terms of the riskiness of the main crops available in our context. In particular maize, tomatoes and peanuts are more sensitive to rainfall compared to beans.[7] This is consistent with the volatility of crop yields in country-level panel data: output of beans has a lower coefficient of variation compared to peanuts, tomatoes or maize. From this perspective, beans are a less risky choice of crop to cultivate (compared to maize, tomatoes or peanuts) in this context. We find that tenants in T1 cultivated more of the riskier crops – maize, tomatoes and peanuts – while there was no significant effect on their cultivation of beans relative to the control group. This suggests that the increase in their crop share increased their risk-taking. For T2 tenants who received the safe income transfer, although weaker, we find some evidence of increased risk-taking as well. In particular, those who were assigned to T2A (risk-free income transfer group) had significantly more expected yield from peanuts relative to the control group. On the other hand, tenants in T2B, who received the risky income transfer, did not have higher yield from any crop. These findings imply that both higher s and higher y led to greater risk-taking by the tenants.

In terms of plot level output, the empirical findings and the theoretical predictions above imply that we should expect higher total output from the plots of T1 tenants (since both input levels and risk-taking is higher for them), while for T2 tenants we should have weak or no effects on output (since we find no effect on their input levels and a weak effect on their risk-taking). The treatment effects on output are in line with this. In particular, we find that tenants with higher s had on average 60% higher output compared to tenants in the control group. On the other hand, tenants in T2 (who retained the same crop share as the control group but received an exogenous cash transfer) did not generate significantly more (or less) output than tenants in the control group. This rules out the possibility that the increase in output of tenants with high crop share was driven by the increase in their expected income.[8] Overall, the findings imply that the tenants who received a higher crop share produced more output, and this effect is driven mainly by the incentive effect of the contract.

While the results above demonstrate that a higher crop share improved the output from plots under contract, this does not necessarily imply an improvement in tenants' welfare. In particular, tenants in T1 may have diverted their investments from other plots or reduced their involvement in other income-generating activities to generate the high output we observe in the experimental plots. In order to test if the high incentive contract crowded out other activities of the tenants (or their households), we estimate the effects on labor income, savings and

[7] We show this by using rainfall variation on plots cultivated by the tenants in the control group, and in a panel data of crop yields in Sub-Saharan African countries from FAOStat.

[8] When we analyze the effects of T2 by the riskiness of the income transfer, we do observe a small, positive and imprecisely-estimated effect of the risk-free payment on the output level of tenants in T2A. This is in line with the finding above that the tenants in this group increased their risk-taking by cultivating riskier crops.

consumption levels of the tenants and their households. We find that total household income is significantly higher among the high-incentive tenants and there is no significant impact on other indicators. This suggests that the higher incentives did not crowd out other income generating activities at the household level.

Another concern with high powered incentives is they may lead to over-investment in technologies that maximize short-term output at the expense of long-term soil quality. To test for this, we collected soil samples from the experimental plots and tested for any impact on indicators of soil quality.[9] We do not find any evidence that the high-incentive tenancy contracts had led to soil degradation by the end of the experiment. However, it is likely that there may have been unobservable changes in soil quality, or that the effects may be stronger in the long term.

Our paper contributes foremost to the empirical literature on the incentive effects of contracts, and in particular of tenancy contracts. Rao (1971) analyses plot data from India and finds output to be higher in owner-operated farms versus sharecropped farms. He shows that some 90% of the variation is explained by differences in observed land quality, and controlling for farm size, the relationship reverts. An important methodological contribution was made by Bell (1977) and Shaban (1987) who used plot-level data, and compared output and input levels across plots with different tenancy statuses within the same household, thus controlling for many unobservable household level characteristics. Nevertheless, the endogeneity of contract choice and the presence of unobserved plot-level characteristics are potential sources of bias in their findings (Arcand et al. (2007); Braido (2008); Jacoby and Mansuri (2009)). Banerjee et al. (2002) evaluate the impact of a tenancy reform which simulatenously changed legal output share of registered tenants and reduced their likelihood to be evicted by the landlord. Their work demonstrates a significant effect of the outside option of tenants on both equilibrium contracts and agricultural output. As far as we are aware, the current paper is the first to provide experimental evidence on the incentive effects of tenancy contracts.

We also contribute to a large literature in public finance that studies the effect of taxation on entrepreneurial risk-taking. That literature analyzes the risk-taking effects of taxation in isolation of any effect on investment levels. It finds that the sign of the effect of taxation on risk-taking is indeterminate in a general setup; predictions depend on the exact shape of the tax schedule as well as the utility function (Domar and Musgrave, 1944; Mossin, 1968; Stiglitz, 1969; Feldstein, 1969). Empirical tests of the theory have been limited due to the endogeneity of taxes to income and wealth (Feldstein, 1976). While some papers (see e.g. Poterba and Samwick, 2003) have exploited changes in tax regimes to study household portfolio choice, the evidence on the effect of taxation on entrepreneurial risk-taking is limited. We contribute to this literature by providing evidence that a lower tax (higher crop share) increases risk-taking among tenants.

The rest of the paper is organized as follows: Section 2 presents a conceptual framework that demonstrates the effects of the contractual changes we study on the tenant's investment in in-

[9] In particular, we test for the levels of nitrogen, potassium, phosphorous, organic matter as well as the Ph-level.

puts and risk-taking, Section 3 describes the setting, the design and the implementation of the field experiment, Section 4 presents the empirical findings, section 5 discusses key implications and Section 6 concludes.

2 Conceptual Framework

Set-Up Suppose that a tenant's preferences can be represented by expected utility maximisation and a Bernoulli utility function $u(c)$, defined over a consumption good c, with $u : \mathbb{R}^+ \to \mathbb{R}$ being increasing, concave and twice differentiable.

The tenant faces two choices: he purchases a bundle of inputs x at unit price p; and he determines the risk profile of returns to his investments. The latter choice represents both which input mix the tenant purchases, and how he chooses to use these inputs. We parametrize this notion by assuming that a tenant's gross return can be written as

$$a\theta f(x) + (1-a)f(x),$$

where $f : \mathbb{R}^+ \to \mathbb{R}^+$ is an increasing, concave and twice differentiable production function, θ is a random variable with positive support, and $a \in [0,1]$ captures the extent to which tenants take on risk. For $a = 0$ the tenant chooses not to be exposed to risk; for $a = 1$ he chooses the maximal level of risk; intermediate choices of a represent a convex combination of the return profiles of these polar cases. We implicitly normalize the return of the risk-free investment to 1. Let the c.d.f. of the distribution of θ be denoted by $F(\theta)$. Further we assume that $\mathbb{E}_\theta[\theta] > 1$.

A linear sharecropping contract specifies that the tenant pays a share s of gross output to the landlord, in addition to a fixed payment. The fixed payment to the landlord can be positive (a wage) or negative (a fixed rent). The tenant may also have additional income. We denote with y the sum of additional income and any payment to the tenant agreed with the landlord.

The tenant's consumption is then $c = s[a\theta f(x) + (1-a)f(x)] - px + y$. He will choose the input bundle x and the risk-profile of investment a to maximize

$$\mathbb{E}_\theta[u(c)] = \int u\left(s[a\theta f(x) + (1-a)f(x)] - px + y\right)\, dF(\theta). \tag{1}$$

This framework captures a number of aspects of a tenant's choice that we consider realistic and potentially important. Firstly, agricultural output is typically subject to aggregate risks that are difficult to insure locally, such as output risks resulting from rainfall and temperature variation or pest outbreaks. Secondly, we model tenants' risk aversion. There is both empirical evidence suggesting that tenants are risk averse, and theoretical reasons to believe that an agent's risk aversion might be important for her productive choices.[10] Third, we restrict attention to linear incentive contracts. This aspect of the model lacks theoretical generality, but not realism: surveys of tenancy contracts show that a large majority of observed sharecropping

[10] Smallholder farmers have been shown to exhibit substantial risk aversion in both survey and lottery based measures of risk aversion (Binswanger, 1980) and farmers' behaviour (Karlan et al., 2014). Risk aversion is central to standard explanations for the existence of partial incentive contracts, pioneered by Stiglitz (1974).

contracts take a linear form. Fourth, and most importantly, we think of the tenant's problem as choosing both the level of investment and the risk profile of investments. We believe this to be a realistic representation of a tenant's choice. Agricultural tenants typically choose the level of inputs such as labor time, and total expenditures on seeds, fertilizer, pesticides and irrigation, amongst others. However, in choosing the specific mix of these inputs, such as the composition of seeds, or how to apply them, they also effectively choose between investments with different risk profiles. Our set-up allows us to study both choices jointly: A change in the terms of the sharecropping arrangement – or, under an alternative interpretation, the effective tax schedule – will potentially lead to a change in the tenant's level of input purchases. A change in the sharecropping arrangement might also change the incentives for risk-taking. Importantly, both of these decisions might interact, and understanding them in isolation might not be possible. The framework outlined here allows us to study the joint determination of the level of investment and its risk profile. It will guide how we interpret the reduced form effects of variation in sharecropping arrangements on outcomes of interest.[11]

Understanding Tenants' Choices Assuming an interior solution, a tenant's optimal choice of (x, a) is characterized by the following first order conditions:

$$\int u_c \cdot [s[a\theta f_x(x) + (1-a) f_x(x)] - p] \; dF(\theta) \;=\; 0 \qquad (2)$$

$$\int u_c \cdot [s\theta f(x) - s f(x)] \; dF(\theta) \;=\; 0, \qquad (3)$$

where $u_c \equiv \frac{\partial u(c)}{\partial c}$. We will denote the elements of the associated Hessian as $D_{ij} = \frac{\partial^2 \mathbb{E}_\theta[u(c)]}{\partial i \partial j}$.

We will discuss how the tenant's optimal level of investment and risk-taking depend on s and y, the crop share and the fixed component of the contract, respectively. Lastly we will discuss the implications of the tenant's choices for expected output levels.

To understand the tenant's decision, it is instructive to first consider (3), the first order condition with respect to a. It captures the trade-off between higher mean returns and additional risk. It states that the tenant will take on risk until the marginal expected utility from additional risk is equal to 0. Note that $\theta - 1$ measures the difference of the risky return from the safe return.[12] An increase in a implies, at any realisation of θ, a larger diversion in gross income from what the tenant would receive from the safe project. For more extreme realisa-

[11] This formulation is restrictive in at least two ways. First, in our formulation $f(x)$ is not linear in x. A set-up where $f(x)$ is linear in x would be closer to the problem analysed in the theory of portfolio choice, where typically the level of asset holdings does not alter the distribution of marginal returns of each asset. Second, we assume, given a level of investment x, a particular relationship between the mean gross return of an investment and the associated dispersion around the mean. In a general framework the tenant would choose between a set of investments with unrestricted distributions of returns. Conditional on any mean return a preferred investment portfolio will always exist. However, the dispersion of returns around the mean of that portfolio might have a general form. In contrast, our formulation implies a particular relationship: at the mean return $[a\mathbb{E}_\theta[\theta] + (1-a)]f(x)$ gross returns have one specific distribution, with variance $a^2\mathbb{E}_\theta[\theta - \mathbb{E}_\theta[\theta]]^2 (f(x))^2$. A feature of this relationship is that higher mean returns require a tenant to take on additional dispersion of returns.

[12] The support of θ needs to include an interval of values smaller than 1 for the tenant not to choose $a = 1$, i.e. for an interior solution to exist.

tions of gross income, the marginal utility of gains relative to the safe investment is decreasing, and the marginal disutility of losses relative to the safe investment is increasing – and for that reason the tenant might not take on maximal risk. Now consider (2) and note that it can be rearranged in two parts as $\int u_c \cdot [sf_x(x) - p]\, dF(\theta) + \int u_c \cdot [saf_x(x)(\theta - 1)]\, dF(\theta) = 0$. The first part captures the increase in the expected marginal utility from increasing the level of returns across investments. The second part captures that a higher x also increases the absolute dispersion of returns, just like risk-taking does. This effect is negligible when the tenant can adjust the level of risk-taking, precisely because the tenant can offset any such effect by adjusting his level of risk-taking.[13] Therefore the only effect determining the level of investment is the standard trade-off between expected marginal utility gains and costs. We can derive the following prediction. (All proofs are in Appendix A.2.)

Result 1. *(Input Effects)*

i. *An increase of the tenant's share in output increases level of investment,* $\frac{dx}{ds} = -\frac{f_x(x)}{sf_{xx}(x)} > 0$.

ii. *An increase of the tenant's income level, y, leaves the level of investment unchanged,* $\frac{dx}{dy} = 0$. *(This result is independent of the stochastic profile of y.)*

The first part of the result captures the intuition that Alfred Marshall had in mind: a higher share of the agent increases the marginal return to investments keeping the costs constant, which increases the level of investments. This result would be straight-forward to demonstrate in a framework where the agent is risk neutral. Result 1 demonstrates that it also hols for risk averse tenants, as long as the tenant can adjust the level of risk-taking. The same would not be true for a risk-averse agent who cannot adjust the level of risk-taking. In that case an increase in s would, in addition to the standard incentive effect, also have a risk exposure and wealth effect. These effects might work in opposite direction, which is a well-known result since Pratt (1964) and Arrow (1971), and the sign of the sum of them would be ambiguous. When the tenant can adjust a endogenously, these additional effects drop out. (See Appendix A.1.)

It is worth noting that the effect of s on x will be larger when a adjusts endogenously than when a is kept fixed.[14] The intuition for this result is that the tenant does not take into account any effect of x on risk exposure when choosing its optimal level, since risk-exposure can be undone by adjusting the level of risk-taking conditional on x. This is important for the interpretation of our results. As we will show, tenants do adjust the risk level in our setting. If however in some other setting tenants cannot adjust a – for technological, institutional or behavioural reasons – we would expect to see smaller effects of changes in the tenants' share on investment levels.

[13]Note that this also implies that the second order conditions are satisfied.

[14]If the level of risk-taking adjusts endogenously, we can show that both the wealth effect and the risk exposure effect drop out. This is because any additional exposure to the risky outcome can be offset by adjusting a, which will also offset the additional average income that comes with holding the risky asset. What is left is the incentive effect. We can write $\frac{dx}{ds}$ as

$$\Psi \times \frac{-1}{D_{xx}} \int u_c \cdot [a\theta f_x(x) + (1-a) f_x(x)]\; dF(\theta),$$

with $\Psi := \frac{D_{xx}}{\int u_c [sa\theta f_{xx}(x) + s(1-a) f_{xx}(x)]} > 1$. Compare this to the incentive effect in Appendix A.1.

A useful corollary of Result 1 is that $\frac{-f_x(x)}{x f_{xx}(x)}$ is a sufficient statistic for the elasticity of investments with respect to the tenant's share s. In particular, no knowledge of the specific utility function is required to predict changes in the investment level when changing s. This implies that estimates of $\frac{dx}{ds}$ have external validity as long as production choices are common – even though tenants might have heterogeneous utility functions.

Lastly, in this framework an increase in y is predicted to leave the choice of x unchanged. This result also holds when the increase in y is stochastic, independent of the type of correlation structure between θ and y.

Next, we turn to the effects of the contractual terms on the tenant's risk-taking behaviour.

Result 2. *(Risk-Taking)*

i. The tenant's level of risk-taking, a, decreases with s when $u(\cdot)$ exhibits CARA, $\frac{da}{ds} < 0$. The sign of the effect is ambiguous when $u(\cdot)$ exhibits DARA.

ii. Consider a safe increase in y. Then the tenant's level of risk-taking, a, stays unchanged with an increase in y when $u(\cdot)$ exhibits CARA, $\frac{da}{dy} = 0$. It increases when $u(\cdot)$ exhibits DARA, $\frac{da}{dy} > 0$.

iii. Consider a stochastic increase in y, independent of the realisation of θ. Then the tenant's level of risk-taking, a, decreases with an increase in y when $u(\cdot)$ exhibits CARA, $\frac{da}{dy} < 0$. The sign of the effect is ambiguous when $u(\cdot)$ exhibits DARA.

A large literature in public finance studies the theoretical effect of taxation on risk-taking, especially entrepreneurial risk-taking. That literature analyzes the risk-taking effects of taxation in isolation of any effect on investment levels. It finds that the sign of the effect of taxation on risk-taking is indeterminate in a general setup; predictions depend on the exact shape of the tax schedule as well as the utility function (Domar and Musgrave, 1944; Mossin, 1968; Stiglitz, 1969; Feldstein, 1969).

The first part of Result 2 shows that this conclusion carries over to our framework. Only when the Bernoulli utility function exhibits CARA can we predict the sign of the effect of s on risk-taking without further assumptions. In this case an increase in s implies a higher exposure to risk – both mechanically and because x increases – as well as higher wealth. Since the additional wealth leaves absolute risk-taking unchanged under CARA, the additional exposure to risk is compensated by decreasing a. This is no longer true when the Bernoulli utility function exhibits DARA, since now the additional wealth implies that the tenant will be more willing to take on risk. Further assumptions are needed to sign the effect of s on risk-taking. This contrast with Result 1; a fixed income transfer was predicted to leave input choices unaffected. Note that DARA is likely a plausible assumption. Therefore this result also highlights how understanding the effect of the tenant's share on risk-taking is an inherently empirical question.

Part (ii.) of Result 2 mirrors the standard effect that absolute risk-taking is unchanged in response to higher y for a tenant characterized by a CARA utility function, and risk-taking decreases for an agent characterized by a DARA utility function. This is nothing more than the name-giving property of such utility functions. Part (iii.) highlights that when the additional income is stochastic, and independently distributed of θ, risk-taking will decrease relative to the result in part (ii.) of Result 2. The reason is that a stochastic y exposes the tenant to additional income risk, which will dampen his willingness to take on additional risk through a. In the plausible case of a DARA utility function, these results predict that fixed transfer will increase risk-taking, while a stochastic income transfer (independent of θ) may decrease risk-taking.

Lastly, much of the interest in sharecropping contracts is concerned with designing contracts and regulation to increase agricultural output. Results 1 and 2 do translate into implications for expected output.

Result 3. *(Output Effects)*

i. The tenant's expected output increases with s, as long as $\frac{da}{ds}$ *exceeds some negative bound.*

ii. The tenant's expected output increases with y if and only if $\frac{da}{dy} > 0$.

This result highlights how an increase in the tenant's share does not necessarily need to translate into higher expected output. The reason is that the increase in output implied by the Marshallian incentive effect might be offset by the tenant taking on less risk. However, moderate levels of risk reduction will still imply increases in expected output, and increases in the level of risk-taking will amplify the effect of the tenant's share on output. Increases in the tenant's income y will not effect the input choice, therefore any effect on expected output from changes in y will be coming from changes in the level of risk-taking.

3 Methods

3.1 Setting

In order to test the theoretical predictions above, we implemented a field experiment in collaboration with BRAC. Uganda has one of the youngest populations in the world. In 2000, 51% of Uganda's population of 23 million was aged 15 or below, while – as a point of comparison – the figure is 21.2% in the US. Among the youth, young girls are particularly at risk as they are more likely to drop out of school at an early age and face social and economic constraints in entering the labor market. As part of its efforts to empower young women in Uganda, BRAC operates a program called Empowerment and Livelihood for Adolescents (ELA). At the core of this program is to open, finance and operate youth "clubs" for girls. In rural areas, each club is assigned to a village. BRAC provides vocational and life skills training, as well as various social activities through these clubs.[15] As part of these efforts, BRAC decided to lease plots of land to women who were interested in becoming farmers. Women in Uganda head 26% of rural households and grow 70% - 80% of food crops, yet own less than 8% of the land (Nafula, 2008). In order to assist young women who wanted to become tenants but faced difficulty in accessing land, BRAC started implementing the intervention that forms the setting of our

[15] See Bandiera et al. (2017) for further details of the ELA program.

experiment. During the design phase, focus group discussions with club members revealed that due to credit constraints and concerns about the riskiness of cultivation, most potential tenants would not find a fixed-rent contract suitable. As such, BRAC decided to implement the intervention under a sharecropping arrangement.

3.2 Timeline

Season 0. In July 2013, BRAC selected 300 clubs in Eastern, Western and Central regions of Uganda to implement the intervention.[16] BRAC then attempted to rent a plot of agricultural land of roughly 0.5 acre close to the club, and searched for up to three club members who would be willing to rent the plot under a $s = 0.5$ sharecropping contract, with no fixed payment component, for one season. In 285 clubs both land and up to three potentially interested tenants were found. Figure 1 shows the location of these clubs. The interested girls were then offered the land, in an order randomized by the authors, until one of them decided to take up the offer and become a tenant. Both a plot and a farmer who actually signed up as tenant of the plot were found in 259 clubs. The tenants cultivated the plot for the following agricultural season, from September 2013 to January 2014 (henceforth 'Season 0'), which served as a pilot season.

Seasons 1 and 2. We collaborated with BRAC to implement the experiment in two agricultural seasons of 2014, spanning from April to July ('Season 1') and September 2014 to January 2015 ('Season 2').

In Season 1, the plots were advertized to be available for tenants under a 50% sharecropping contract with no fixed component. Tenants who had cultivated the plots in Season 0 were given priority. A little more than half of the Season 0 tenants decided to continue in Season 1. In the remaining cases new tenants signed up. Additionally BRAC decided to scale up the program for Season 1, both by renting an additional plot in clubs where a plot was rented in Season 0, and also by re-attempting to rent plots close to clubs for which no plots were found in Season 0. As a result of these changes 304 tenants signed a 50% sharecropping at the beginning of Season 1.

In preparation of Season 2, the plots were again offered under a 50% sharecropping contract with no fixed component, with priority given to Season 1 tenants.

Within-Season Procedures In each agricultural season BRAC provided the tenants with agricultural training. The training taught best-practise recommendations on (a) how to prepare the land and plant, (b) grow, and (c) harvest crops. The first training session was prior to planting, the last training session was prior to harvesting.[17] During the first of these training sessions, BRAC also provided the tenants with a bundle of high yield variety seeds. In Season 0 tenants

[16]Uganda has four main regions: Eastern, Western, Southern and Northern. The Northern region differs significantly from the other three in terms of geography, climate and socio-economic organisation.

[17]In Seasons 1 and 2 there were only two training sessions, and topics (a) and (b) were both taught during the first training session. In Season 0, topic (b) was taught in a separate mid-season training session.

were given maize, beans, cabbages and tomato seeds, for a total seed bundle value of 12 PPP USD; in Seasons 1 and 2 tenants were given maize, beans, and peanut seeds for a total seed bundle value of 32 PPP USD.[18] The training focussed on techniques related to these crops, respectively.

During the first training session the tenants signed the 50% sharecropping contract, valid for one season, in the presence of the BRAC program assistant as well as another witness.

3.3 Experiment

Treatments. The experiment was implemented in Seasons 1 and 2.[19] In both of these seasons the plots were advertized under a 50% sharecropping contract, and the tenants signed that contract during the first training session.

After the tenants signed the contract, they were exposed to one of four treatment conditions:

Control (C): Tenants keep the $s = 0.5$ contract.

High s (T1): Tenants are offered a contract with $s = 0.75$.

High y, safe (T2A): Tenants keep $s = 0.5$ and are offered a fixed payment y, with y being calibrated to 25% of Season 1's median harvest value, to be paid at the time of the next harvest.[20]

High y, risky (T2B): Tenants keep $s = 0.5$ and are offered a payment y, with y being 20% of Season 1's median harvest value with probability 0.5, and 30% of Season 1's average harvest value with probability 0.5, to be determined and paid out at harvest time.

We refer to the union of T2A and T2B as T2.

The updated contracts were first announced to the tenant through phone calls. During these calls tenants were first reminded that they have signed a $s = 0.5$ sharecropping agreement, and comprehension checks were performed and repeated until passed satisfactorily. Tenants in treatment groups T1 and T2 were informed about the change in the terms of their contract, and comprehension checks were performed, which were iterated until passed satisfactorily. During the phone calls the tenants in group T1 and T2 were told that they had been selected for the more favourable contract by a lottery. The terms of the new contract were explained to them in detail. Tenants in T2 were informed of the amount of cash transfer they would receive at the end of the season, those in T2B were explained the details of the lottery (i.e. the risky cash transfer) they would participate in. After the phone calls the BRAC program assistant delivered a letter to the tenant specifying the updated contract. Additionally all tenants received this information in a text message.

[18] In two areas potato seedlings were provided instead of peanuts. In that case the seed bundle value was 28 PPP USD.

[19] In the study area there are two agricultural seasons per year. The first one extends from March to August, the second from September to February. Rains in the first season are usually heavier, and the chance of crop failure is lower.

[20] The level of the transfer was calculated at the BRAC branch office level, using the median yield in Season 0.

Rationale. The objective of the research project was to understand the nature and magnitude of a number of specific effects of agricultural land tenure systems on the behaviour of the tenants on input choices, risk-taking and agricultural output. The experimental design allows us to test the Marshallian hypothesis and identify the mechanisms behind it.

Firstly, BRAC advertized the same contract (with s=50%) in all treatment groups. This design feature is a version of the seminal experimental design in Karlan and Zinman (2009) and controls for selection effects. As such, there is no reason to believe that tenants who sign up are systematically different on any unobservable characteristics across the different treatment groups.

Secondly, after the tenancy contracts were signed, tenants in T1 were offered s=75%, in order to generate variation in the tenant's share in output. We chose to implement a change to the tenancy contracts in T1 which we surely knew was dominating the original contract from the perspective of the tenant, in order to avoid design-induced attrition. The exogenous variation in crop share induced in T1 is key to test the incentive effects of sharecropping contracts.

Third, the comparison of input intensities and output levels between C and T1 does not necessarily allow to estimate the incentive effect of a higher share in the output. Increasing a tenant's share of the output does not only have an effect on the marginal revenue of the tenant, but might also have an income effect on effort choice. A classic income effect driven by the tenant's labor-leisure choice would suggest that individuals at higher expected income levels might choose to work less. We expect that such an effect might not be strong or not exist at all at low income levels. Higher expected income may also increase the tenant's access to credit which may enable her to increase the supply of inputs. In order to test for the collection of these effects, we introduce T2. In this group, tenants are offered the same crop share (s=50%) as in C, but receive a fixed payment. This allows to estimate the size of the income effect. If this estimate is 0, the comparison of C and T1 estimates the incentive effect.[21]

Finally, within T2, half of the tenants were offered a risk-free cash transfer (T2A) while for half of them, part of the payment was based on a lottery (T2B). The expected transfer amount is the same across the two groups. However, to the extent that any income effect exists in T1, this is the effect of a *risky income*, since agricultural output is necessarily stochastic from the point of view of the tenant. Any income effect likely varies with the risk profile of the additional income, either because the tenant is not risk neutral or because credit access is affected by the stochastic nature of the additional income. The treatment T2B mirrors the nature of the income effect in T1. It also allows, by comparison with T2A, to test whether indeed the risk profile of income is important to understand tenants' behaviour.

Implementation Challenges. In implementing the experimental design we faced two challenges. First, the amount of additional income provided in T2 was determined as 25% of the

[21] If the estimate of the income effect is significantly different from 0, we can estimate a structural model of labor supply which features two structural parameters, one governing the income effect, and one governing the incentive effect.

BRAC branch level median yield of Season 0. This might incorrectly reflect the (expected) income effect of treatment condition T1. We will address this when discussing the main effect of treatment condition T2 relative to treatment condition T1. Second, the information about the updated contract was to be provided shortly after the first training session, prior to the start of the agricultural season. This feature was implemented as such in Season 1. However, in Season 2, due to administrative constraints on the ground, the information about the updated contracts was provided to the tenants only in January 2015, three months late into the agricultural season. This needs to be kept in mind when interpreting the findings.

Randomization. The randomization was conducted at the club level, at the beginning of Season 0.[22] We grouped the 300 clubs originally designated as potential study sites into clusters of three clubs (henceforth referred to as 'blocks'), with the heuristic objective to minimize within-block geographic distance. The study groups were typically geographically bunched – see Figure 1 for a visualisation of this. We also grouped clubs into these large clusters (henceforth referred to as 'strata'). Assignment to treatment was randomized at the club level. We assigned equal fractions of the 300 potential study clubs to each treatment condition, stratified by blocks. Within T2 clubs we assigned 50 clubs to T2A and T2B, respectively, stratified by strata.

3.4 Surveys

During the course of the experiment we collected data through two types of survey instruments, a tenant level survey ('Tenant Survey') and a plot level survey designed to estimate yields ('Crop Assessment').

The Tenant Survey collected information on the tenants' and their households' demographic and socioeconomic characteristics. We recorded their educational history, health status, labor supply and employment characteristics, the household structure, detailed agricultural practices and output on each of the household's cultivated plots, including the plot rented from BRAC, ownerships status of plots, the household's asset holdings, and consumption expenditures, the tenant's savings and loans. The survey was administered by enumerators who were hired by BRAC and managed by the research team. The survey was administered to all potential tenants before each season of cultivation. It was also administered to all tenants about one month after the end of the season. It provides baseline information on the tenants in our sample (collected at the end of Season 0), as well as followup information at the end of Seasons 1 and 2.

A central challenge was that tenants have an incentive to misreport the agricultural yield when a share of the output's value has to be given to the landlord. Moreover, the incentive to misreport will be affected by their contract terms. For example, tenants who get to keep a higher crop share may underreport less, causing a downwards bias in the estimates. To obtain objective measures of agricultural yields we conducted each season a pre-harvest Crop Assessment survey.

[22]Typically there is only one BRAC club per village. As such, our unit of randomization is a village.

In Season 0 the Crop Assessment was conducted by BRAC: A BRAC program assistant and the tenant jointly determined the value of the harvest just before the maturity of maize. Then the tenants' due payment was determined based on this value and collected by the BRAC program assistant after the harvest.[23] This procedure turned out to have a number of drawbacks.[24] We therefore implemented an altered Crop Assessment procedure from Season 1 onwards: First, we conducted the survey before the harvest time of the earliest crops. Second, we measured the size of the plots using GPS trackers. Third, we collected exhaustive data on the plot, including signs of agricultural practices applied. To measure plant density and characteristics we repeatedly placed 1.5m $\times$ 1.5m quadrants, on representative sections of the plot's parcels (8 quadrants per acre), and measured detailed plant characteristics within each quadrant. Fourthly, we hired students of agriculture as enumerators who were trained to assess the expected yield at harvest time for every plant in every quadrant. Fifth, we took soil samples from the plots and tested levels of nitrogen, phosphorus, potassium, organic matter and soil PH. Sixth, we conducted a survey of crop prices at the nearest local markets at harvest time to obtain estimates of the harvest value. Starting from Season 1 these estimates were used to determine the tenants' due payment.

3.5 Sample and Attrition

Sample. Subsequently we will report results using data from the Tenant and Crop Assessment surveys in Seasons 1 and 2. All analysis is based on the sample of tenants who signed the tenancy contract in the beginning of Season 1 and the plots of those tenants. We will not report results for tenants who only started renting a plot in Season 2.

Attrition. Of the 304 tenants who signed a tenancy contract in the beginning of Season 1, we successfully surveyed 252 tenants during Follow-Up 1, and we surveyed the plots of 228 tenants in Crop Assessment 1.[25] Table A.1 tests whether attrition during Season 1 was differential by treatment status. In the control group, 24% of the tenants did not have a Crop Assessment in Season 1 and 20% of tenants could not be surveyed in the Tenant survey. The attrition rates in the treatment groups were similar to the control and to each other. The table shows that any differences in attrition rates across the different groups are not statistically significant.

As described in Section 3.2, tenants who participated in the first season of the experiment were invited to renew and continue the same contract for the second season of the experiment.[26] In

[23]Two BRAC program assistants, the tenant, and an enumerator visited the plot at harvest time and surveyed plant density, quality and other characteristics for maize, beans, tomatoes and cabbage, and estimated the plot size. In addition the tenants were asked to report the recalled amount and value of crops that had already been harvested, both for sale or own consumption.

[24]A drawback of the Season 0 Crop Assessment was that it was conducted shortly before the harvest time of maize. The harvest time of other crops, such as beans and tomatoes for example, would likely have been earlier. The tenants' self-reported outcome is likely measured with error, both because it suffers from recall bias and – more importantly – because tenants had an incentive to underreport their yield.

[25]This excludes plots on which the measured yield was above the 99th percentile of the distribution of measured yields, which we trimmed. Of those 228 tenants, 195 had rented one plot, 16 had rented two plots and 1 tenant had received 3 plots. There are therefore 262 plots from Season 1 in our dataset.

[26]In most cases where the tenants from Season 1 did not want to carry on cultivating the plot in Season 2,

Season 2 we surveyed 179 of the Season 1 tenants in Follow-Up 2, and we surveyed the plots of 192 of the Season 1 tenants in Crop Assessment 2.[27] In Table A.2, we test if the attrition rate in Season 2 – defined as a successful Crop Assessment or Tenant survey – was differential across the treatment and control groups. Differences in the rate of attrition are not significant throughout. They are also small in quantitative terms for the Crop Assessment 2 survey; however, the attrition rate in Follow-Up 2 is around 11 percentage points higher amongst treatment tenants. For this reason, we will present bounds for all the estimates where the bounds assume the tracked sample is either negatively or positively selected – described in detail in Section 4.1 below.

Balance. Table 1 provides balancing tests for the baseline characteristics of the tenants, such as their age, schooling, marital status, household demographics and socioeconomic status. The data was collected at the end of Season 0, prior to the start of Season 1. The average tenant in the sample is 21 years old, has 8 years of schooling, has 2 children and lives in a household with 5.4 people; 51% of the tenants are married. These observable characteristics are balanced across treatment groups. Out of 45 pairwise tests comparing C, T1 and T2 for each characteristic, we find that none are significantly different at conventional levels based on randomization inference p-values. With conventional standard errors, 2 out of the 45 pairwise tests are significant at 90% confidence level: tenants in T1 had higher consumption expenditure than those in T2, and less tools than in C. These differences are unlikely to be important for the interpretation of our results.

4 Results

4.1 Estimation

In order to identify the treatment effects of different contractual variations, we estimate:

$$y_{ict} = \sum_{k=1}^{2} \lambda_k T_{ik} + \delta_s + \epsilon_{ict}, \quad (4)$$

where y_{ict} is the outcome of interest for tenant i from club c in season t; T_{ik} is an indicator variable equal to 1 if tenant i belonged to a club of treatment group k and 0 otherwise, and δ_s are strata fixed effects. The sample includes tenants who were contracted at the beginning of season 1, prior to randomization. We use observations from both seasons 1 and 2 in order to improve statistical power.

The key parameters of interest are λ_k, the difference between outcomes of tenants who were assigned to treatment k and the control group. Under the identifying assumption that the

BRAC found replacement tenants. However, since this round of recruitment was carried on after the random assignment into treatment and control groups, we exclude these replacement tenants from the analysis in order to control for any selection effects.

[27]This excludes plots on which the measured yield was above the 99th percentile of the distribution of measured yields, which we trimmed. Of those 192 tenants, 173 had rented one plot, and 19 had rented two plots. There are therefore 211 plots from Season 2 in our dataset.

control group represents a valid counterfactual, λ_k identifies the causal effect of the change in tenant i's contract on y_{ict}. In all regressions we report standard errors, clustered at the club level (the unit of randomization).

Throughout the paper, the p-values associated with hypothesis tests are calculated using randomization inference. We estimate the coefficient of interest in 1000 alternative random assignments, chosen randomly with replacement from the set of possible assignments given our stratified randomization procedure. In each iteration we cluster standard errors at the club level, and record the distribution of the F-statistic associated with the hypothesis of interest. The randomization inference p-values report the percentile of the F-statistic found under the actual treatment assignment in the distribution of F-statistics found under alternative treatment assignments.

In order to assess the sensitivity of our findings to differential attrition (see Section 3.5), we calculate bounds that adjust for differential attrition across the treatment and control groups under different assumptions regarding the positioning of the attritors within the distribution. 'Lee bounds' (Lee, 2009) trim observations from above (below) in the group(s) with lower attrition, to equalize the response rates across the treatment and control groups.[28] We then re-estimate the treatment effects in the trimmed sample to deliver the lower (upper) bounds for the true treatment effects. We also calculate alternative bounds, following Fairlie et al. (2015). For non-responders we impute – within treatment groups – the mean minus (plus) a specified standard deviation multiple of the observed distribution of outcomes in that treatment group. We then re-estimate the treatment effects in the sample including imputed data to find their lower (upper) bounds.

4.2 Effects on Input Use

We start by testing the predictions of Result 1 in Section 2. In particular, Result 1 implies that the increase in crop share (s) for tenants in T1 should induce them to increase their input utilization while the increase in the output-independent income (y) of tenants in T2 should have no impact on their input use. In order to test these predictions, we use data from the tenant surveys that were conducted at the end of each season and recorded tenants' use of labor and capital inputs.

In terms of capital inputs, the tenants were asked to report the amount (if any) of any type of fertilizer and insecticide they used; and whether they acquired any agricultural tools during the past season.[29] Table 2 presents the effects of the treatment(s) on indicators of tenants' investments in capital inputs. Panel A of the table shows the effects on the extensive margin, while panel B presents the effect on the intensive margin (monetary value) of each input

[28] In particular, we find – by season – the group with highest attrition, and then delete – by season – observations with the highest (or lowest for the upper bounds) values in the other treatment groups until we have the same attrition rate as in the group with the highest attrition.

[29] All tenants were provided seeds by BRAC and, while they were free to use other seeds, only 13% of tenants reported using any seeds from another source, and this rate was not diffent across the treatment and control groups.

used.[30] In the first column, the outcome is any type of fertilizer use (either chemical or organic) by the tenants. Consistent with evidence from other East African settings (Duflo et al., 2011), fertilizer use was low among tenants in our sample. Only 28% of the tenants in the control group reported using any fertilizer on their plots. As a result of the higher crop share, tenants in T1 were 9.5 percentage points (ppt) more likely to use any type of fertilizer. This corresponds to a 34% increase relative to the control group. While this effect is large, it is not precisely estimated at conventional levels (p-value=0.174). Panel B shows that the intensive margin effect on fertilizer usage is even larger (in percentage terms) and precisely estimated. Tenants in T1 used on average USD 1.13 more fertilizer, which is 118% more compared to the average tenant in the control group. The corresponding effects for T2 are imprecisely estimated, although the point estimates are positive and not statistically different from the effects of T1. The test of equality between the treatment effects of T1 and T2 results in a p-value of 0.265 (0.280) for the extensive (intensive) margin of fertilizer use – reported at the lower section of each panel.

The second column of Table 2 displays the effects on insecticide use. In the control group, 28% of tenants reported using insecticide and spent on average USD 1.8 on it. Relative to the control, insecticide use was not significantly different among tenants in T1 or T2, neither on the extensive nor on the intensive margin. However, tenants in T1 spent significantly more on insecticide relative to tenants in T2 (p-value=0.038). The third column of the table shows that tenants in T1 were 9 ppt more likely to have purchased or acquired tools, and at the end of the season, the value of agricultural tools owned by the respondent was higher by USD 11 in T1 (30% relative to C). This latter effect is precisely estimated. We find no such effect for tenants in T2 and the difference between the coefficients of T1 and T2 is also statistically significant (p-value=0.059).

We have discussed the results of the treatment effect on a number of sub-categories of capital usage. Testing multiple hypothesis poses well-known challenges to the interpretation of p-values. We present results of two approaches to deal with these challenges. First, in the final column of Table 2 we use an aggregate index that combines the four indicators presented in the table. To construct this index, we first standardize each outcome into a z-score, by subtracting the control group mean at the corresponding survey round and dividing by the control group standard deviation. We then average all the z-scores, and again standardize to the control group. The results show that while there were no significant differences on the extensive margin, the tenants in T1 spent on average 0.2 standard deviations more on capital inputs compared to tenants in the control group. The corresponding effect for T2 tenants is -0.07 standard deviations and imprecisely estimated (the difference between T1 and T2 is significant with a p-value of 0.059). Second, we estimate the equations in columns 1 through 4 jointly, and then test the null hypothesis that a specified restriction holds in all estimating equations across columns. The results of these tests are consistent with what we found before when constructing an index: There is no robust evidence for an extensive margin effect. On the other hand, there is robust

[30] For fertilizer and insecticide used, the monetary value corresponds to the amount spent on the relevant input used for the experimental plot; while for tools the monetary value corresponds to the total value of agricultural tools that the tenant owned at the time of the survey.

evidence that tenants in T1 have more intensive usage of capital inputs (p-value=0.042). No such effects exists for tenants in T2. And the effect of treatment condition T1 on the intensive margin of capital usage is significantly different from the effect of treatment condition T2 at the 10% level (p-value=0.035).

Table A.11 in the Appendix reports bounds that adjust for differential attrition across the treatment groups. The results show that the effects on the intensive margin (of fertilizer and tools) are robust if we impute – within treatment groups – the mean minus (plus) up to 10% of a standard deviation multiple of the observed distribution of outcomes in that treatment group. However, they are not robust if we conduct the imputation with 20% of a standard deviation, or if we trim observations at the top of the distribution to equalize the attrition rates across the groups (i.e. the lower Lee bound). They should be interpreted with this caveat in mind.

Next, we estimate the effects on labor inputs. Tenants reported their own labor hours as well as any outside labor that they may have used on the plot, broken down into paid versus unpaid labor. Table 3 reports the results of estimating specification 4 where the outcomes are variables pertaining to labor inputs used on the plot. Column 1 shows that tenants in T1 and T2 did not spend more hours working on their plots relative to tenants in the control group nor relative to each other. Similarly, in column 2, we do not find any significant differences in terms of paid labor across the treatment groups. On the other hand, column 3 shows that tenants in T1 had more "unpaid workers" working on their plots. In particular, they used 8 more days of unpaid labor during the season.[31] Relative to the mean in the control (12.5 days/season) this corresponds to a 64% greater use of unpaid labor on the plot. The difference between T1 and T2 in terms of unpaid labor is also large (approximately 6 days) but statistically not significant at conventional levels (p-value=0.173).

To deal with the statistical challenges of multiple hypothesis testing we again follow the two approaches discussed above. The final column of the table we use an aggregate index that combines the three types of labor (own, paid and unpaid). The results show that the effect of T1 on this aggregate index is 0.2 standard deviation but imprecisely estimated at conventional levels (p-value=0.157) and the effect of T2 is 0.05 standard deviations, also imprecisely estimated (p-value=0.721). The difference between the two indices is insignificant (p-value=0.280). The same result is obtained when testing the corresponding cross-equation hypothesis.

Table A.12 in the Appendix shows that these effects are not likely to be driven by differential attrition – the magnitudes of both the lower and upper bounds under alternative assumptions about the attritors are similar to the unadjusted estimates.

Figure 2 provides a visual summary of the effects on input use. It plots the standardized effect size and the 90% confidence interval around the treatment effects for labor and capital inputs. The solid squares correspond to the effects of T1, while the hollow ones show the effect of T2 relative to control. Overall, the results show that the tenants in T1 have responded to the increase in their crop share by increasing their use of inputs – in particular fertilizer, tools and

[31] A further breakdown of labor shows that the effect is driven by a combination of family and friends helping with cultivation, results available from the authors upon request.

unpaid laborers – while the increase in the income of tenants in T2 had no such impact. These effects are perfectly in line with Result 1 of the framework: higher s increases input utilization, while higher y does not.

4.3 Effects on Risk-Taking

Result 2 of the conceptual framework implies that the increase in s or y may also affect tenants' level of risk-taking. The direction of the effect is in general ambiguous, as it depends on the shape of the tenants' utility function.

In general, it is difficult to test this prediction as often the researcher does not observe the risk associated with different input combinations. In our context, the type(s) of crop(s) the tenant chooses to cultivate provide a useful proxy for their risk-taking. There is a clear ranking in terms of riskiness of the different crops that BRAC offered seeds for. In particular, peanuts, tomatoes and maize are more sensitive to rainfall variation and exhibit greater output volatility in our data.[32] In Appendix Table A.3, we use two different approaches to demonstrate this. First, in Panel A, we exploit geographical variation among the plots cultivated by the control group of tenants to estimate the effect of rainfall throughout the season on the yield of each crop. Second, in Panel B, we use data from FAOStat on crop yields of countries across time in Sub-Saharan Africa.[33] Both approaches demonstrate that maize and peanut yields are particularly sensitive to rainfall, while beans are less sensitive. We cannot use the first approach for tomatoes or potatoes, since no tenant in the control group chose to cultivate these two crops, but the results from the second approach demonstrate that tomatoes are as sensitive to rainfall as peanuts.[34] To the extent that rainfall is a good proxy for aggregate income shocks, this implies that the return to maize, peanuts and tomatoes has a high uninsurable risk component, while for beans this is not the case. Although we are not able to measure well average returns on each crop, standard asset pricing theory would suggest that this might also imply a greater average return to investments in peanuts.

In order to test Result 2, we test if the increase of s in T1 or of y in T2 had any impact on tenants' incentives to grow certain crops more than others. To measure output of each crop, we use information from the crop assessment surveys during which the expected yield of each crop present on the plots was recorded pre-harvest.[35]

[32]This may not hold in other contexts. The FAO publication *Irrigation and Drainage Paper No. 33* relates yield to water intake using evapotranspiration as a main parameter, rather than rainfall. It reports maize and beans as sensitive to water deficit, while groundnuts are described as tolerant to water deficit. While these findings are different from ours with respect to beans and groundnuts, one should notice that they are not specific to East African cultivars and local crop management practices.

[33]Available from: `http://www.fao.org/faostat/en`

[34]As an alternative way to quantify the riskiness of these crops, we used the FAOStat data to calculate the coefficients of variation in the outputs of maize, beans, peanuts, tomatoes and potatoes. We did so using cross-country variation, as well as time variation within countries, and finally using both cross-country and time variation in the panel data. Table A.5 shows that the coefficients of variation for maize, beans and tomatoes are greater than those for beans.

[35]Each plot that was part of the experiment was visited by surveyors from an agricultural survey firm. They were trained to record which crops were cultivated on the plot and estimate the quantity of each crop that would be harvested. In order to calculate the monetary value of the output of a given crop, we use price data for each crop, collected at the local village market. While in theory it is possible that local prices may be affected by the

Table 4 presents the results of estimating specification 4 on these outcomes. Panel A of the table shows the effects on the extensive margin (whether the relevant crop was present on the plot at the time of the crop assessment survey) while Panel B reports the intensive margin (the value of the expected output of the relevant crop). The first row of Panel A shows that the tenants in T1 were significantly more likely to have maize and tomatoes on their plots compared to tenants in C. While the coefficients for beans and peanuts are also positive, they are not precisely estimated. When we compare the effect of T1 with T2, we find that the only crop that is significantly more likely to be present on T1 plots compared to T2 plots was tomatoes. Panel B shows that on the intensive margin, tenants in T1 produced more peanuts as well as tomatoes compared to tenants in C and T2. In particular, their expected output was USD 33 more for peanuts and USD 8 more for tomatoes, and these effects are significantly different from the corresponding effects of T2 (p-values of 0.065 and 0.074 respectively). As such, we can conclude that the increase in s led to greater risk-taking by tenants in T1, by inducing them to increase their cultivation of riskier crops (maize, peanuts and tomatoes) compared the the safer option (beans).[36]

Table A.13 provides attrition bounds for the effects on crop choice. While most bounds are similar to the main estimates, there are a few notable differences. The Lee lower bound for the intensive margin of peanuts is close to zero and imprecisely estimated; while for tomatoes the Lee lower bound for both the intensive and the extensive margins are zero. This is because we have a small sample, and most tenants do not grow any tomatoes and few grow peanuts. Therefore when we trim the observations on top of the distribution for both of these crops, we lose all or almost all of the positive observations.

4.4 Effects on Output

Having analyzed the effects on input use and risk-taking, we now test for the effect on the output that the tenants generated from the experimental plots. Result 3 specifies that as long as increases in s do not reduce tenant's risk-taking considerably (i.e. if $\frac{da}{ds}$ exceeds some negative bound), the aggregate impact on output should be positive. The effects on crop choice suggest that tenants in T1 did not decrease, but rather increased their risk-taking. As such, we expect to see higher output on their plots – both due to the increase in input levels and the change in the crop mix towards riskier but potentially higher return crops.

Table 5 presents the treatment effects on the total output (of all crops) that was observed on the plots during the pre-harvest crop assessment surveys.[37] Column 1 shows that the average

treatment assignment, in practice it is unlikely as the plots are small (0.5 acre on average) and therefore the crops harvested from the experimental plots make up only a very small fraction of the total output in each village. Hence, any general equilibrium effect on local prices are highly unlikely.

[36] An alternative explanation could be that tenants in T2 diversify their crop portfolio in order to lower output variability. This would be the case if different crops had negatively correlated expected yields, then the tenants could lower their risk exposure by intercropping them. Table A.5 shows that, among the control group, outputs of maize, beans and peanuts are not negatively correlated. If anything, the covariances are positive (imprecisely estimated). Moreover, as we show in the following section, tenants in T2 ended up having higher output variability relative to the control group. As such, a diversification strategy to insure against risks is unlikely to be driving the effects we observe on crop choice.

[37] Table A.9 in the Appendix shows the effects on self-reported output. The level of output is lower in all

tenant in the control group had an expected output of USD 93 (at PPP). Relative to that, tenants in T1 had USD 56 more expected yield on their plots. This implies that the 50% increase in their crop share (from 50% to 75%) increased their output by 60%. On the other hand, tenants in group T2 had USD 5 more output relative to C, but this is imprecisely estimated. Moreover, the difference between T1 and T2 is significant (p-value=0.024). Overall, these findings imply that the tenants who were given a higher crop share were more productive, and this was driven by the incentive effect as opposed to an expected income effect.[38]

Column 2 shows the effects for groups T2A and T2B separately. There is no significant difference between the coefficients of T2A and T2B. This implies that the risk profile of additional income does not play a significant role as T2A and T2B had similar effects on output. This reinforces the idea that the effect of treatment status T2 does capture any income effect induced by treatment condition T1. Nevertheless, it is important to note that the point estimates of T2A and T2B have different signs. Moreover, the difference between T1 and T2A is large (the magnitude of the point estimate for T1 is more than twice as large as that of T2A), but not statistically significant. This suggests that some tenants in T2A, who were promised a safe income transfer at the end of the season, may have generated higher output than the control tenants while for tenants with the risky income transfer (T2B) this was not the case. One potential explanation for this may be related to the effect of the increase in tenant's income on risk-taking, as captured by Result 2(ii) in Section 2. In fact, Table A.10 in the Appendix shows that the higher output in T2A is driven mainly by an increase in the cultivation of peanuts as opposed to the other crops. This suggests that some tenants in T2A may have increased their risk-taking, which is in line with the prediction of Result 2(ii).

The rest of Table 5 presents the effects on output per square meter. These regressions provide a robustness check of the functional form assumptions we implicitly made when estimating columns 1 and 2. We find that an increase in the tenant's share of output from 50% to 75% increases the value of his output by 0.073 USD (PPP) per square meter (p-value equal to 0.024 and 0.026, respectively). We find no income effect in the specification of column 3 where we do not differentiate between T2A and T2B tenants (p-value=0.993). When estimating the effect of T2A and T2B separately in column 4 we find a small positive effect of treatment condition T2A and a negative effect of T2B. None of the effects are significant at conventional levels. These effects are qualitatively similar to those on total output. This conclusion is perhaps unsurprising given that BRAC explicitly aimed to rent plots of roughly equal size, name half an acre.

In Table 5, output value is trimmed at the top so that the top 99% of each treatment group is coded to missing. Effects without trimming are reported in Table A.8 where we find an even

groups and while the signs of the point estimates are similar, the magnitudes are much smaller. This highlights the importance of using observed as opposed to self-reported information on output for our methodology.

[38]The finding that T2 tenants did not generate more output while T1 tenants did suggest that an efficiency wage story or a behavioral mechanism based on reciprocity are unlikely to be driving the effect of T1. If tenants in T1 were more productive because they received a better deal than they expected and wanted to work hard to reciprocate this favor (or to maintain it in the future), then we should see a similar effect on tenants who were given a cash transfer.

larger effect for being assigned to T1, and no significant effect of being assigned to T2. The larger effect of T1 in the non-trimmed results are driven by a handful of highly productive tenants in T1. Therefore, we rely on the trimmed observations as the main results. Table A.14 provides attrition bounds for the effects on output. Overall, the estimates are robust to different adjustments for differential attrition.

In Section 3.3 we discussed that the income transfer in T2 might have been different from the (pre-season expected) income effect of treatment condition T1.[39] Since the income transfer in T2 was determined at the branch level, there exists branch level variation in the ratio of the income transfer we did implement over the income transfer we should have implemented. In Table A.7 we exploit this variation to assess whether a mis-calibration of T2 could explain why we do not find any significant income effect. In particular, this table presents results of regressions analogous to 5, with the only exception that T2 is a continuous variable measuring the aforementioned ratio. We proxy the pre-season expected income effect of T1 by half the realised yield of tenants in the control group in the respective season, calculated at the branch level. To the extend that this is a suitable proxy, the ratio will be 1 in branches and seasons where the actual income transfer in treatment group T2 matches what we should have implemented. And it is proportionally higher (lower) in branches where the income transfer in treatment group T2 is higher (lower) than what we should have implemented. Under the assumption that the marginal income effect is constant, the coefficient on T2 will then estimate the true income effect of T1. The analogous statement holds for T2A and T2B. The results in Table A.7 indicate that our previous conclusions in Table 5 do still hold with this alternative definition of the treatment variable. In particular, we continue to find very similar, quantitatively large, significant effects of treatment condition T1 on output, even though the level of significance decreases somewhat. In contrast, we do not find any significant income effect on output levels.

Figure 3 shows the cumulative distribution function (CDF) for output in each treatment group. One can see that the CDF of output for tenants who were assigned the high-incentive contract (T1) lies to the left of the CDF for tenants with the standard contract (control group). This implies that the differences in average output levels reported above are not driven by a particular group of tenants responding to the high-incentive contract, but rather by an effect throughout the distribution, in particular from the median upwards. The figure also shows that tenants in T1 performed better than the tenants who were given a cash transfer (T2), which demonstrates that the effects are not driven by the increase in expected earnings. A summarized version of these findings is presented as box plot in Figure A.1.

Treatment effects are likely to be heterogenous, since tenants in T1 (and some tenants in T2) respond to the treatment by cultivating riskier crops with greater output variability (Sub-Section 4.3). Thus, we expect to see greater dispersion in the outputs generated by tenants

[39] An alternative experimental design would have been to link the income transfer in T2 to the season's realised yields in geographically close control clubs. This would have circumvented the challenges we faced in calibrating T2. That design requires to inform participants of the existence of other treatment conditions, which our design does not require. Whether this is an important advantage will depend on the specific setting.

in T1 relative to the control group or T2.[40] To test for heterogeneity in the treatment effects, we estimate quantile treatment effects (QTE) using the following specification:

$$Quant_\tau\,(y_{ict}) = \sum_{k=1}^{2} \beta_\tau^i T_{ik} + \phi_\tau \delta_s, \tag{5}$$

where y_{ict} is the output level of tenant i from club c in season t; T_{ik} is an indicator variable equal to 1 if tenant i belonged to a club of treatment group k and 0 otherwise and δ_s are strata fixed effects. One caveat to bear in mind is that, due to the small sample size, we have low power in estimating the treatment effects across the distribution.

Figure 4 displays the results. The QTE estimates reveal that there is considerable heterogeneity in the effects of incentives on the realized output levels: the effect on the 90th centile of output is 4 times more than the effect on the 50th centile. Moreover, while we observe no negative effect on output at any centile, the treatment effect at the lower centiles are indistinguishable from zero. As mentioned above, one reason behind this variation in the effect of receiving higher s may be due to its effect on risk-taking – tenants in T1, by cultivating riskier crops, expose themselves to greater output variability. On the other hand, the lower panel of Figure 4 reveals that tenants in the high-income group (T2) do not generate more output than the control group, at any decile.

Figure A.2 displays QTEs for the sub-group of tenants who received safe versus risky y (T2A vs. T2B) cash transfers. For the group of tenants with additional safe income (T2A) we observe positive point estimates of the treatment effect in the highest deciles. This is consistent with the idea that tenants in T2A take on more risk, as predicted in part (ii.) of Result 2 and found when analysing their crop choice. Receiving additional stochastic income (T2B) seems to have the opposite effect. Again this is consistent with the prediction of part (iii.) of Results 2: relative to safe income y, additional stochastic income will induce less risk-taking and might have a negative effect on risk-taking. It should be noted that these quantile treatment effects are estimated imprecisely, given the small sample size.

4.5 Welfare

Results presented above showed that tenants in the high-incentive group (T1) invested more in cultivating their rented plots and generated more revenue from them. A natural question is whether this was welfare-improving for them and their households. In particular, since we observe an increase in unpaid labor, in part driven by family labor, this raises the question of whether the increased labor activity on the plot crowded out other income-generating activities and reduced household earnings. To shed light on this, we estimate the impacts on respondent's and her household's economic wellbeing. Table 6 presents the results. The table shows that tenants in T1 did not have lower labor income, consumption, cash savings, household income or assets at the end of the experiment. If anything, column 3 shows that they had

[40] Another source of heterogeneity may be differences in tenants' innate abilities. More able tenants in T1 are likely to choose to work harder as they can earn more under the high-incentive contract (Lazaer, 2000). Even though we did not find a significant difference in terms of hours worked by T1 tenants relative to the control group (subsection 4.2), they may have exerted more effort during those hours.

higher household income and column 4 shows that they had more households assets (both marginally significant at 10% level) relative to C.[41] These findings imply that the high incentive contract did not crowd out any other productive activities. If anything, the evidence is in line with it increasing household income.[42]

While high tenant incentives may increase output and their households' economic well-being, they may have negative consequences for the environment. In particular, short-term, high-incentive contracts (such as those we study here) may lead the tenant to overwork the land (e.g. by overusing fertilizers) which may lead to environmental degradation. To test for such an effect, at the end of the experiment (i.e. at the end of the second experimental season) we collected soil samples from the plots that were part of the experiment, and tested their chemical composition. In particular, we measured the amount of Nitrogen, Phosphorous, Potassium, Organic matter, and the Ph-level of the sample. Table 7 shows the results of estimating the effects of the treatment(s) on these soil quality indicators. We do not find any significant differences in terms of soil quality of the plots in different treatment arms. While this suggests that the high incentive contract did not come at a cost to the soil quality in the short run, we cannot rule out long-run negative effects or changes in unobservable dimensions of soil quality.

5 Discussion

The results presented in this paper suggest that sharecropping contracts might be an important driver of low agricultural output. We find that contracts which specify that tenants pay 25% of output in compensation for the land induce 60% higher output than contracts which specify that tenants need to pay 50% of output in compensation for the land. The size of this output effect is large relative to what has been observed in previous studies. Exploiting within-household variation in the tenure status of plots in 8 Indian villages, and controlling for observable determinants of productivity (e.g soil quality), Shaban (1987) finds that owner-cultivated plots produce 16.3% more output compared to sharecropped plots. Laffont and Matoussi (1995) estimate, using farm-level data from Tunisia, that fixed rent tenancy or owner cultivation is associated with a 33% increase in output relative to sharecropping. Banerjee et al. (2002) evaluate the effects of a land reform that entitled registered tenants to 75% of their output and improved their tenure security, effectively improving the outside option of sharecroppers. They find that the reform increased sharecropper yields by 62%. While it is difficult to make direct comparison between these studies and ours (due to differences in methodologies and the underlying contractual changes), it is clear that the effect we find is an order of magnitude larger than all except Banerjee et al. (2002). There could be many potential explanations behind this difference, including the endogeneity of tenure status in previous studies (with the exception of Banerjee et al. (2002) where their methodology exploits variation in the placement and implementation of the land reform).

We find that tenants respond to higher incentive contracts both by purchasing further inputs,

[41] Findings in Table 2 showed that tenants in T1 were more likely to invest in tools for their plots. This may generate positive spillover effects on their households' other plots, which may explain the larger effect on their household income relative to their personal labor income.

[42] Table A.15 displays bounds for differential attrition for the effects reported in Table 6.

and by taking on more risk. To the extent that either of these responses is having externalities, such responses might not be socially optimal. For example, the tenants might be depleting the lands' soil quality. We do not find any such evidence, but we cannot exclude that unmeasured negative effects do exist. To the extent that a tenant's actions has pecuniary externalities on crop prices and insurance markets are incomplete, the optimal level of private risk taking might also be different from the socially optimal level of risk taking.

It should also be noted that we do not find strong evidence that tenants increase their level of 'effort'. Partly this might reflect that the 'effort' metaphor in traditional moral hazard models stands for factors that are non-observable (and therefore non-contractable). If these factors are truly unobservable by the landlord, they might also not be observable to us as researchers. In our view a more suitable interpretation is that contracts are not written contingent on these factors because the cost of observing those factors is high. In the end, at some cost many important dimensions of agricultural practise are in fact observable.[43] However, these informational costs might be prohibitively high. (As researchers, a large fraction of our research budget was spent on conducting high-intensity pre-harvest land-measurements, crop assessment surveys and soil tests.) Additionally, contracts which are contingent on the specific use of inputs might be particularly difficult to enforce.

Lastly we find that the effects of high-incentive sharecropping contracts on agricultural input choices and output are largely to be interpreted as an incentive effect. This is an important insight for the design of optimal policies. It suggests that policies that seek to increase agricultural productivity need to effectively increase the tenants' share in output. Note that this might be achieved through many means: for example, land redistribution, regulatory reform of tenancy contracts, improvements to the tenants' outside option, or advances in the provision of insurance might all end up increasing the tenants' share in output. Note that some of these policies might additionally have adverse effects. However, to the extent that they effectively increase the tenants' share in output they will likely unleash the incentive effect described in this paper. In contrast, policies that increase the tenants' income in a way that is independent of his output level are unlikely to trigger the same type of output response.

In interpreting these findings it is important to keep in mind that the effects of sharecropping contracts could be different in other settings. The setting in which we conducted the experiment is special, relative to poor rural areas elsewhere in the world, in at least three ways. First, the experiment reported in this paper was conducted in an area where agricultural productivity is particularly low. These conditions imply that there was ample scope for change in the tenants' behaviour. That said, increasing agricultural productivity is of particular interest for policy makers precisely where agricultural productivity is low. Second, the tenants in our experiment were relatively young women. Even though BRAC provided them with intensive training before the experiment, it might be that their level of knowledge about efficient farm-

[43]When calculating residual measure of TFP at the plot level, following the methodology of Restuccia and Santaeulalia-Llopis (2017), we find no significant impact of receiving higher s (or higher y) on this measure (results available upon request). This suggests that the changes in the observed input levels are sufficient to explain a seizable share of the increase in the output level resulting from higher s.

ing techniques was more limited than the knowledge that an experienced farmer would have. In our view this is likely to induce a lower effect of sharecropping contracts on behaviour, since the known scope of possible responses is more limited within our set of tenants. And third, sharecropping contracts are by no means common in rural Uganda. To the extent that this implies that the tenants are imperfectly aware of the functioning of sharecropping contracts, this would again imply a muted response toward contractual changes relative to a situation where sharecropping contracts are well understood. However, the fact that sharecropping contracts are largely absent in Uganda might also be the consequence of underlying differences between rural Uganda and other areas where sharecropping contracts are more prevalent. If such differences are related to the elasticity of tenant responses towards changes in s – as would for example be the case if the underlying agricultural production function is different – our findings are unlikely to be externally valid.

6 Conclusion

The question of how output sharing rules affect economic agents' incentives for investment and risk-taking is central to contract theory and public economics. In the context of agricultural tenancy contracts, the idea that a tenant who has to share a large part of her output with the landowner will have little incentive to invest in cultivation has been long established. Yet, the empirical evidence on this is scant. There is also limited evidence on the effects of such an arrangement on tenant's incentives for risk-taking.

In this paper, we presented results from a field experiment in Uganda aimed at testing for the incentive effects of tenancy contracts. In particular, we collaborated with the NGO BRAC to induce random variation in sharecropping contracts that they signed with young, female tenants. The standard contract entitled tenants to keep 50% of their output. We randomly selected a subset of the tenants to keep 75% of their output. A third group was kept at 50% crop share but received an income transfer (i.e. a fixed wage).

We find that tenants who received a higher crop share invested more in inputs, especially capital (fertilizer and tools). Moreover, they showed a higher propensity to take risks by choosing to cultivate more rain-sensitive crops with greater output variation (maize, tomatoes, peanuts) as opposed to a safer option (beans). As a result of these changes, they produced more output. In particular, tenants whose crop share was increased from 50% to 75% produced 60% more output. On the other hand, tenants who were kept at 50% and given a cash grant did not experience any significant change in their utilization of inputs, and as a result did not produce a higher output compared to the control group. Overall, the findings support the notion that the productivity effect of having a higher crop share is due to moral hazard, and not due to the effect of having a higher expected income.

Our findings lend support to the idea that tenancy reforms that increase the crop share of tenants are likely to improve agricultural output. The magnitude of the effect on output is large. Moreover, the fact that tenants who retain a larger share of their output start growing riskier crops provides a test of the work in public finance which seeks to understand the effect of taxation on entrepreneurial risk-taking.

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Tables

Table 1: DESCRIPTIVES CHARACTERISTICS OF THE TENANTS AND BALANCE TESTS

	(1)	(2)	(3)	(4)	(5)
		Difference between			
	C	T1-C	T2-C	T1 - T2	N
Young (Age $\leq$ 21)	0.557	-0.044	0.027		262
	(0.500)	(0.075)	(0.074)	(0.318)	
		[0.552]	[0.721]	[0.313]	
Low Schooling ($\leq$ 8 years)	0.550	-0.028	0.005		265
	(0.501)	(0.082)	(0.075)	(0.657)	
		[0.731]	[0.946]	[0.671]	
School enrolment	0.089	-0.010	-0.038		264
	(0.286)	(0.045)	(0.037)	(0.499)	
		[0.823]	[0.317]	[0.519]	
Raven test score (0-100)	51.543	2.881	5.015		269
	(24.952)	(3.294)	(3.468)	(0.510)	
		[0.419]	[0.159]	[0.527]	
Health status (0-10)	8.111	0.190	0.044		269
	(1.643)	(0.225)	(0.229)	(0.420)	
		[0.391]	[0.865]	[0.418]	
Married	0.512	-0.004	-0.029		268
	(0.503)	(0.083)	(0.078)	(0.743)	
		[0.962]	[0.718]	[0.761]	
Number of children	1.750	-0.197	-0.026		268
	(1.710)	(0.235)	(0.255)	(0.473)	
		[0.426]	[0.906]	[0.484]	
Labor income	29.280	3.789	-5.722		264
	(38.096)	(6.689)	(5.361)	(0.119)	
		[0.574]	[0.270]	[0.123]	
Cash savings	122.170	-13.296	-7.878		266
	(145.514)	(19.947)	(21.600)	(0.808)	
		[0.523]	[0.725]	[0.826]	
Consumption	142.583	10.993	-17.312		261
	(91.177)	(14.517)	(12.328)	(0.037)**	
		[0.495]	[0.187]	[0.062]*	
Household size	5.346	-0.213	0.010		269
	(2.001)	(0.327)	(0.267)	(0.451)	
		[0.488]	[0.970]	[0.431]	
Household sex ratio	0.425	-0.041	-0.002		269
	(0.208)	(0.032)	(0.030)	(0.217)	
		[0.174]	[0.957]	[0.212]	
Household income	194.626	10.802	-3.591		235
	(171.870)	(25.435)	(24.462)	(0.523)	
		[0.666]	[0.872]	[0.550]	
Household assets	1,506.931	-273.941	-518.401		265
	(2,714.334)	(380.210)	(332.138)	(0.384)	
		[0.480]	[0.135]	[0.409]	
Agricultural tools	49.121	-6.763	-3.493		265
	(33.042)	(4.770)	(4.292)	(0.475)	
		[0.178]	[0.422]	[0.499]	

Notes: The table shows the differences in baseline characteristics of tenants assigned to treatment and control groups. Each row is based on a regression of the covariate on dummy variables for treatment status, controlling for strata fixed effects. The standard errors in parentheses are clustered at the club level. In square brackets we provide the randomization inference p-value of a test of the null hypothesis that C, $T1-C$, $T2-C$ and $T1-T2$ is equal to 0, respectively. "Young" is a dummy variable equal to 1 if respondent's age is below the sample median, which is 21 years old. "Low schooling" is a dummy variable equal to 1 if respondent's years of schooling is below the sample median, which is 8 years of schooling. "School enrolment" is a dummy variable equal to 1 if the respondent was enrolled in school at time time of the baseline survey. "Health status" is the self-reported health status of the respondent, on a scale between 0 and 10. "Raven test score" is the percentage of correct answers that the respondent had in a Raven Matrices test. "Household size" is the number of people living in the respondent's household. "Household sex ratio" is the fraction of respondent's household members who are female. "Labor income" is the average monthly labor income of the respondent during the 12 months preceding the survey. "Consumption" is the monthly consumption expenditure of the respondent; it is calculated by adding her monthly personal consumption on non-food items and services with her household's per-capita food consumption where monthly food consumption is imputded from previous 2 days' recall."Cash savings" is the value of savings that the respondent has at the time of the survey. "Household income" is the response to the question "What is the total income of your household in a typical month?". "Household assets" is the monetary value of durable assets owned by the respondent's household. "Agricultural tools value " is the value of agricultural tools that the tenant had. All monetary values are in PPP USD.

Table 2: EFFECTS ON CAPITAL INPUTS

	Fertilizer	Insecticide	Tools	Index
	(1)	(2)	(3)	(4))
Panel A:		*Extensive Margin*		
High s (T1)	0.095	-0.010	0.086	0.202
	(0.061)	(0.052)	(0.055)	(0.133)
	[0.174]	[0.866]	[0.123]	[0.157]
High y (T2)	0.021	-0.071	0.007	-0.066
	(0.059)	(0.054)	(0.053)	(0.138)
	[0.767]	[0.216]	[0.901]	[0.661]
Within-Equation Test				
H_0: T1 = T2	0.265	0.255	0.142	0.059
Cross-Equations Test				
H_0: T1 = 0		0.286		-
H_0: T2 = 0		0.550		-
H_0: T1 = T2		0.323		-
Mean Outcome (C)	0.277	0.276	0.500	0.000
Observations	432	423	432	423
Panel B:		*Intensive Margin (USD)*		
High s (T1)	1.13*	0.43	11.36**	0.434***
	(0.55)	(0.51)	(5.04)	(0.152)
	[0.065]	[0.418]	[0.039]	[0.007]
High y (T2)	0.53	-0.53	1.59	0.016
	(0.42)	(0.47)	(4.32)	(0.124)
	[0.246]	[0.259]	[0.727]	[0.887]
Within-Equation Test				
H_0: T1 = T2	0.280	0.038	0.059	0.008
Cross-Equations Test				
H_0: T1 = 0		0.042		-
H_0: T2 = 0		0.308		-
H_0: T1 = T2		0.035		-
Mean Outcome (C)	0.96	1.81	37.81	0.000
Observations	419	413	427	402

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Cross-Equations Tests report the randomization inference p-value for a test of the hypothesis that the specified restriction holds in all estimating equations across columns. Within-Equation Tests report the randomization inference p-value for a test of the specified compound hypothesis. In Panel A, "Fertilizer (Insecticide)" is a dummy variable equal to 1 if the tenant said she used fertilizer (insecticide) on her plot during the past season; "Tools" is a dummy variable equal to 1 if the tenant said she bought agricultural tools to cultivate her plot. In Panel B, the dependent variable is the monetary value of the input used in PPP USD terms. For agricultural tools, the intensive margin is the vaue of agricultural tools owned by the respondent's household at the time of the survey. The "Index" combines the four indicators by first standardizing each outcome into a z-score (by subtracting the control group mean at the corresponding survey round and dividing by the control group standard deviation), then takes the average of the z-scores, and again standardizes to the control group.

Table 3: EFFECTS ON LABOR INPUTS

	Own labor	Paid	Unpaid	Index
	(hours/week)	*(days/season)*		
	(1)	(2)	(3)	(4)
High s (T1)	0.34	-0.05	8.02*	0.20
	(1.28)	(1.98)	(4.03)	(0.12)
	[0.781]	[0.982]	[0.065]	[0.157]
High y (T2)	-0.03	1.06	1.79	0.05
	(1.22)	(2.08)	(3.31)	(0.12)
	[0.984]	[0.628]	[0.626]	[0.721]
Within-Equation Test				
H_0: T1 = T2	0.783	0.550	0.173	0.280
Cross-Equations Test				
H_0: T1 = 0		0.277		-
H_0: T2 = 0		0.909		-
H_0: T1 = T2		0.575		-
Mean Outcome (C)	17.13	4.28	12.54	-0.00
Observations	417	432	432	417

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Cross-Equations Tests report the randomization inference p-value for a test of the hypothesis that the specified restriction holds in all estimating equations across columns. Within-Equation Tests report the randomization inference p-value for a test of the specified compound hypothesis. "Own labor" is the number of hours that the tenant said she worked on the plot in a typical week during the past season. The dependent variables in columns 2 and 3 are the number of worker-days of paid and unpaid labor respectively that the tenant said she had working on the plot for throughout the season. The "Index" combines the three indicators by first standardizing each outcome into a z-score (by subtracting the control group mean at the corresponding survey round and dividing by the control group standard deviation), then takes the average of the z-scores, and again standardizes to the control group.

Table 4: EFFECTS ON CROP CHOICE

	Maize (1)	Beans (2)	Peanuts (3)	Tomatoes (4)	Potatoes (5)	Others (6)
Panel A:			*Extensive Margin*			
High s (T1)	0.112**	0.049	0.055	0.021***	0.012	0.000
	(0.047)	(0.042)	(0.040)	(0.010)	(0.008)	(0.037)
	[0.025]	[0.253]	[0.212]	[0.008]	[0.201]	[0.997]
High y (T2)	0.090*	0.032	0.049	-0.001	0.002	-0.016
	(0.048)	(0.041)	(0.038)	(0.004)	(0.003)	(0.040)
	[0.084]	[0.447]	[0.239]	[0.805]	[0.686]	[0.712]
H_0: T1 = T2	0.652	0.720	0.899	0.013	0.217	0.728
Mean Outcome (C)	0.620	0.300	0.327	0.000	0.000	0.140
Observations	479	479	479	479	479	479
Panel B:			*Intensive Margin*			
High s (T1)	4.51	5.40	32.77***	7.67*	0.27	28.55
	(4.85)	(6.17)	(11.04)	(4.23)	(0.24)	(34.42)
	[0.384]	[0.389]	[0.003]	[0.051]	[0.447]	[0.473]
High y (T2)	-2.43	1.78	4.72	-0.25	0.05	-11.47
	(4.40)	(6.84)	(9.38)	(1.89)	(0.11)	(31.39)
	[0.591]	[0.820]	[0.655]	[0.917]	[0.814]	[0.754]
H_0: T1 = T2	0.152	0.613	0.065	0.074	0.318	0.403
Mean Outcome (C)	28.43	15.78	22.44	0.00	0.00	58.61
Observations	479	479	479	479	479	479

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionally the randomization inference p-value of a test of the null hypothesis that the effect of T1 and T2 are equal is provided for all estimating equations. Dependent variables in Panel A are dummy variables equal to 1 if at the time of the pre-harvest crop assessment survey, any of the following crops were observed on the plot: maize in column (1), beans in column (2), peanuts in column (3), tomatoes in column (4), potatoes in column (5) and other types of crops in column (5). In Panel B, the dependent variable is the expected output of the relevant crop on the plot. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets. All monetary values are in PPP USD.

Table 5: EFFECTS ON OUTPUT

	$\mathbb{E}$[Yield]		$\mathbb{E}$[Yield]/m^2	
	(1)	(2)	(3)	(4)
High s (T1)	56.11***	55.92***	0.073**	0.073**
	(18.33)	(18.40)	(0.030)	(0.031)
	[0.004]	[0.004]	[0.024]	[0.026]
High y (T2)	5.42		-0.000	
	(16.93)		(0.030)	
	[0.762]		[0.993]	
High y, safe (T2A)		18.00		0.043
		(25.48)		(0.048)
		[0.541]		[0.405]
High y, risky (T2B)		-6.84		-0.043
		(15.64)		(0.031)
		[0.652]		[0.207]
H_0: T1 = T2	0.024		0.046	
H_0: T1 = T2A		0.214		0.592
H_0: T1 = T2B		0.001		0.002
H_0: T2A = T2B		0.347		0.123
Mean Outcome (C)	93.43	93.43	0.171	0.171
Observations	473	473	473	473

Notes: The table reports ordinary least square estimates based on specification (4) at the plot level, for both Season 1 and Season 2. $\mathbb{E}$[Yield] is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. $\mathbb{E}$[Yield]/m^2 is the expected output of the plot divided by the area (in square meters) cultivated. Values are in PPP USD. T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. T2A and T2B indicate subgroups of treatment group 2 (T2). T2A received a fixed income transfer, and T2B received a stochastic income transfer, with mean equal to T2A. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionaly randomization inference p-values for the specified compound hypotheses are reported.

Table 6: WELFARE

	Labor income (1)	Consumpt. (2)	Cash savings (3)	Household income (4)	Household assets (5)
High s (T1)	4.07	4.43	56.83	33.04*	656.54*
	(7.33)	(9.60)	(35.39)	(18.34)	(332.13)
	[0.626]	[0.678]	[0.127]	[0.076]	[0.060]
High y (T2)	14.98*	-3.98	66.12	0.49	183.46
	(8.35)	(7.84)	(39.27)	(18.04)	(209.29)
	[0.086]	[0.652]	[0.102]	[0.982]	[0.396]
H_0: T1 = T2	0.214	0.372	0.852	0.064	0.164
Mean Outcome (C)	36.65	115.34	143.63	181.80	1242.61
Observations	424	421	427	398	427

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionally the randomization inference p-value of a test of the null hypothesis that the effect of T1 and T2 are equal is provided for all estimating equations. All monetary values are in PPP USD terms. "Labor income" is the average monthly labor income of the respondent during the 12 months preceding the survey. "Consumption" is the monthly consumption expenditure of the respondent; it is calculated by adding her monthly personal consumption on non-food items and services with her household's per-capita food consumption where monthly food consumption is imputed from previous 2 days' recall."Cash savings" is the value of savings that the respondent had at the time of the survey. "Household income" is the response to the question "What is the total income of your household in a typical month?". "Household assets" is the value of durable assets owned by the respondent's household.

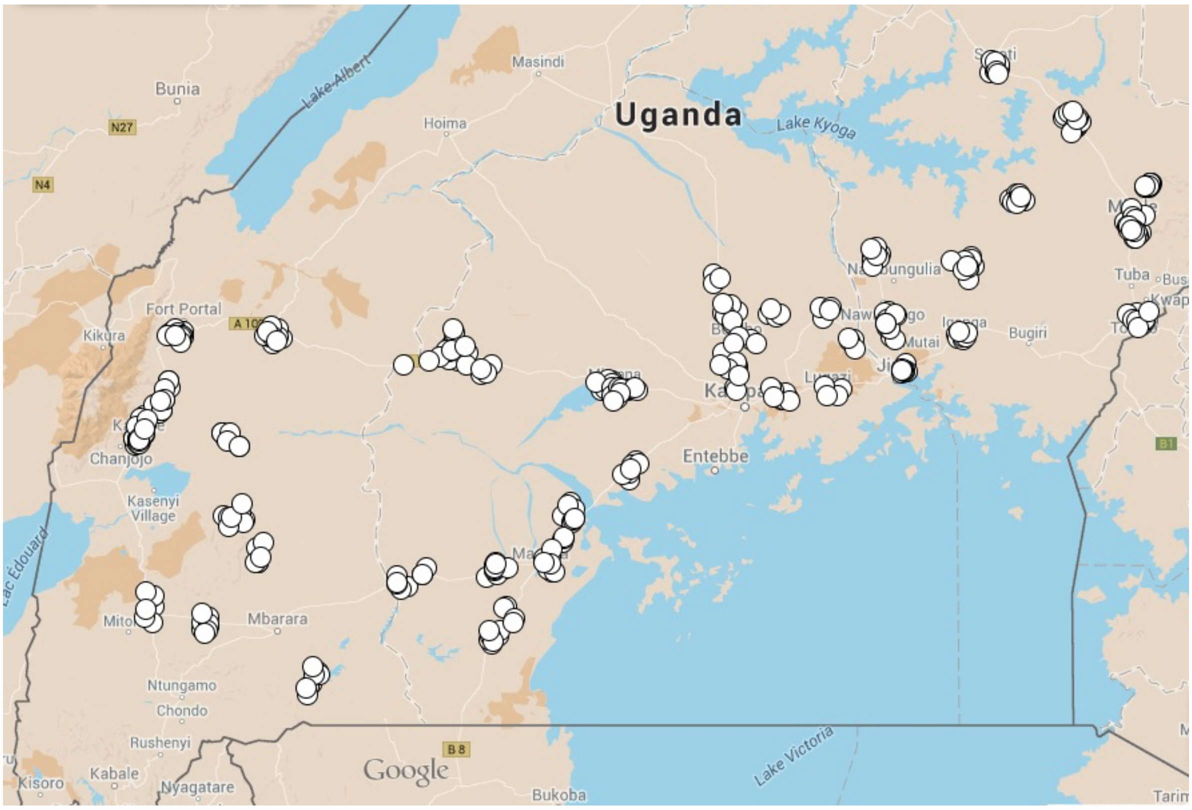
Table 7: SOIL QUALITY AT THE END OF THE EXPERIMENT

	N	K	P	Org. M.	Ph	Index
	(1)	(2)	(3)	(4)	(5)	(6)
High s (T1)	-0.11	-0.00	0.06	-0.06	0.05	-0.04
	(0.08)	(0.05)	(0.11)	(0.09)	(0.12)	(0.13)
	[0.216]	[0.975]	[0.598]	[0.515]	[0.685]	[0.793]
High y (T2)	-0.00	-0.02	0.10	0.01	-0.01	0.07
	(0.08)	(0.05)	(0.11)	(0.09)	(0.12)	(0.12)
	[0.993]	[0.711]	[0.369]	[0.912]	[0.917]	[0.574]
Within-Equation Test						
H_0: T1 = T2	0.185	0.760	0.779	0.476	0.592	0.441
Cross-Equations Test						
H_0: T1 = 0			0.711			-
H_0: T2 = 0			0.959			-
H_0: T1 = T2			0.797			-
Mean Outcome (C)	2.29	0.77	2.33	2.11	5.21	-0.00
Observations	324	322	323	321	324	318

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Cross-Equations Tests report the randomization inference p-value for a test of the hypothesis that the specified restriction holds in all estimating equations across columns. Within-Equation Tests report the randomization inference p-value for a test of the specified compound hypothesis. The dependent variables are the results of soil tests conducted on sampled of soil taken from the plots that were part of the experiment. For Nitrogen (N) the index is: 1=lack, 2=inadequate, 3=adequate; for Potassium (K): 0=deficient, 1=sufficient; for Organic Matter: 1=low, 2=high, 3=very high; for Phosphorous (P): 1=very low, 2=moderate, 3=adequate, 4=high. For Ph-level the index report the phd level of the soil sample.

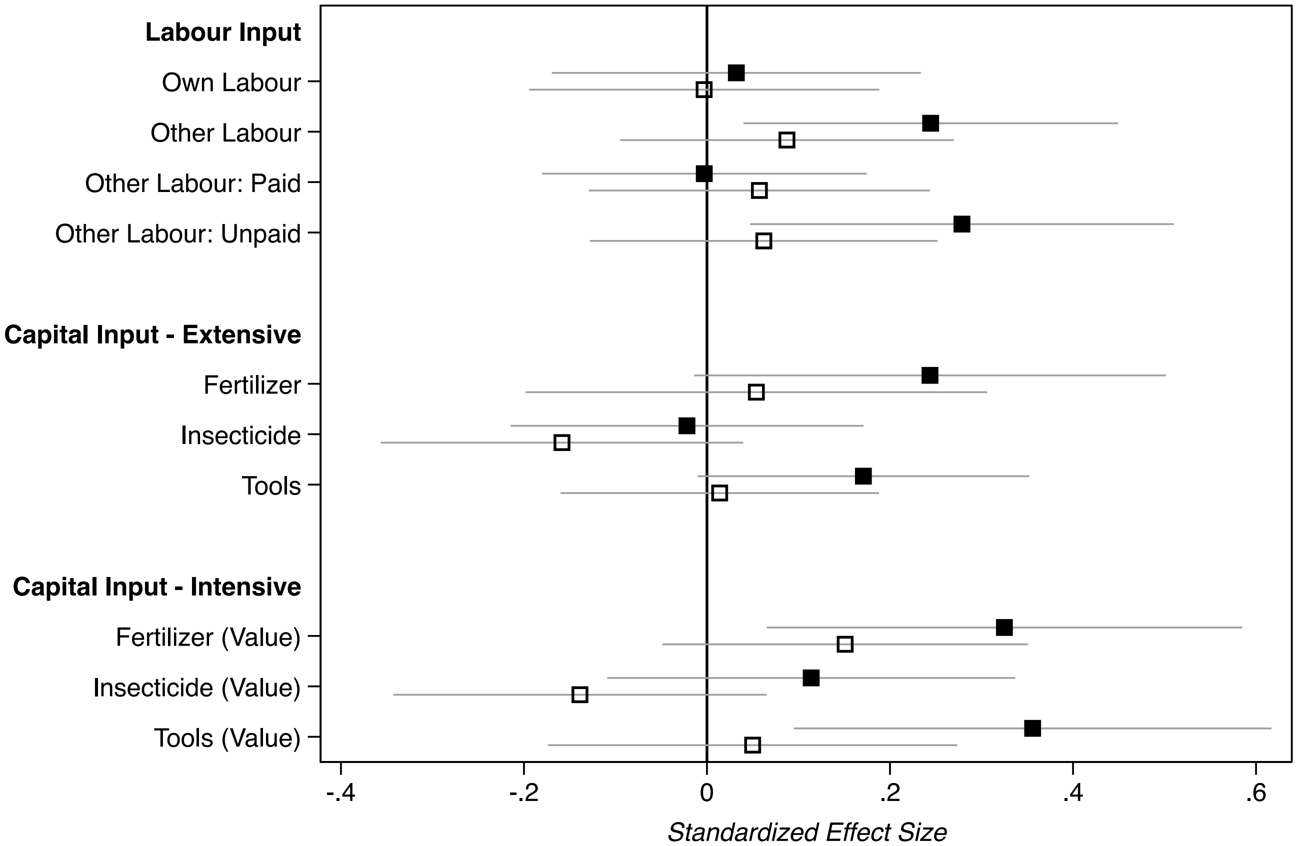
Figures

Figure 1: Location of the Plots



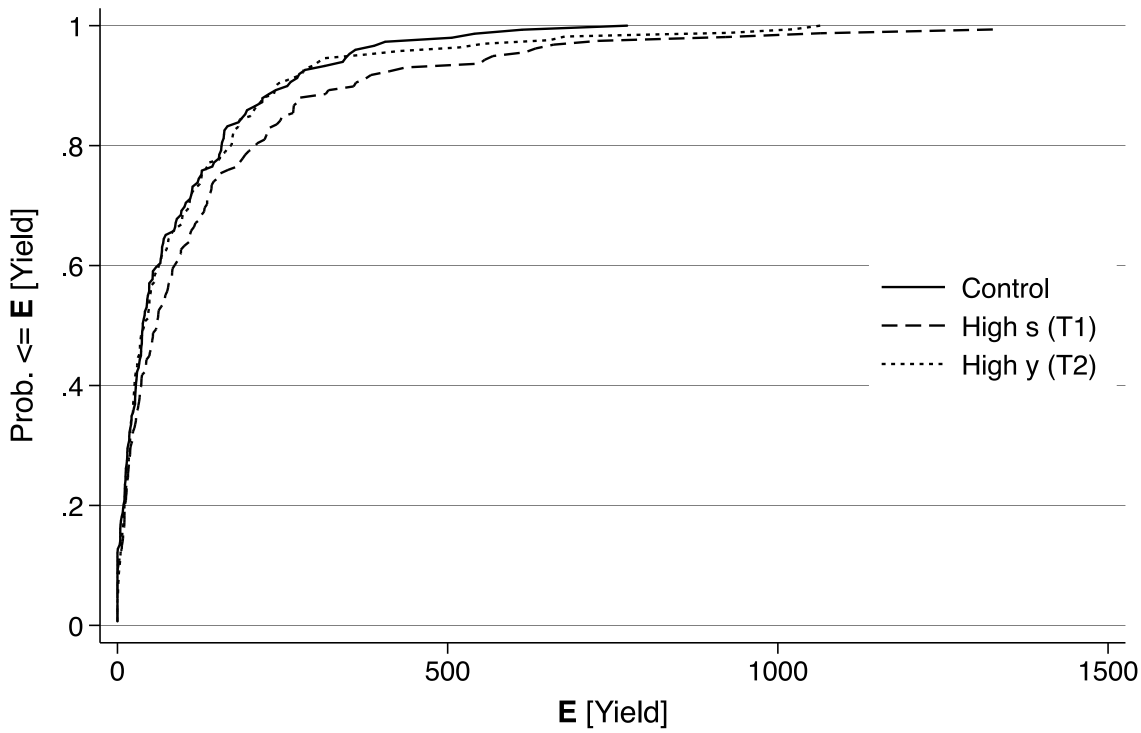
Notes: Figure shows the location of clubs that were selected by BRAC to participate in the experiment.

Figure 2: Contracts and Input Choice



Notes: Figure plots the standardized effect sizes and 95% confidence intervals for labor and capital inputs used for cultivation. The solid squares show the effects of being selected to receivehigh (75%) crop share (T1) relative to the control group, while the hollow squares show the effect of receiving the same crop share as the control group (50%) plus an additional cash transfer (T2). The effects are estimated using ordinary least square estimates based on specification (4). All specifications control for strata fixed effects and standard errors are clustered at the club level. For capital inputs, the extensive margins correspond to dummy variables equal to 1 if the tenant used any fertilizer; any insecticide; if she bought any agricultural tools to cultivate her plot. The intensive margins are the monetary value of the inputs used in PPP USD. For irrigation, the intensive margin is the amount of money the tenant spent on improving the irrigation of the plot; for tools, it gives the value of agricultural tools that the tenant had at the time of the survey.

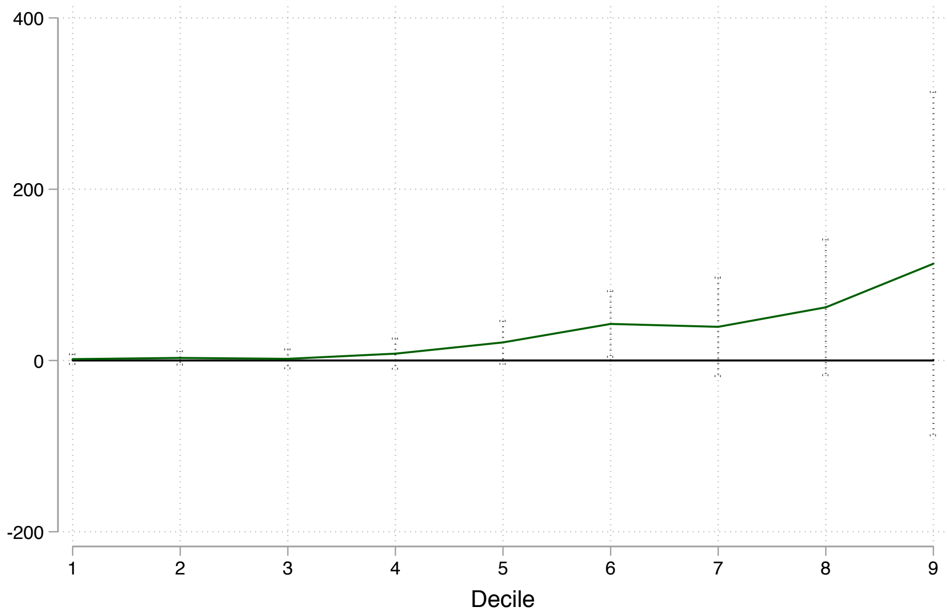
Figure 3: Contracts and Distribution of $\mathbb{E}$[Yield]



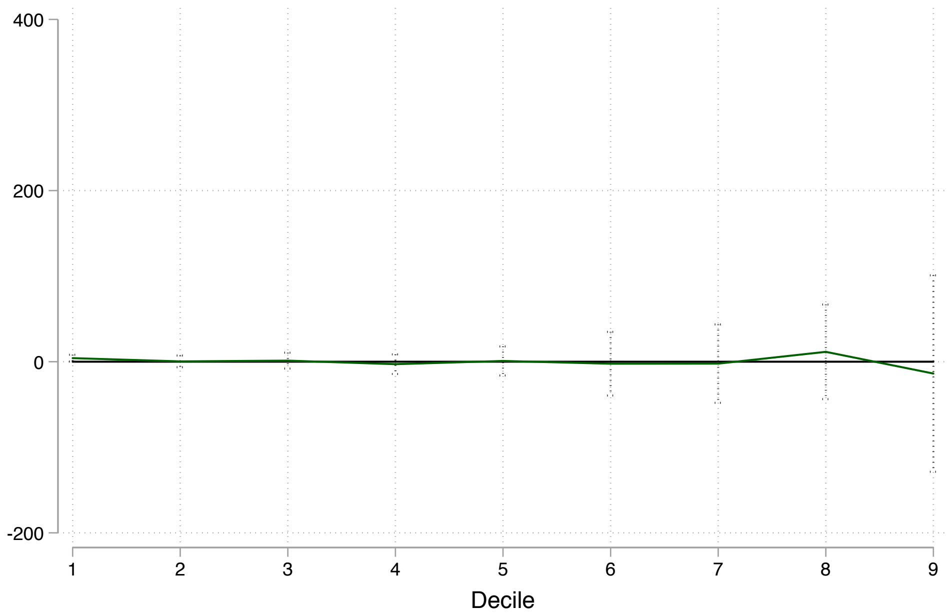
Notes: Figure plots the cumulative distribution function of expected yield from the plots, by treatment status. Tenants in T1 are those who were randomized to receive high (75%) crop share, tenants in T2 received the same crop share as control (50%) and an additional cash transfer. $\mathbb{E}$[Yield] is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. Values are in PPP USD.

Figure 4: Heterogeneity of Output Effects

(a) Quantile Treatment Effects of T1 vs. C



(b) Quantile Treatment Effects of T2 vs. C



Notes: Figure plots quantile treatment effects and 90% confidence intervals based on bootstrapped (with 500 replications) standard errors clustered at the club level (unit of randomization). Each specification controls for the randomization strata. $\mathbb{E}$[Yield] is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. Values are in PPP USD.

ONLINE APPENDIX:
MORAL HAZARD: EXPERIMENTAL EVIDENCE FROM TENANCY CONTRACTS

Konrad Burchardi, Selim Gulesci, Benedetta Lerva, Munshi Sulaiman

A Theory and Proofs

A.1 Decomposition of dx/ds

To understand the role of the endogenous adjustment of risk-taking for Result 1, consider the case where the level of risk-taking is given exogenously and does not adjust, i.e. $\frac{da}{ds} = 0$. We can decompose $\frac{dx}{ds}|_a$ into three separate effects:

$$\underbrace{\frac{-1}{D_{xx}} \int u_{cc} \cdot a(\theta - \mathbb{E}_\theta[\theta]) f(x) \cdot c_x(x,\theta)\, dF(\theta)}_{\text{Risk Exposure Effect}} + \underbrace{\frac{-1}{D_{xx}} \int u_{cc} \cdot (a\mathbb{E}_\theta[\theta] + (1-a)) f(x) \cdot c_x(x,\theta)\, dF(\theta)}_{\text{Wealth Effect}} + \underbrace{\frac{-1}{D_{xx}} \int u_c \cdot [a\theta f_x(x) + (1-a) f_x(x)]\, dF(\theta),}_{\text{Incentive Effect}}$$

where $c_x(x,\theta) := [s[a\theta f_x(x) + (1-a) f_x(x)] - p]$ and

$$D_{xx} = \int u_{cc} \cdot [sa\theta f_x(x) + s(1-a) f_x(x) - p]^2 + u_c \cdot [sa\theta f_{xx}(x) + s(1-a) f_{xx}(x)]\, dF(\theta) < 0$$

since $u_{cc} < 0$ and $f_{xx} < 0$. Firstly, an increase in the crop share of the tenant increases the marginal return to investments, holding marginal utilities constant. It is the only effect of a change in s on x when the tenant is risk-neutral, or the Bernoulli utility function is linear. However, when tenants are risk averse, a change in s alters the marginal expected utility of investing in x through two more channels. It makes the tenant on average wealthier ("Wealth Effect"). With decreasing marginal utility that implies that in states of the world where $\theta > 1$ the tenant values additional consumption less, and in states of the world where $\theta < 1$ the tenant has lower disutility from losses in consumption. Further, an increase in s will also amplify any deviations in returns around the mean incurred from the risky investment ("Risk Exposure Effect"). This alters the expected marginal benefit of investing in x in a generally unknown direction. The total effect on the incentives to invest in x depends on the curvature of the utility function.

A.2 Proofs

Proof of Result 1. Part i. Note that a, $f(x)$ and s are positive constants in the integration. Then (3) can be written as $\int u_c \cdot [\theta - 1]\, dF(\theta) = 0$, which implies with (2) that $\int u_c \cdot [s f_x(x) - p]\, dF(\theta) =$

0. Since $u_c > 0$, this is satisfied if and only if $sf_x(x) - p = 0$. Totally differentiating we obtain $\frac{dx}{ds} = -\frac{f_x(x)}{sf_{xx}(x)}$. Noting that $f_x(x) > 0$ and $f_{xx}(x) < 0$ completes the proof.

Part ii. We find $\frac{dx}{dy}$ by totally differentiating (2) and (3) with respect to x, a and y as: $\frac{dx}{dy} = -\frac{D_{xy} \cdot D_{aa} - D_{xa} \cdot D_{ay}}{D_{xa} \cdot D_{ax} - D_{xx} \cdot D_{aa}}$. Using the result $sf_x(x) = p$, and noting that a, s, $f_x(x)$ and $f_{xx}(x)$ are constants in the integrals, it is straightforward to show that the denominator is strictly negative, and the numerator is 0. □

Proof of Result 2. Part i. Totally differentiate (2) and (3). Rearranging gives $\frac{da}{ds} = -\frac{D_{as}D_{xx} - D_{xa}D_{xs}}{D_{aa}D_{xx} - D_{xa}D_{ax}}$. Simplify the denominator to $sf_{xx}(x)(sf(x))^2 \int u_c[a\theta + (1-a)]\, dF(\theta) \times \int u_{cc}[\theta - 1]^2\, dF(\theta) > 0$, where the inequality follows from $f_{xx}(x) < 0$ and $u_{cc} < 0$. We can then write, using $sf_x(x) = p$ throughout:

$$\frac{da}{ds} = (-D_{aa})^{-1} \cdot \left(\underbrace{\frac{asf(x)}{f_{xx}(x)} \left(f_x(x) f_{xx}(x) - sf(x)(f_x(x))^2 \right) \left[\int u_{cc} \cdot [\theta - 1]^2\, dF(\theta) \right]}_{\text{Component 1}} + \underbrace{s(f(x))^2 \left[\int u_{cc} \cdot [\theta - 1]\, dF(\theta) \right]}_{\text{Component 2}} \right)$$

Notice that $-D_{aa} > 0$. Further Component 1 is negative since $f_{xx}(x) < 0$ and $u_{cc}(c) < 0$. The sign of Component 2 is determined by $-\int u_c(-\frac{u_{cc}}{u_c})[\theta - 1]\, dF(\theta)$, where $-\frac{u_{cc}}{u_c}$ is the coefficient of absolute risk aversion. Under CARA the coefficient of absolute risk aversion is a multiplicative constant in the integration, and we know from (3) that $\int u_c \cdot [\theta - 1]\, dF(\theta) = 0$. Therefore this term drops out, and since all other terms are negative we have $\frac{da}{ds} < 0$ under CARA. If $u(.)$ instead exhibits DARA, the term $-\int u_c(-\frac{u_{cc}}{u_c})[\theta - 1]\, dF(\theta)$ is positive, since relative to CARA, the coefficient of absolute risk aversion gives higher "weight" to realisations of θ s.t. $\theta < 1$. Examples can be constructed to s.t. $\frac{da}{ds}$ is smaller, equal and bigger than zero.[44]

Part ii. We find $\frac{da}{dy}$ by totally differentiating (2) and (3) with respect to x, a and y as: $\frac{da}{dy} = \frac{D_{ay} \cdot D_{xx} - D_{xy} \cdot D_{ax}}{D_{xa} \cdot D_{ax} - D_{xx} \cdot D_{aa}}$. The denominator is negative. The numerator simplifies to $\int u_{cc} sf(x)[\theta - 1]\theta\, dF(\theta) \cdot \int u_c f_{xx}(x)[sa\theta + s(1-a)]\, dF(\theta)$. Notice that the latter integral is unambiguously negative by concavity of $f(x)$. The former integral can be written as $-\int \left(-\frac{u_{cc}}{u_c}\right) u_c sf(x)[\theta - 1]\, dF(\theta)$, where $-\frac{u_{cc}}{u_c}$ is the coefficient of absolute risk aversion.

By (3) we have $\int u_c[\theta - 1]\, dF(\theta)$ equals zero. With CARA utility this immediately implies the result $\frac{da}{dy} = 0$. If $u(.)$ instead exhibits DARA, the term $-\int sf(x)u_c(-\frac{u_{cc}}{u_c})[\theta - 1]\, dF(\theta)$ is positive, since relative CARA, the coefficient of absolute risk aversion gives higher "weight" to realisations of θ s.t. $\theta < 1$. Combining all sign results, we have that $\frac{da}{dy} > 0$ for any utility function that exhibits DARA.

Part iii. For the purpose of this proof, denote θ as θ_a, write the exogenous income as $\theta_y y$, and denote with $F(\theta_a, \theta_y)$ the joint cumulative distribution function of θ_a and θ_y. Further assume that θ_a and θ_y are independent and $\mathbb{E}\left[\theta_y\right] = \mathbb{E}\left[\theta_y | \theta_a\right] = 1$; these are realistic representations of

[44] For example, make the following assumptions: θ is taking a value of 0.8 and 1.3 with probability 0.5 each; $u(c) = \frac{c^{1-\rho}}{1-\rho}$, with $\rho = 10$; $f(x) = \log(x) + 5$; $p = 5$; $s = 0.5$. Assuming that $y = 0.0$, we have $\frac{da}{ds} > 0$; assuming that $y = 0.2$, we have $\frac{da}{ds} < 0$.

experimental group T2A. Following the same steps as in *Part ii*, we find that the sign of $\frac{da}{dy}$ is determined by the sign of $-\int u_{cc} s f(x)\theta_y[\theta_a - 1]\, dF(\theta_a, \theta_y) \cdot \int u_c f_{xx}(x)[sa\theta_a + s(1-a)]\, dF(\theta_a, \theta_y)$. Again the latter part is negative, and the former part can be written as $\int u_c \left(-\frac{u_{cc}}{u_c}\right) sf(x)\theta_y[\theta_a - 1]\, dF(\theta_a, \theta_y)$. Under CARA $(-u_{cc}/u_c)$ is constant. By the first order conditions we have $\int u_c[\theta_a - 1]\, dF(\theta_a, \theta_y) = 0$. Note that $\int u_c[\theta_a - 1]\, dF(\theta_y|\theta_a) > \int u_c\theta_y[\theta_a - 1]\, dF(\theta_y|\theta_a)$, since θ_y acts to re-weight relative to the expression in the first order condition and $u(c)$ is concave. Therefore $0 = \int u_c[\theta_a - 1]\, dF(\theta_a, \theta_y) = \int\int u_c[\theta_a - 1]\, dF(\theta_y|\theta_a)\, dF_{\theta_y}(\theta_a) > \int\int u_c\theta_y[\theta_a - 1]\, dF(\theta_y|\theta_a)\, dF_{\theta_y}(\theta_a) = \int u_c\theta_y[\theta_a - 1]\, dF(\theta_a, \theta_y)$. If $u(.)$ instead exhibits DARA examples can be constructed examples can be constructed to s.t. $\frac{da}{ds}$ is smaller, equal and bigger than zero. □

Proof of Result 3. Part i. Expected output is $\mathbb{E}_\theta = \int [a\theta_a + (1-a)] f(x)\, F(\theta_a)$. It is straightforward to calculate the total differential of $\mathbb{E}_\theta$ and derive:

$$\frac{d\mathbb{E}_\theta}{ds} = (\mathbb{E}_\theta[\theta] - 1)\frac{da}{ds} + (a\mathbb{E}_\theta[\theta] + (1-a))\, f_x(x)\frac{dx}{ds}.$$

This implies that $\frac{d\mathbb{E}_\theta}{ds} > 0$, if the following condition is satisfied:

$$\frac{da}{ds} > -f_x\frac{dx}{ds}\left[\frac{a\mathbb{E}_\theta[\theta] + (1-a)}{\mathbb{E}_\theta[\theta - 1]}\right].$$

Part ii. Follows from Results 1 and 2. □

B List of variables

Outcome variables:

- *Fertilizer* – A dummy variable taking the value of one if the tenant said she used fertilizer on her plot during the past season. The intensive margin gives the monetary value of fertilizer that was used on the plot in PPP USD terms.
- *Insecticide* – A dummy variable taking the value of one if the tenant said she used insecticide on her plot during the past season. The intensive margin gives the monetary value of insecticide that was used on the plot in PPP USD terms.
- *Tools* – A dummy variable taking the value of one if the tenant said she bought agricultural tools to cultivate her plot. The intensive margin gives the monetary value of agricultural tools owned by the respondent's household at the time of the survey in PPP USD terms.
- *Own labor* – Respondents were asked to report how many days they worked on the plot in a typical week of the past season, and how many hours they worked for in a typical day. The variable combines these two pieces of information to calculate the number of hours that the tenant said she worked on the plot in a typical week during the past season.

- *Paid (unpaid) labor* – For each person who worked on the plot (other than the respondent), respondents were asked to report the number of months they worked on the plot during the last season; how many days per month they worked on the plot and whether they were paid or unpaid. The variable combines these pieces of information to calculate the number of worker-days of paid (unpaid) labor that the tenant said she had working on the plot for throughout the season.
- *Crop choice outcomes* – Dummy variables taking the value of one if at the time of the pre-harvest crop assessment survey, any of the following crops were observed on the plot: maize, beans, peanuts, tomatoes, potatoes or any other types of crops. The intensive margin of each crop gives the expected output of the relevant crop (in PPP USD) on the plot. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets.
- $\mathbb{E}$*[Yield]* – The expected output of the plot (in PPP USD) measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops.
- $\mathbb{E}[Yield]/m^2$ – The expected output of the plot divided by the area (in square meters) cultivated.
- *Labor income* – Average monthly labor income (in PPP USD) of the respondent during the 12 months preceding the survey.
- *Consumption* – The monthly consumption expenditure (in PPP USD) of the respondent. It is the sum of the respndent's monthly personal consumption on non-food items and services with her household's per-capita food consumption. Household per capita monhtly food consumption is imputed from previous 2 days' recall. The respondent's non-food personal expenditure includes the following items: clothes, shoes, phone airtime, transportation, jewelry/ornaments, hairdressing, soda, alchohol, gifts.
- *Cash savings* – The value (in PPP USD) of cash savings that the respondent had at the time of the survey.
- *Household income* – Response to the question "What is the total income of your household in a typical month?", converted to PPP USD terms.
- *Household assets* – The value (in PPP USD) of durable assets owned by the respondent's household at the time of the survey.

Baseline variables:

- *Young* – A dummy variable taking the value of one if respondent's age is below the sample median, which is 21 years old.
- *Low schooling* – A dummy variable taking the value of one if respondent's years of schooling is below the sample median, which is 8 years of schooling.

- *School enrolment* – A dummy variable taking the value of one if the respondent was enrolled in school at time time of the baseline survey.
- *Health status* – The self-reported health status of the respondent, on a scale between 0 and 10.
- *Raven test score* – The percentage of correct answers that the respondent had in a Raven Matrices test.
- *Household sex ratio* – The fraction of respondent's household members who are female.
- *Agricultural tools* – The monetary value of agricultural tools owned by the respondent's household at the time of the survey in PPP USD terms.

C Additional Results

C.1 Attrition Analysis

Table A.1: ATTRITION SEASON 1

Attrition in:	*Crop A. Survey*		*Tenants Survey*	
	(1)	(2)	(3)	(4)
High s (T1)	-0.053	-0.053	-0.034	-0.033
	(0.052)	(0.052)	(0.054)	(0.054)
	[0.315]	[0.313]	[0.531]	[0.542]
High y (T2)	0.001		-0.038	
	(0.052)		(0.051)	
	[0.994]		[0.466]	
High y, safe (T2A)		0.010		-0.076
		(0.068)		(0.056)
		[0.874]		[0.189]
High y, risky (T2B)		-0.009		0.000
		(0.066)		(0.068)
		[0.881]		[1.000]
H_0: T1 = T2	0.341		0.921	
H_0: T1 = T2A		0.385		0.415
H_0: T1 = T2B		0.546		0.623
H_0: T2A = T2B		0.804		0.304
Mean Outcome (C)	0.245	0.245	0.204	0.204
Observations	304	304	304	304

Notes: The table reports ordinary least square estimates based on specification (4). The sample includes all tenants who signed a tenancy contract with BRAC at the beginning of Season 1. The dependent variable is an indicator variable that is equal to 1 if no pre-harvest crop assessment survey was conducted (in columns 1 and 2) or no Follow-Up survey was conducted (in columns 3 and 4) for that tenant at the end of Season 1. T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. T2A and T2B indicate subgroups of treatment group 2 (T2). T2A received a fixed income transfer, and T2B received a stochastic income transfer, with mean equal to T2A. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionaly randomization inference p-values for the specified compound hypotheses are reported.

Table A.2: ATTRITION SEASON 2

Attrition in:	*Crop A. Survey*		*Tenants Survey*	
	(1)	(2)	(3)	(4)
High s (T1)	-0.002	-0.002	-0.107	-0.106
	(0.065)	(0.065)	(0.068)	(0.068)
	[0.969]	[0.968]	[0.138]	[0.145]
High y (T2)	0.009		-0.070	
	(0.063)		(0.069)	
	[0.892]		[0.351]	
High y, safe (T2A)		0.004		-0.125
		(0.081)		(0.087)
		[0.962]		[0.180]
High y, risky (T2B)		0.014		-0.015
		(0.079)		(0.085)
		[0.859]		[0.875]
H_0: T1 = T2	0.846		0.579	
H_0: T1 = T2A		0.939		0.846
H_0: T1 = T2B		0.842		0.277
H_0: T2A = T2B		0.921		0.295
Mean Outcome (C)	0.367	0.367	0.469	0.469
Observations	304	304	304	304

Notes: The table reports ordinary least square estimates based on specification (4). The sample includes all tenants who signed a tenancy contract with BRAC at the beginning of Season 1. The dependent variable is an indicator variable that is equal to 1 if no pre-harvest crop assessment survey was conducted (in columns 1 and 2) or no Follow-Up survey was conducted (in columns 3 and 4) for that tenant at the end of Season 2. T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. T2A and T2B indicate subgroups of treatment group 2 (T2). T2A received a fixed income transfer, and T2B received a stochastic income transfer, with mean equal to T2A. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionaly randomization inference p-values for the specified compound hypotheses are reported.

C.2 Crop Risk Profile

Table A.3: CROP SENSITIVITY TO RAINFALL

	Maize	Beans	Peanuts	Tomatoes	Potatoes
	(1)	(2)	(3)	(4)	(5)
Panel A:	*Crop Sensitivity to Rainfall in the Control Group*				
L(rainfall)	2.536***	0.496	2.692**	0.000	0.000
	(0.926)	(1.007)	(1.080)	(.)	(.)
Observations	150	150	150	150	150
Panel B:	*Crop Sensitivity to Rainfall in Sub-Saharan Africa*				
L(Rainfall)	0.212***	0.023	0.084*	0.093*	0.005
	(0.066)	(0.042)	(0.049)	(0.052)	(0.038)
Observations	2358	683	2245	1752	1697

Notes: In Panel A, 'L(rainfall)' is log precipitation in mm during the season in a cell of size 10 km^2 that contains the plot. The sample is restricted to the control group with 50% crop share. All specifications control for strata fixed effects. Standard errors are clustered at the club level and *** (**) (*) indicates significance at the 1% (5%) (10%) level. Dependent variables are the expected output of the relevant crop on the plot: maize in column (1), beans in column (2), groundnuts in column (3), tomatoes in column (4) and potatoes in column (5). It is calculated by multiplying the quantity of output of crops reported, multiplied by the price of the relevant crop (as measured on local markets). All values are then converted to PPP USD terms. In Panel B, dependent variable is the log of annual crop yield (tonnes) in a country. 'Rainfall' is log annual precipitation in mm. Crop yield data are from FAOStat. Weather data is from University of Delaware. Sample includes all Sub-Saharan African countries with recorded yield for a given crop in the data. All specifications control for country and year fixed effects. Standard errors are clustered at the country level.

Table A.4: Crop Variability in FAO Data

	Maize (1)	Beans (2)	Peanuts (3)	Tomatoes (4)	Potatoes (5)
cross-section	0.597	0.489	0.535	0.694	0.580
time-series	0.335	0.191	0.253	0.236	0.293
panel	0.655	0.543	0.546	0.752	0.623

Notes: The table provides the coefficient of variation of the crop yield at the country level. Crop yield data are from FAOStat. Sample includes all Sub-Saharan African countries with recorded yield for a given crop in the data. The first row provides the average annual coefficient of variation across countries, the second row gives the country-level average coefficient of variation of the crop yield within countries, the third row gives the coefficient of variation in the full panel.

Table A.5: Covariance of Crop Yields in the Control Group

	Maize (1)	Maize (2)	Beans (3)
Beans	0.071		
	(0.138)		
Peanuts		0.052	0.009
		(0.050)	(0.039)
Observations	150	150	150

Notes: The table provides the correlations of crop yields for maize, beans and peanuts in the control group with 50% crop share. In column (1), expected yield of maize is regressed on the expected yield of beans; and in column (2) on expected yield of peanuts. In column (3), expected yield of beans is regressed on expected yield of peanuts. All specifications control for strata fixed effects. *** (**) (*) indicates significance at the 1% (5%) (10%) level.

C.3 Additional Output Results

Table A.6: EFFECTS ON OUTPUT, SEASON 1

	$\mathbb{E}$[Yield]		$\mathbb{E}$[Yield]/m^2	
	(1)	(2)	(3)	(4)
High s (T1)	90.52***	90.31***	0.105*	0.104*
	(30.84)	(30.92)	(0.056)	(0.056)
	[0.006]	[0.006]	[0.076]	[0.079]
High y (T2)	27.89		-0.002	
	(28.00)		(0.050)	
	[0.34]		[0.967]	
High y, safe (T2A)		60.96		0.065
		(38.80)		(0.077)
		[0.15]		[0.449]
High y, risky (T2B)		-4.16		-0.067
		(26.66)		(0.055)
		[0.887]		[0.252]
H_0: T1 = T2	0.046		0.074	
H_0: T1 = T2A		0.505		0.683
H_0: T1 = T2B		0.002		0.009
H_0: T2A = T2B		0.104		0.161
Mean Outcome (C)	120.99	120.99	0.214	0.214
Observations	262	262	262	262

Notes: The table reports ordinary least square estimates based on specification (4) at the plot level using the sample of Season 1 observations only. $\mathbb{E}$[Yield] is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. $\mathbb{E}$[Yield]/m^2 is the expected output of the plot divided by the area (in square meters) cultivated. Values are in PPP USD. T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. T2A and T2B indicate subgroups of treatment group 2 (T2). T2A received a fixed income transfer, and T2B received a stochastic income transfer, with mean equal to T2A. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionaly randomization inference p-values for the specified compound hypotheses are reported.

Table A.7: EFFECTS ON OUTPUT - CONTINUOUS T2

	$\mathbb{E}$[Yield]		$\mathbb{E}$[Yield]/m^2	
	(1)	(2)	(3)	(4)
High s (T1)	49.88**	49.95**	0.071*	0.071*
	(19.50)	(19.53)	(0.035)	(0.035)
	[0.025]	[0.025]	[0.065]	[0.066]
High y (T2)	-0.24		-0.000	
	(0.33)		(0.000)	
	[0.736]		[0.929]	
High y, safe (T2A)		0.02		-0.000
		(0.37)		(0.000)
		[0.980]		[0.947]
High y, risky (T2B)		-0.51		-0.001
		(0.26)		(0.000)
		[0.556]		[0.739]
Observations	409	409	409	409

Notes: The table reports ordinary least square estimates based on specification (4) at the plot level. It is constructed the same way as Table 5, with the exception of how we construct the variables T2, T2A and T2B. Denote with m^C_{bs} the median of the value of output of plots in the control group in season s in branch b. The variable T2, T2A and T2B take on the value $(m^C_{b0} \times 0.25)/(m^C_{bs} \times 0.25)$ for Season $s \in \{1, 2\}$ for a tenant/plot randomized to be part of T2, T2A and T2B, respectively, and zero otherwise. The numerator of the ratio is the value of actual (expected) payments to T2 tenants, and the denominator is the value of (expected) payments to T2 tenants that would ex-post correspond to the pure treatment effect of T1 in Season s. All specifications control for strata fixed effects. The number of observations is smaller relative to Table 5 since m^C_{bs} does not exist or is zero for some b and $s, s \geq 1$. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionaly randomization inference p-values for the specified compound hypotheses are reported.

Table A.8: EFFECTS ON OUTPUT - WITHOUT TRIMMING

	Not truncated at 99th percentile			
	$\mathbb{E}$[Yield]		$\mathbb{E}$[Yield]/m^2	
	(1)	(2)	(3)	(4)
High s (T1)	90.992*** (27.223) [0.000]	90.654*** (27.391) [0.000]	0.114*** (0.047) [0.008]	0.114*** (0.047) [0.009]
High y (T2)	-0.813 (20.058) [0.963]		-0.010 (0.034) [0.822]	
High y, safe (T2A)		22.581 (31.750) [0.526]		0.012 (0.058) [0.860]
High y, risky (T2B)		-23.946 (21.063) [0.275]		-0.032 (0.037) [0.440]
H_0: T1 = T2	0.004		0.017	
H_0: T1 = T2A		0.173		0.243
H_0: T1 = T2B		0.000		0.001
H_0: T2A = T2B		0.239		0.576
Mean Outcome (C)	97.182	97.182	0.182	0.182
Observations	479	479	479	479

Notes: The table reports ordinary least square estimates based on specification (4) at the plot level, for both season 1 and season 2. $\mathbb{E}$[Yield] is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. $\mathbb{E}$[Yield]/m^2 is the expected output of the plot divided by the area (in square meters) cultivated. Values are in PPP USD. The only difference from Table 5 is that the outcome variable is not trimmed at the 99th percentile. T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. T2A and T2B indicate subgroups of treatment group 2 (T2). T2A received a fixed income transfer, and T2B received a stochastic income transfer, with mean equal to T2A. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionaly randomization inference p-values for the specified compound hypotheses are reported.

Table A.9: SELF-REPORTED OUTPUT

	Yield		Yield/m^2	
	(1)	(2)	(3)	(4)
High s (T1)	1.04	1.07	-0.00	-0.00
	(8.81)	(8.82)	(0.00)	(0.00)
	[0.925]	[0.926]	[0.333]	[0.335]
High y (T2)	-10.39		-0.01	
	(7.75)		(0.00)	
	[0.253]		[0.200]	
High y, safe (T2A)		-12.02		-0.01
		(9.24)		(0.00)
		[0.253]		[0.180]
High y, risky (T2B)		-8.56		-0.00
		(10.81)		(0.01)
		[0.496]		[0.501]
H_0: T1 = T2	0.258		0.816	
H_0: T1 = T2A		0.266		0.657
H_0: T1 = T2B		0.485		0.929
H_0: T2A = T2B		0.814		0.663
Mean Outcome (C)	43.41	43.41	0.02	0.02
Observations	396	396	395	395

Notes: The table reports ordinary least square estimates based on specification (4) at the plot level, for both season 1 and season 2. $\mathbb{E}$[Yield] is the value of output of the plot, as reported by the tenants in the post-harvest survey. It is calculated by multiplying the quantity of output of crops reported with the price of the relevant crop measured on local markets and summing over crops. $\mathbb{E}$[Yield]/m^2 is the output of the plot, as reported by the tenant, divided by the area (in meters-squared) of the plot. This is the only difference to Table 5, were the yield measure is calculated from the Crop Assessment data. All monetary values are in PPP USD terms. T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. T2A and T2B indicate subgroups of treatment group 2 (T2). T2A received a fixed income transfer, and T2B received a stochastic income transfer, with mean equal to T2A. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionaly randomization inference p-values for the specified compound hypotheses are reported.

Figure A.1: Contracts and $\mathbb{E}$[Yield]

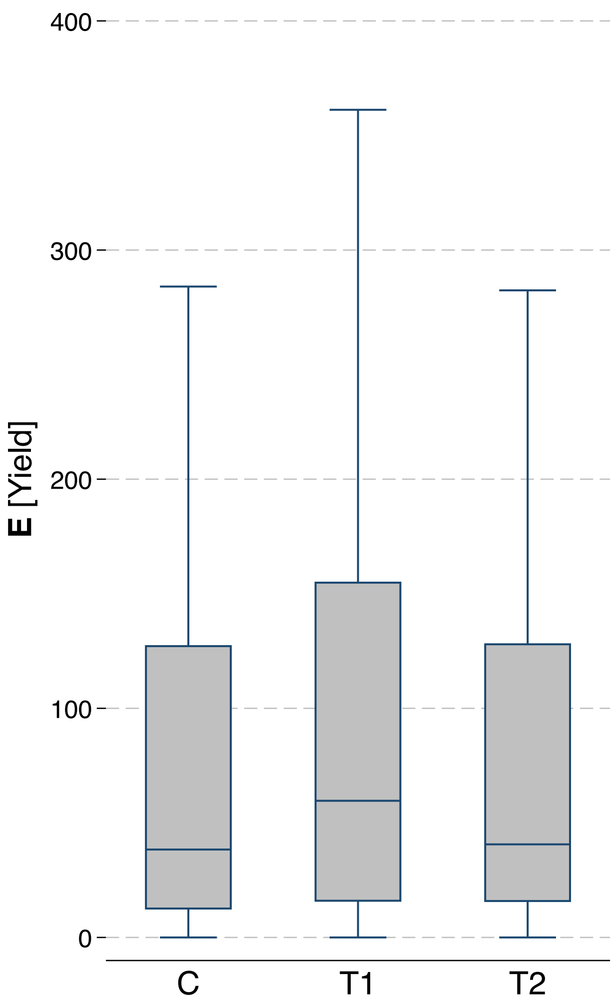
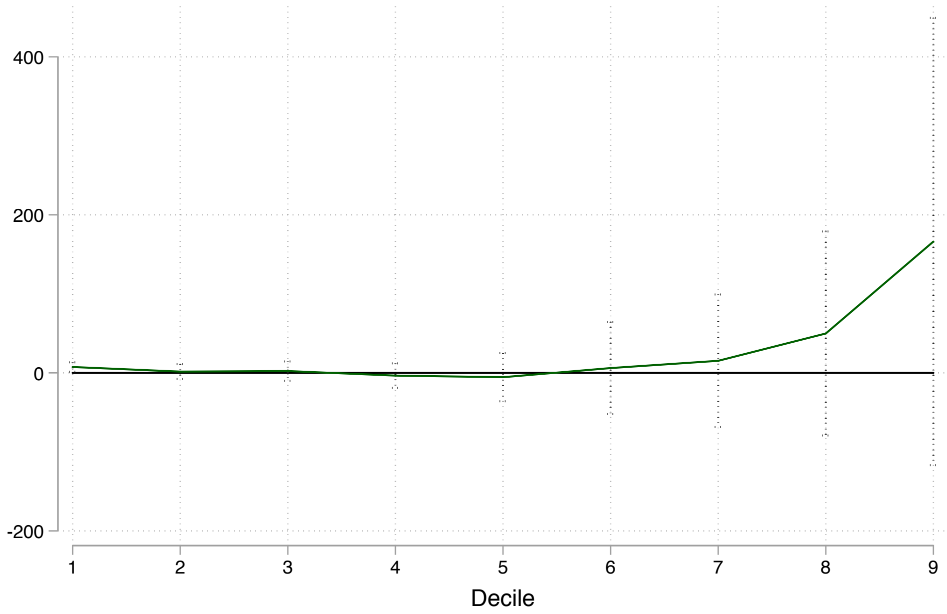
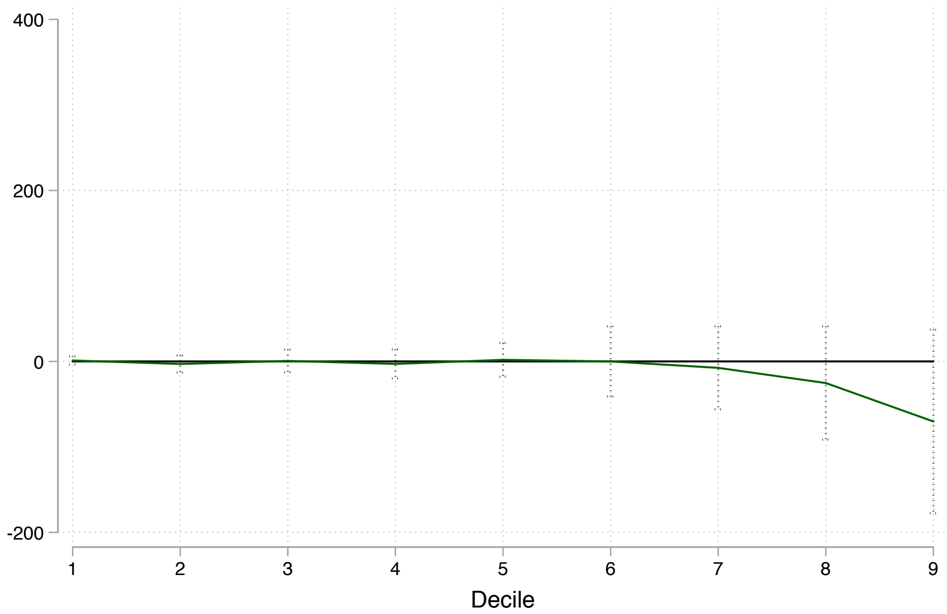


Figure A.2: Heterogeneity of Impact, Safe vs Risky Income y

(a) Quantile Treatment Effects of T2A



(b) Quantile Treatment Effects of T2B



Notes: Figure plots quantile treatment effects and 90% confidence intervals based on bootstrapped (with 500 replications) standard errors clustered at the club level (unit of randomization). Each specification controls for the randomization strata. $\mathbb{E}$[Yield] is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. Values are in PPP USD.

Table A.10: EFFECTS ON CROP CHOICE

	Maize (1)	Beans (2)	Peanuts (3)	Tomatoes (4)	Potatoes (5)	Others (6)
Panel A:			*Extensive Margin*			
High s (T1)	0.112**	0.049	0.055	0.022***	0.012	-0.000
	(0.047)	(0.042)	(0.040)	(0.010)	(0.008)	(0.038)
	[0.026]	[0.249]	[0.212]	[0.008]	[0.197]	[0.994]
High y, safe (T2A)	0.081	0.012	0.073	-0.008	0.005	0.010
	(0.059)	(0.052)	(0.050)	(0.008)	(0.004)	(0.059)
	[0.198]	[0.840]	[0.165]	[0.468]	[0.199]	[0.874]
High y, risky (T2B)	0.099	0.052	0.025	0.005	-0.001	-0.042
	(0.062)	(0.051)	(0.049)	(0.005)	(0.005)	(0.048)
	[0.132]	[0.324]	[0.655]	[0.338]	[0.930]	[0.404]
H_0: T1 = T2A	0.606	0.518	0.776	0.029	0.122	0.881
H_0: T1 = T2B	0.828	0.965	0.626	0.001	0.184	0.408
H_0: T2A = T2B	0.812	0.556	0.511	0.242	0.388	0.517
Mean Outcome (C)	0.620	0.300	0.327	0.000	0.000	0.140
Observations	479	479	479	479	479	479
Panel B:			*Intensive Margin*			
High s (T1)	4.54	5.30	32.56***	7.69*	0.26	28.95
	(4.85)	(6.16)	(10.94)	(4.25)	(0.24)	(34.57)
	[0.381]	[0.393]	[0.003]	[0.050]	[0.450]	[0.471]
High y, safe (T2A)	-4.57	8.82	19.61*	-1.87	0.11	-39.00
	(5.81)	(11.35)	(10.39)	(3.35)	(0.11)	(37.39)
	[0.474]	[0.478]	[0.077]	[0.737]	[0.556]	[0.359]
High y, risky (T2B)	-0.31	-5.18	-10.00	1.34	-0.00	15.76
	(4.51)	(4.89)	(13.84)	(2.23)	(0.15)	(56.91)
	[0.955]	[0.343]	[0.553]	[0.716]	[1.000]	[0.801]
H_0: T1 = T2A	0.153	0.775	0.396	0.179	0.340	0.261
H_0: T1 = T2B	0.351	0.070	0.025	0.030	0.323	0.861
H_0: T2A = T2B	0.486	0.250	0.093	0.623	0.672	0.512
Mean Outcome (C)	28.43	15.78	22.44	0.00	0.00	58.61
Observations	479	479	479	479	479	479

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. T2A and T2B indicate subgroups of treatment group 2 (T2). T2A received a fixed income transfer, and T2B received a stochastic income transfer, with mean equal to T2A. All specifications control for strata fixed effects. Standard errors are clustered at the club level and given in round brackets. In square brackets randomization inference p-values of the null hypothesis of no effect are provided; *** (**) (*) indicates significance of that test at the 1% (5%) (10%) level. Additionally the randomization inference p-value of a test of the null hypothesis that the effect of T1 and T2 are equal is provided for all estimating equations. Dependent variables in Panel A are dummy variables equal to 1 if at the time of the pre-harvest crop assessment survey, any of the following crops were observed on the plot: maize in column (1), beans in column (2), peanuts in column (3), tomatoes in column (4), potatoes in column (5) and other types of crops in column (5). In Panel B, the dependent variable is the expected output of the relevant crop on the plot. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets. All monetary values are in PPP USD.

C.4 Attrition Bounds

Table A.11: EFFECTS ON CAPITAL INPUTS - BOUNDS

	Fertilizer (1)	Insecticide (2)	Tools (3)
Panel A:		*Extensive Margin*	
High s (T1)	0.095	-0.010	0.086
Lee Bounds	[0.041, 0.124*]	[-0.053, 0.016]	[0.047, 0.140**]
Imputation 5%	[0.086*, 0.109**]	[-0.029, -0.001]	[0.065, 0.093**]
Imputation 10%	[0.075, 0.120**]	[-0.043, 0.013]	[0.050, 0.108**]
Imputation 20%	[0.052, 0.143***]	[-0.071*, 0.041]	[0.021, 0.137***]
High y (T2)	0.021	-0.071	0.007
Lee Bounds	[-0.054, 0.047]	[-0.134**, -0.053]	[-0.052, 0.059]
Imputation 5%	[0.030, 0.053]	[-0.086**, -0.059]	[-0.012, 0.017]
Imputation 10%	[0.019, 0.064]	[-0.099***, -0.046]	[-0.026, 0.032]
Imputation 20%	[-0.003, 0.086*]	[-0.126***, -0.019]	[-0.055, 0.061]
Observations	432	423	432
Lee Bounds	403	399	403
Imputation	608	608	608
Panel B:		*Intensive Margin (USD)*	
High s (T1)	1.134*	0.432	11.356**
Lee Bounds	[0.163, 1.245**]	[0.082, 0.626]	[3.392, 15.814***]
Imputation 5%	[1.193***, 1.746***]	[0.299, 0.591]	[7.603**, 10.494***]
Imputation 10%	[0.917**, 2.023***]	[0.154, 0.737**]	[6.158*, 11.939***]
Imputation 20%	[0.364, 2.575***]	[-0.138, 1.029***]	[3.267, 14.830***]
High y (T2)	0.527	-0.527	1.594
Lee Bounds	[-0.091, 0.680]	[-0.908**, -0.387]	[-5.001, 5.510]
Imputation 5%	[0.446, 0.834***]	[-0.392, 0.241]	[-0.781, 1.976]
Imputation 10%	[0.251, 1.028***]	[-0.709**, 0.557]	[-2.159, 3.354]
Imputation 20%	[-0.137, 1.417***]	[-1.342***, 1.190***]	[-4.916, 6.111**]
Observations	419	413	427
Lee Bounds	397	392	398
Imputation	599	599	599

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level based on randomization inference. "Lee bounds" provides estimates where we trim observations from above (below) in the group with lower attrition, to equalize the response rates in groups T1, T2A, T2B with respect to the control group. We then re-estimate the treatment effects in the trimmed sample to deliver the lower (upper) bounds for the true treatment effects. "Imputation x%" provides estimates where we impute to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. "Fertilizer (Insecticide) use" is a dummy variable equal to 1 if the tenant said she used fertilizer (insecticide) on her plot during the past season. "Invested in irrigation" is a dummy variable equal to 1 if the tenant said she spent time and/or money improving the irrigation of her plot. "Invested in tools" is a dummy variable equal to 1 if the tenant said she bought agricultural tools to cultivate her plot. In Panel B, the dependent variable is the monetary value of the input used in PPP USD. For irrigation, the intensive margin is the amount of money the tenant spent on improving the irrigation of the plot; for tools, it gives the value of agricultural tools that the tenant had at the time of the survey.

Table A.12: EFFECTS ON LABOR INPUTS - BOUNDS

	Own labor	Paid	Unpaid
	(hours/week)	*(days/season)*	
	(1)	(2)	(3)
High s (T1)	0.34	-0.05	8.02*
Lee Bounds	[-1.13, 1.06]	[-2.43, 0.54]	[6.23, 9.92**]
Imputation 5%	[-0.69,-0.01]	[-0.39, 0.03]	[8.13***, 8.91***]
Imputation 10%	[-1.03, 0.34]	[-0.60, 0.24]	[7.74**, 9.30***]
Imputation 20%	[-1.72*, 1.02]	[-1.01, 0.66]	[6.96**,10.09***]
High y (T2)	-0.03	1.06	1.79
Lee Bounds	[-1.49, 0.91]	[-2.04, 1.64]	[-0.60, 3.56]
Imputation 5%	[-1.41,-0.78]	[0.07, 0.44]	[0.87, 1.45]
Imputation 10%	[-1.72*,-0.47]	[-0.11, 0.63]	[0.58, 1.75]
Imputation 20%	[-2.34***, 0.15]	[-0.48, 1.00]	[0.00, 2.33]
Observations	417	432	432
Lee Bounds	399	403	403
Imputation	608	608	608

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level based on randomization inference. "Lee bounds" provides estimates where we trim observations from above (below) in the group with lower attrition, to equalize the response rates in groups T1, T2A, T2B with respect to the control group. We then re-estimate the treatment effects in the trimmed sample to deliver the lower (upper) bounds for the true treatment effects. "Imputation x%" provides estimates where we impute to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. "Own labor" is the number of hours that the tenant said she worked on the plot in a typical week during the past season. "Hired labor" in the number of worker-days the tenant said she had people working on the plot for. The dependent variables in columns 3 and 4 are the number of days of paid and unpaid labor respectively.

Table A.13: Effects on Crop Choice - Bounds

	Maize	Beans	Peanuts	Tomatoes	Potatoes	Others
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:			*Extensive Margin*			
High s (T1)	0.112**	0.049	0.055	0.021***	0.012	0.000
Lee Bounds	[0.111**,0.130***]	[0.020,0.062]	[0.020,0.073*]	[0.000,0.023***]	[0.000,0.013]	[-0.031,0.007]
Imputation 5%	[0.085**,0.108***]	[0.005,0.018]	[0.014,0.037]	[0.022***,0.026***]	[0.012***,0.013***]	[-0.017,-0.001]
Imputation 10%	[0.073*,0.120***]	[-0.002,0.025]	[0.002,0.049]	[0.020***,0.028***]	[0.011***,0.014***]	[-0.026,0.008]
Imputation 20%	[0.049,0.144***]	[-0.015,0.038]	[-0.021,0.072**]	[0.016**,0.032***]	[0.010,0.015***]	[-0.042,0.024]
High y (T2)	0.090*	0.032	0.049	-0.001	0.002	-0.016
Lee Bounds	[0.097*,0.082*]	[0.022,0.029]	[0.055,0.046]	[0.000,-0.001]	[0.000,0.002]	[0.002,-0.016]
Imputation 5%	[0.074**,0.100***]	[0.016,0.027]	[0.025,0.049]	[-0.001,-0.001]	[0.003,0.003]	[-0.011,0.009]
Imputation 10%	[0.061,0.113***]	[0.010,0.033]	[0.012,0.062**]	[-0.001,-0.001]	[0.003,0.003]	[-0.021,0.019]
Imputation 20%	[0.035,0.138***]	[-0.002,0.045]	[-0.013,0.087***]	[-0.001,-0.001]	[0.003,0.003]	[-0.040,0.038]
Observations	479	479	479	479	479	479
Lee Bounds	463	463	463	463	463	463
Imputation	664	664	664	664	664	664
Panel B:			*Intensive Margin*			
High s (T1)	4.51	5.40	32.77***	7.67*	0.27	28.55
Lee Bounds	[-0.41, 5.42]	[-3.58, 5.68]	[1.89,35.49***]	[0.00, 8.12*]	[0.00, 0.33]	[-0.76,35.53]
Imputation 5%	[1.99, 4.47]	[2.31, 3.86]	[29.33***,34.56***]	[8.29***,10.07***]	[0.33***, 0.38***]	[18.86,32.67]
Imputation 10%	[0.74, 5.72]	[1.54, 4.63]	[26.71***,37.17***]	[7.39***,10.97***]	[0.31*, 0.41***]	[11.96,39.57]
Imputation 20%	[-1.75, 8.21*]	[0.00, 6.17]	[21.48**,42.40***]	[5.61,12.76***]	[0.26, 0.46***]	[-1.84,53.38**]
High y (T2)	-2.43	1.78	4.72	-0.25	0.05	-11.47
Lee Bounds	[2.47,-2.63]	[3.83, 1.23]	[9.12, 4.65]	[0.00,-0.28]	[0.00, 0.07]	[6.97,-10.71]
Imputation 5%	[-4.53,-2.08]	[0.72, 2.57]	[4.95, 8.00]	[0.02,-0.04]	[0.09, 0.09]	[-8.67, 7.68]
Imputation 10%	[-5.75*,-0.85]	[-0.21, 3.50]	[3.43, 9.53]	[0.04,-0.06]	[0.09, 0.08]	[-16.85,15.86]
Imputation 20%	[-8.20***, 1.60]	[-2.07, 5.36]	[0.38,12.58*]	[0.10,-0.12]	[0.09, 0.08]	[-33.21,32.21]
Observations	479	479	479	479	479	479
Lee Bounds	463	463	463	463	463	463
Imputation	664	664	664	664	664	664

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level based on randomization inference. "Lee bounds" provides estimates where we trim observations from above (below) in the group with lower attrition, to equalize the response rates in groups T1, T2A, T2B with respect to the control group. We then re-estimate the treatment effects in the trimmed sample to deliver the lower (upper) bounds for the true treatment effects. "Imputation x%" provides estimates where we impute to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. Dependent variables in Panel A are dummy variables equal to 1 if at the time of the pre-harvest crop assessment survey, any of the following crops were observed on the plot: maize in column (1), beans in column (2), peanuts in column (3), tomatoes in column (4), potatoes in column (5) and other types of crops in column (5). In Panel B, the dependent variable is the expected output of the relevant crop on the plot. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets. All monetary values are in PPP USD.

Table A.14: EFFECTS ON OUTPUT - BOUNDS

	$\mathbb{E}$[Yield]		$\mathbb{E}$[Yield]/m^2	
	(1)	(2)	(3)	(4)
High s (T1)	56.11***	55.92***	0.073**	0.073**
Lee Bounds	[35.08**, 63.66***]	[34.93**, 62.04***]	[0.016, 0.084**]	[0.015, 0.084**]
Imputation 5%	[43.39***, 54.21***]	[43.09***, 53.96***]	[0.063**, 0.081***]	[0.063**, 0.081***]
Imputation 10%	[37.99**, 59.62***]	[37.66**, 59.39***]	[0.054**, 0.091***]	[0.054**, 0.090***]
Imputation 20%	[27.17*, 70.43***]	[26.79*, 70.25***]	[0.036, 0.109***]	[0.035, 0.108***]
High y (T2)	5.422		-0.000	
Lee Bounds	[-0.12, 5.32]		[0.002, -0.002]	
Imputation 5%	[6.18, 14.56]		[0.003, 0.018]	
Imputation 10%	[1.99, 18.74]		[-0.004, 0.026]	
Imputation 20%	[-6.38, 27.12**]		[-0.019, 0.041*]	
High y, safe (T2A)		17.999		0.043
Lee Bounds		[9.13, 17.61]		[0.028, 0.045]
Imputation 5%		[26.88, 36.75**]		[0.037, 0.054]
Imputation 10%		[21.95, 41.69**]		[0.029, 0.062*]
Imputation 20%		[12.07, 51.56***]		[0.013, 0.078**]
High y, risky (T2B)		-6.840		-0.043
Lee Bounds		[-8.93, -9.99]		[-0.024, -0.047]
Imputation 5%		[-14.46, -8.60]		[-0.032, -0.018]
Imputation 10%		[-17.39, -5.67]		[-0.038, -0.011]
Imputation 20%		[-23.24**, 0.19]		[-0.052**, 0.003]
Observations	473	473	473	473
Lee Bounds	457	457	457	457
Imputation	656	656	656	656

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level based on randomization inference. “Lee bounds" provides estimates where we trim observations from above (below) in the group with lower attrition, to equalize the response rates in groups T1, T2A, T2B with respect to the control group. We then re-estimate the treatment effects in the trimmed sample to deliver the lower (upper) bounds for the true treatment effects. “Imputation x%" provides estimates where we impute to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. $\mathbb{E}$[Yield] is the expected output of the plot measured through the pre-harvest crop assessment survey. It is calculated by multiplying the expected quantity of output of each crop with the price of the relevant crop measured on local markets, and summing over crops. $\mathbb{E}$[Yield]/m^2 is the expected output of the plot divided by the area (in square meters) cultivated. Values are in PPP USD.

Table A.15: WELFARE - BOUNDS

	Labor income (1)	Consumpt. (2)	Cash savings (3)	Household income (4)	Household assets (5)
High s (T1)	4.07	4.43	56.83	33.04*	656.54*
Lee Bounds	[-4.87, 5.80]	[-3.85, 9.21]	[13.64, 67.65*]	[29.31, 36.02*]	[177.07, 879.75**]
Imputation 5%	[2.47, 8.37]	[-1.80, 2.44]	[13.74, 36.57*]	[37.43***, 51.54***]	[498.43***, 798.28***]
Imputation 10%	[-0.48, 11.32**]	[-3.92, 4.56]	[2.33, 47.98**]	[30.38**, 58.59***]	[348.51*, 948.20***]
Imputation 20%	[-6.38, 17.22***]	[-8.15, 8.79]	[-20.50, 70.81***]	[16.28, 72.69***]	[48.66,1248.05***]
High y (T2)	14.98*	-3.98	66.12	0.49	183.46
Lee Bounds	[-3.97, 19.34**]	[-9.64, 1.87]	[6.41, 82.47**]	[-16.26, 10.31]	[51.52, 263.41]
Imputation 5%	[9.19, 13.76**]	[-5.26, -0.87]	[12.01, 35.76]	[-8.27, 4.19]	[176.59, 326.06**]
Imputation 10%	[6.90, 16.05***]	[-7.45, 1.32]	[0.13, 47.63**]	[-14.50, 10.43]	[101.85, 400.79**]
Imputation 20%	[2.33, 20.62***]	[-11.83*, 5.70]	[-23.62, 71.39***]	[-26.96**, 22.89*]	[-47.62, 550.26***]
Observations	424	421	427	398	427
Lee Bounds	396	395	398	382	398
Imputation	600	600	600	600	600

Notes: The table reports ordinary least square estimates based on specification (4). T1 is a dummy variable equal to 1 if the tenant/plot was randomized to receive high (75%) crop share, T2 is a dummy variable equal to 1 if the tenant/plot was randomized to receive same crop share as control (50%) and an additional cash transfer. All specifications control for strata fixed effects. *** (**) (*) indicates significance for the test of the null hypothesis of no effect at the 1% (5%) (10%) level based on randomization inference. “Lee bounds" provides estimates where we trim observations from above (below) in the group with lower attrition, to equalize the response rates in groups T1, T2A, T2B with respect to the control group. We then re-estimate the treatment effects in the trimmed sample to deliver the lower (upper) bounds for the true treatment effects. “Imputation x%" provides estimates where we impute to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to the nonresponders in the control group. “Labor income" is the average monthly labor income of the respondent during the 12 months preceding the survey. “Consumption" is the monthly consumption expenditure of the respondent; it is calculated by adding her monthly personal consumption on non-food items and services with her household’s per-capita food consumption where monthly food consumption is imputded from previous 2 days’ recall.“Cash savings" is the value of savings that the respondent has at the time of the survey. “Household income" is the response to the question “What is the total income of your household in a typical month?”. “Household assets” is the monetary value of durable assets owned by the respondent’s household. Values are in PPP USD.