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LAND RENTAL MARKETS: EXPERIMENTAL EVIDENCE FROM KENYA

Michelle Acampora, Lorenzo Casaburi, and Jack Willis

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Michelle Acampora
University of Zurich

Lorenzo Casaburi
University of Zurich

Jack Willis
Columbia University

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Abstract

Land market incompleteness is argued to have pervasive effects in Sub-Saharan Africa, including on agricultural efficiency, equity, and structural transformation. Yet experimental evidence on land market participation is virtually non-existent. We randomly allocate subsidies for agricultural rentals in Kenya, and study who selects into land markets, what renters do differently from owners, and the resulting effects on agricultural and owner outcomes. The induced rentals increase equity —reallocating plots to farmers who own fewer plots and are younger and more market-oriented —and persist beyond the subsidy. Renters increase output and value added on the rented plot, by more than owners under an equivalent unconditional cash transfer, and they do so by increasing commercial crop cultivation and non-labor inputs, rather than labor. Although owners cultivate less land under the rental subsidy, their non-agricultural labor decreases. The results shed light on the nature and magnitude of land market frictions, and on their interactions with other missing markets.

*Michelle Acampora, michelle.acampora@econ.uzh.ch; Lorenzo Casaburi, lorenzo.casaburi@econ.uzh.ch; Jack Willis, jack.willis@columbia.edu. We wish to thank Tasso Adamopoulos, Kevin Donovan, Doug Gollin, Selim Gulesci, Gunther Fink, Kelsey Jack, Paul Niehaus, Diego Restuccia, Mark Rosenzweig, Nick Ryan, Eric Verhoogen, and seminar audiences at Barcelona Summer Forum, Basel, Ben Gurion, BREAD, CEPR/Misum/SITE, Cattolica Milan, Columbia, Japan Empirical Economics Seminar, LEAP, Naples, Northwestern, NOVAFRICA, NYU Abu Dhabi, Paris-Dauphine, STEG, University of Southern California, Tilburg, Tinbergen, Trinity College Dublin, USC, Venice, Williams and Yale for useful comments. We thank Carol Nekesa, Winnie Ariya, Kadoro Mwaniki, Winfred Sakwa, and the entire REMIT team for their excellent work in managing the field activities. We are grateful to Nikolas Anic, Philippe Brügger, Maria Cedro, Hamza Husain, Malavika Mani, Nicholas Oderbolz, Flurina Schneider, and especially Jack Skelley for excellent research assistance. The project was funded by the Swiss National Science Foundation (*Eccellenza* grant), to whom we are very grateful. The experiment was registered at the AEA RCT registry, ID AEARCTR-0004530. All errors are our own. We declare that we have no relevant or material financial interests that relate to the research described in this paper.

1 Introduction

Agriculture is the main source of livelihood for over half of all households in Sub-Saharan Africa, yet markets for its key input, land, function far from perfectly. Rental markets operate under many frictions in rural areas, while sales markets are sparse, such that in many settings most plots are farmed by whoever inherited them (Deininger et al., 2017). Land market incompleteness has far-reaching consequences for the economic lives of the poor and is often considered a major obstacle to increasing agricultural productivity and to economic development. While land markets are a topic of active debate in policy and academia centers, experimental evidence on the determinants of land market participation and its effects is virtually non-existent.

The debate on land market participation focuses on three sets of outcomes: productive efficiency, equity, and structural transformation. On efficiency, one recent set of studies documents wide dispersion in productivity and argues that improvements in land markets would generate significant output gains, by reallocating land toward more productive farmers (Chen et al., 2021). In contrast, another argues that measurement error and unobserved land heterogeneity explain a large fraction of the dispersion in measured productivity (Gollin and Udry, 2021). On equity, land markets may equalize access to land, inducing its transfer towards land-poor households (Ali et al., 2015), or they may foster "land-grabs," where wealthy households acquire land, increasing its concentration (Jayne et al., 2016). On structural transformation, land markets untie landowners from their land; this may push them out of agriculture and into other activities, or the constraints to such change may lie elsewhere (Gollin, 2021).

This paper brings experimental evidence to this debate. We present results from a randomized controlled trial in Western Kenya in which we offer landowners subsidies to rent out one of their plots, thus inducing marginal land rentals. Our design allows us to identify who selects into land markets at the margin, to compare renters to owners and contrast their agricultural choices and outcomes on the rented plots, and to identify how owners' outcomes, such as food security, non-farm labor and migration, change upon renting out part of their land. In turn, the findings add not only to our understanding of land market frictions, but also to their interaction with frictions in markets for other inputs, such as labor, credit, and management.

The experiment takes place in a setting where land rental markets do operate, but imperfectly, as is often the case in Sub-Saharan Africa (Christiaensen, 2017). At baseline, farmers report multiple sources of frictions, including search costs, land disputes, concerns about soil exploitation, and transaction costs. The rental subsidy paid to landowners —worth approximately 30% of the average rental price in the village and payable for up to three crop seasons (one and a half years)

—aims to compensate owners for part of these frictions, and hence to increase the gains from, and the volume of, trade. While induced rentals could increase productive efficiency, it is not a given —the subsidy may induce experimentation, or, if too large, may induce trades known ex-ante to be inefficient, as we illustrate in a simple conceptual framework.

We began by listing plot owners in 161 villages in Western Kenya. We first listed the planned use of each plot for the upcoming season (cultivating, fallowing, renting out), and then asked owners about their interest in a subsidy to rent out one additional plot, among those they were not planning to rent out. 17% of eligible owners expressed interest in the subsidy; throughout the paper, we refer to the plot they chose to be eligible as the *Target Plot*. Interested farmers owned more land, left a higher share of their plots unused, and mentioned cash needs and a lack of inputs for cultivation as the primary reasons for their interest in the rental subsidy. After conducting a baseline survey with 521 owners who were interested in the rental subsidy, we randomized who was assigned to receive it. In addition to the control group and the rental subsidy group, we allocated one-third of the plot owners to an unconditional cash transfer group, which enables us to benchmark the income effect of the rental subsidy.

The rental subsidy led to a large and persistent increase in the likelihood that the Target Plot was rented out. While 23% of owners in the control group rented out the Target Plot, 69% did so in the rental subsidy group, a 46 percentage-point (p.p.) effect which was consistent across the three agricultural seasons in which the subsidy was offered. This effect did not displace renting out of other (non-Target) plots, which occurred rarely, and for which we estimate precise zero effects. Moreover, the effect on Target Plot rentals persisted in the fourth and fifth season of the study, when rentals were no longer subsidized, with 58% of owners in the rental subsidy group continuing to rent out, almost always to the same renter.

We identify the distributional effects of marginal rentals on land access —the differences between owners and renters for rentals induced by the subsidy —by comparing baseline characteristics of Target Plot managers across treatment groups. Renters owned 1.9 fewer plots than the 3.2 owned by owners, on average, despite having similar household sizes. Promoting land rental markets thus increased equity in land use and reduced dispersion in labor-land ratios, rather than increasing the concentration of land among wealthier households. Renters were also 7.6 years younger on average, more likely to be male, and more educated than owners. Their reason for renting does not appear to have been food insecurity —if anything, they were less likely to have experienced hunger than owner households —but they were arguably more market oriented, devoting a greater fraction of their plots to cash crops, and they were more likely to have borrowed.

We use four follow-up surveys with plot managers, undertaken at the end of each of the four crop seasons following the baseline, to identify the effects of the interventions on agricultural production. Both the rental subsidy and the unconditional cash transfer increased the likelihood that the plot was cultivated, by a similar amount (+6-8 p.p. from a control mean of 82%). However, while the unconditional cash transfer increased maize cultivation, the rental subsidy induced cultivation of commercial crops (+10 p.p. from a control mean of 9%), consistent with the finding that the rentals induced a land transfer toward younger, more educated, and potentially more entrepreneurial farmers, who are willing to engage in more commercially oriented agriculture. The rental subsidy also increased the value of non-labor inputs on the Target Plot (seeds, fertilizer, chemicals) by $14 (42% of the control mean), but there was no significant effect on labor, both household and hired, despite renter households cultivating fewer plots overall and having a higher labor-land ratio. The value of harvest increased by $31.5 (47%), and there was an increase of $23 in value added, a result robust to a wide range of valuations of household labor (Agness et al., 2022). The treatment effects of the rental subsidy were much larger than, and significantly different from, those of the unconditional cash transfer to the owner, which were mostly small. Two rounds of soil testing demonstrate that the rental subsidy had a small and non-significant effect on a soil quality index, a potential cost of more intensive cultivation. Finally, while renting out the Target Plot could have affected agricultural production on owners' other (non-Target) plots, we do not find meaningful spillovers to them.

In a final set of results, we compare owner outcomes across treatment groups. The rental subsidy did affect the non-agricultural economic activity of owners, leading to 9.1 *fewer* person-days per season of non-agricultural work. This decrease is inconsistent with land market participation leading to structural transformation by untying labor from the land, but is consistent with seasonal income effects on labor supply (Fink et al., 2020), as is the negative but insignificant effect of the cash drop. The rental subsidy had no detectable effect on food security, a non-land wealth index, and household finances.

These results shed light on multiple aspects of land market frictions, three of which we highlight: their source, their interactions with other input market frictions, and their magnitude. First, while the rental subsidy intervention is purposefully agnostic about which sources of land market frictions it relaxes, and indeed it could relax several (e.g., it may cover search or transaction costs or induce some owners to rent out even if they perceive a risk of land disputes), the results are informative of the nature of land frictions. The persistence of the experimentally induced rentals after the subsidy ended suggests a substantial fixed cost component of the frictions. Consistent with this, many owners and renters reported search costs in finding a match, which the rental subsidy may have helped overcome. It may also have fostered experimentation and learning. While at baseline owners were concerned about land disputes and soil deterioration, at endline we find little evidence of either occurring. Moreover, Target Plots on which rentals persisted beyond the intervention had higher revenue and value added than those where they did not, despite no difference in rental prices or baseline revenues, consistent with renters learning about their own productivity on the Target Plot but not suggestive of asymmetric information about its productivity.

Second, we can infer how frictions in the land market interact with frictions in other input markets. It is well understood that, if all other markets worked perfectly, land market frictions alone would not generate distortions (Singh et al., 1986; Jones et al., 2022), so frictions in other markets must be making them bind. Labor markets, contrary to other settings (see, e.g., Holden et al., 2010a), do not appear to play this role: despite renters having higher labor-land ratios, and hence potentially facing lower costs of labor, they did not increase labor on the Target Plot; moreover, owners *decrease* their labor supply off the farm when they rent out. Instead, the increase in non-labor inputs and in value added are consistent with renters having better access to capital and, possibly, higher management skills —land market frictions appear to interact with credit and management frictions, rather than with labor market frictions. Another common argument is that land markets induce efficiency gains by reducing plot fractionalization, generating economies of scale through combining contiguous plots and reducing commuting times. In two recent papers, Foster and Rosenzweig (2022) provide an analysis of economies of scale among Indian farms and Bryan et al. (2019) conduct a lab-in-the field experiment using market design to foster land consolidation in Uganda. However, while the debate on land markets often focuses on large-scale land transactions (see, e.g., Knapman et al., 2017), most rentals involve smallholders and small or medium plots (Deininger et al., 2017), with little scope to achieve large scale. Our experiment demonstrates that rental subsidies can induce increases in inputs, output, and value added despite a lack of land consolidation: if anything, renters were more likely to come from different villages and less likely to manage other plots contiguous to the Target Plot.[1]

Third, under additional assumptions, we can quantify land frictions. Overall, only 16% of farmers were interested in a 30% subsidy, showing that for most farmers the perceived trade frictions were large relative to any potential gains from trade. Among interested farmers, three back-of-the-envelope exercises, based on a simple framework of the rental market, suggest that:

[1]Another large literature on land market participation focuses on the efficiency costs of sharecropping, a contract under which the rent is paid as a fraction of the harvest (see, e.g., Burchardi et al., 2019 for a recent experiment varying the tenant's output share, without changing the identity of the plot manager). While widespread in other settings, especially in South Asia, in our setting there is no sharecropping.

the subsidy induced marginal rentals that should have taken place in a frictionless market, i.e., those with positive gains from trade; the (per-acre) value of the frictions is \$45-56, larger than the rental price (\$34); owners bear a large share ($> 90\%$) of these frictions. While the subsidy induced rentals with positive gains from trade, a misallocation exercise based on baseline productivity data predicts that other trades may have led to substantially larger gains, underlining both the importance of frictions and that there may be more cost-effective ways to improve land markets at scale.

To conclude, this paper brings experimental evidence to the debate about the gains from land reallocation across farmers in developing countries, which so far has been based on observational studies (among many others, see, e.g., Deininger et al., 2008; Jin and Jayne, 2013), quantitative analyses of misallocation (e.g., Adamopoulos et al., 2022b) or natural experiments based on institutional reforms of land rights (Chari et al., 2021 in China; Chen et al., 2022 in Ethiopia) or land administration (e.g., Beg, 2022).[2] Our approach, inducing land rentals, illustrates the feasibility of field experiments on the important but delicate topic of land leasing. Besides achieving identification under weaker assumptions, an advantage of randomized interventions is that they can foster changes in land managers while holding fixed any other differences which would arise through improved property rights and land administration.[3] Moreover, primary data collection tailored to the experiment can target households and land involved in marginal transactions and shed light on the nuances of the responses to the rentals.

The remainder of the paper is organized as follows. Section 2 describes the study setting. Section 3 presents the experimental design. Section 4 describes the effects of the rental subsidy on the likelihood of renting out the Target Plot. Section 5 studies the distributional effects of the experimentally induced land rentals. Section 6 presents treatment effects on agricultural outcomes. Section 7 reports treatment effects on owners of the Target Plot, including food security and non-agricultural labor supply. Section 8 discusses the nature and magnitude of land market frictions. Section 9 concludes.

[2] Recent papers on rental markets of other factors include Bassi et al. (2021), which shows that rental market interactions allow small firms in Uganda to increase their effective scale and mechanize production, and Caunedo and Kala (2021), which presents the results of an experiment that subsidizes access to agricultural rental equipment markets in India.

[3] Improved property rights may increase investment and productivity for many reasons beyond fostering gains from trade, including lowering the risk of expropriation, facilitating credit access via the collateral channel and reducing the need for unproductive "guard labor" (see, e.g., Besley, 1995; Besley and Ghatak, 2010; Goldstein et al., 2018; Manysheva, 2022).

2 Setting: agriculture, property rights, and land rental markets in Western Kenya

Agriculture. Agriculture employs more than 40% of the national workforce in Kenya. The sector is dominated by small-scale rainfed farming, with smallholders producing approximately 75% of the country's total food production. In our data, the median plot size among those managed by farmers is 0.5 acres, with the median household owning 3 plots and a total of 1.3 acres of land.[4] Agricultural production follows two main cropping seasons: a long rain season from March to July/August, and a short rain season from October to December. The main staple crop is maize, while commercial crops include sugarcane, tobacco, and groundnuts. 49% of farmers in our data leave at least one of their plots fallow at baseline and approximately half of these farmers do so because they cannot afford inputs.

Property Rights. Since independence in 1963, the government's legal and policy framework of land tenure has fostered private ownership, instead of communal. Today, individual property rights are prevalent in many regions of the country, including in Western Kenya, where we conduct our study. Private property rights are, however, often imperfect in rural areas, as some farmers do not hold certificates of ownership, and land disputes over competing claims to land often occur (Holden et al., 2010b).

There is little reallocation of land in our setting. Farmers own 92% of all the plots they manage, with ownership mostly acquired through inheritance (92%), although some plots are purchased (8%). The 8% of plots which are not managed by their owner are either rented in from someone else or obtained for free from family members to cultivate crops.

Land Rental Markets. Recent work documents that land rental markets operate in many African countries (see, e.g., Ali et al., 2015; Christiaensen, 2017; Deininger et al., 2017). This is also true in Kenya. Our listing data suggests that 4% of households are renting out some land; among owners interested in the subsidy, this share is 10%.[5] Rental contracts usually include an upfront cash payment and a rental period of 1 to 2 years, covering 2 to 4 agricultural seasons. The average size of the plots rented out is 1 acre and the average rental price per acre per season is \$37.8 (median \$30). The rental price varies, both within and across villages, with a standard deviation of \$32.3 across villages, suggesting that land markets are not subject to a binding price norm, unlike labor markets in many settings (Kaur, 2019).

[4] Section 3.4 presents an overview of our primary data sources.

[5] In our context, as it is often the case, we may underestimate the overall engagement in land rental markets as landowners residing outside the villages may not be captured by our surveys (Deininger et al., 2017). For comparison, in Europe and in the U.S. a substantial proportion of agricultural land is rented – 80% in France, 60% in Germany, 46% in Italy (Vranlen et al., 2021) and 40% in the U.S. (USDA 2017 Census of Agriculture).

While rental markets are active in our context, their functioning is constrained by several institutional factors. In our data, owners perceive several barriers to renting out: search costs associated with finding a renter (57%), concerns about soil exploitation from renters (51%), fear of land disputes (41%), and costs associated with rental contracts, such as fees to the village chief (22%). Renters also perceive several risks and costs associated with renting in, which include: potential land disputes and eviction risk before harvest (36%), asymmetric information over the quality of the rented land resulting in low yields (31%), time and resources required to learn how to best farm the rented land (37%), and costs associated to the rented plots being far from the homestead (33%).[6]

3 Experimental design

To implement the study, and to induce trades in a notoriously complex and sensitive market (United Nations, 2012), we had to make important decisions about critical aspects of the design, such as the identification of potential compliers (of both owners and plots), the timeline and duration of the intervention, the amount of the subsidy, and the conditions for its disbursement. In this section, we discuss some of the key trade-offs in these decisions and the rationales for our choices, as we describe the study design, including the sample selection, intervention, randomization strategy, data collection, and balance. Figure 1 illustrates the timeline of the field and survey activities. We also present a simple framework to characterize the trades induced by the subsidy.

3.1 Eliciting interest: farmer listing, sample selection, and owner baseline

Listing and eliciting interest. After a small-scale pilot in the first half of 2019, field activities for the main evaluation began in July 2019, towards the end of the 2019 Long Rains crop season. Enumerators visited 161 villages in four West Kenyan counties (Bungoma, Kakamega, Migori and Siaya) and conducted a brief listing module with 7,545 plot owners. Each respondent answered a short section on demographics and listed each of their owned plots. For each plot, we asked questions on size, distance from the respondent's house, and use —cultivation, fallowing, and renting out —for both the 2019 Long Rains season and the upcoming 2019 Short Rains.

At the end of the listing survey, we asked whether the respondent would be interested in receiving a subsidy ("top up") for renting out one plot among those she was not already planning to rent out (based on the answers in the listing). We provide further details on the rental subsidy in Section 3.2. For ethical considerations, only owners with at least two plots (N=5,425) were

[6] Experimental evidence from Uganda in Kaboski et al. (2022) suggests that relaxing liquidity constraint may also increase land market transactions.

eligible for the subsidy. Each of these interested owners was then asked to identify one plot they would be interested in renting out, should they receive the rental subsidy. In the rest of the paper, we refer to this plot as the '*Target Plot*.' At the end of the listing, our enumerators conducted a GPS measurement of the Target Plot.

This sampling procedure aimed to identify likely compliers: owners who had not yet rented out the Target Plot, but who would rent it out should they receive the rental subsidy. For our research design, it is crucial to identify this plot before the randomization and the rentals take place: this step allows us to compare plots that are similar ex ante, but are then exposed to different treatments during the experiment and, as we will see later, experience different likelihoods of being rented; i.e., it gives us a plot-level counterfactual. The timing of the listing and of the baseline was in line with this goal: we completed the listing activities in the last pre-intervention harvest season (2019 Long Rains) and offered the rental subsidy to the selected households shortly thereafter (more details below).[7] Regarding selection, Section 4.1 compares owners' interested in the subsidy to those who were not interested and Target vs non-Target Plots.

Sample selection and baseline owner survey. Shortly after the listing and while harvesting of the 2019 Long Rains (i.e., the last pre-intervention season) was ongoing, we conducted a baseline household survey with owners who expressed interest in the subsidy during the listing. The experimental design required conducting the randomization and offering the subsidies before the 2019 Short Rains crop season started. Thus, due to time constraints, we attempted to baseline only 767 of 879 interested owners and tracked and interviewed 618 of them (80.5%). After applying a few sample restriction criteria, our final study sample included 521 owners interested in the subsidy (and their Target Plots).[8] The baseline survey collected information on demographics, agricultural activities for the previous two crop seasons (2019 Long Rains and 2018 Short Rains) on each plot owned or managed by the respondent (including the Target Plot), non-agricultural activities, food security, assets, and access to financial markets.[9]

[7] Running the listing earlier would have likely reduced compliance, as we would have identified many plots as eligible for the rental subsidy that eventually would have been rented regardless. Going too late would have reduced the chances that interested owners could find a renter for the soon-to-start season.

[8] Sample restriction criteria included: outliers in plot size, enumerator reporting the wrong subsidy amount to the respondent, owners reporting they had already rented out the plot or that they did not own the plot or that they did not expect to be able to find a renter. Appendix Table A.1 compares listing data for interested owners who were surveyed (and thus entered the study) and those who weren't. The two groups are comparable in terms of gender, age, land ownership, cultivation rates, and crop choices, but non-surveyed owners had somewhat higher likelihood of renting out one of their plots at baseline (0.13 vs. 0.06).

[9] To leave those owners who were subsequently assigned to the conditional subsidy group enough time to find renters, we ran the baseline survey while the 2019 Long Rains harvesting was ongoing. Therefore, we do not have information on harvest amount for that season for a large portion of the sample. We did however collect information on harvest amount in the previous season, i.e., the 2018 Short Rains crop season.

3.2 Interventions: the rental subsidy and the unconditional cash transfer

The main treatment of interest is the rental subsidy. To benchmark it and control for income effects, we also introduced an unconditional cash transfer. We discuss their details below.

Rental subsidy value and duration. Owners randomly selected into the rental subsidy group received the subsidy if they rented out the Target Plot identified in the listing. The rental subsidy was expressed in per-acre terms and was worth approximately 30% of the average rental price.[10] We collected the average rental price in each village through a brief community survey ran before the listing. In most villages, rental prices were between $30 and $40 per acre per season. We set on a subsidy of approximately 30% of the rental price because initial qualitative fieldwork suggested this would ensure a sufficient number of interested owners. Smaller subsidies may have led to an excessively small number of compliers; higher subsidies did not seem to induce much additional supply (i.e., the elasticity seemed small above the chosen rate).

We offered the subsidy for up to three crop seasons. As we discuss later (Section 4.3), this duration is in line with the average duration of non-incentivized rentals. We announced that we would be paying the rental subsidy at the same time of the renters' payment. Since in multi-season contracts renters usually pay the owner upfront for the entire duration of the contract, we also paid the rental subsidy upfront for all seasons in these cases. Payment of the rental subsidy occurred mostly via mobile money, with a handful of payments in cash.

Rental subsidy restrictions and verification. We placed no restrictions on who the plot could be rented out to, beyond it being someone outside of the household of the owner. We did not want to restrict the choice of renter for three reasons: first, so that our matches would be "organic", i.e., close to occurring naturally; second, so that our intervention was as close as possible to a pure monetary incentive, without further restrictions whose effects would be hard to quantify; and third, since pilot work suggested rental markets were thin with substantial search costs, limiting the set of potential renters may have led to little renting out.

This decision not to restrict the set of potential renters made it infeasible to have a counterfactual for those renting in, and hence to observe treatment effects on them. Alternative experimental designs that would have provided a renter counterfactual were not feasible for logistical or bud-

[10] This per-acre rate was based on the average Target Plot size between the one reported by the plot owner and the one measured with GPS. We averaged over the two measures for two reasons. First, both measures suffer from measurement error, and averaging them may remove some of the noise. Second, we did not want to run the risk of discouraging the owner and reducing compliance in those cases where the self-reported size was higher. On average, self-reported plot sizes were approximately 20% higher than GPS ones. In the rest of the paper, we use this average measurement when referring to Target Plot size, except in Table 1 – Panel B, where we use the size reported by the owner when comparing Target Plots to non-Target plots, for which we don't have GPS measurement.

getary reasons.[11] In addition, our individual-level randomization design cannot shed light on general equilibrium effects on rental prices induced by the intervention.

A natural concern is that owners in the rental subsidy group may try to misrepresent the rental status of the plot to receive the subsidy. We put in place several measures to mitigate this. First, we required written confirmation of the rental agreement by the local chief, including the signatures of two witnesses. In most of these cases, we paid a small token to the chief ($1), which we factor in when computing the amount for the unconditional cash transfer treatment, described below. By giving rights to another person on the plot harvest and by thus raising the cost of cheating for the owner, the chief rental confirmation was an important step for the validity of the experimental design. It is also possible that the chief confirmation raised security of the rental and thus may affect behavior, e.g., increase the renter's willingness to invest on the plot. We return to this point in Section 3.7 when we discuss our estimation strategy. Second, our enumerators conducted an extensive verification with both the owner and the renter before disbursing the payment. The verification checked for consistency in the rental terms reported by the two parties. In Section 4.3, we show that the basic terms of the rentals (price, duration, relationship between the owner and the renter) are similar across rentals that occurred in the three treatment groups, adding further confidence that our intervention induced real rentals.

Of course, while these measures plausibly reduced cheating, we cannot claim they eliminated it completely. We observe that any remaining cheating would inflate the measured effects of the rental subsidy on rental probability, but it would reduce the treatment-on-treated effects on agricultural outcomes (e.g., the effects of receiving the rental subsidy on input use).

Unconditional cash transfer. We compare the effects of the rental subsidy with those of an unconditional cash transfer designed to match the size of the rental subsidy. As with the rental subsidy, the per-season value of the unconditional cash transfer was based on the Target Plot's size. We also calibrated the number of seasons for which we offered the cash transfer on the distribution of the number of seasons in the rental subsidy group (Section 4.2 provides more details). The cash transfer payment occurred mostly via mobile money, with a handful of payments in cash.

3.2.1 Conceptualizing the interventions

To characterize the trades induced by the intervention, we introduce a very simple framework. Denote by Δ the gains from a rental, absent any costly frictions (i.e., the increase in profits a

[11] For instance, one alternative would be to restrict the sample to Target Plots where the owner had already identified potential renters, but we were concerned this may lead to a small treatment effect on the likelihood of renting out the Target Plot. Another design would identify lists of potential renters in each village and randomize rental subsidies at the cluster-level, but this was not possible with our budget.

renter can accrue on the land). The owner's participation constraint is $p > \pi$ (where p is the rental price and π is the profit the owner makes on the plot) and the renter's participation constraint is $\pi + \Delta > p$. With frictionless markets, a rental occurs if $\Delta > 0$, i.e., as long as the gains from rentals are positive.

Now denote by τ the cost of land market frictions. The owner's participation constraint is now $p - \alpha\tau > \pi$, where α is the share of frictions borne by the owner, and the renter's participation becomes $\pi + \Delta > p + (1 - \alpha)\tau$. Therefore, with frictions, rentals occur if $\Delta > \tau$: frictions hamper trades with $\Delta \in (0, \tau]$. With a rental subsidy s, rentals occur if $\Delta + s > \tau$. The marginal trades induced by the subsidy are those with $\Delta \in (\tau - s, \tau]$. They would be efficient in a world without frictions (since $\Delta > 0$), unless the value of the subsidy is too large ($s > \tau$).

The framework is of course extremely simplified and ignores uncertainty and dynamics (e.g., fixed cost vs per period costs for frictions, learning), among other complexities. Nevertheless, it is useful to illustrate how the subsidy may offset frictions and to characterize the marginal rentals. In Section 8.3, we conduct simple back-of-the-envelope exercises based on this framework to recover the value of land frictions, τ, and the share of frictions borne by the owner, α.

3.3 Randomization

We randomized the 521 study owners into three groups: rental subsidy, unconditional cash transfer, and control. We performed the randomization in five waves. Within each wave, we stratified the randomization by county, intended Target Plot use reported by the owner for the upcoming crop season (66% cultivating vs 34% fallowing or undecided), and plot size group. In the rest of the paper, we refer to the stratum where the owner was planning to cultivate the Target Plot as *Stratum C* and to the stratum where the owner was not planning to cultivate (including a few undecided cases) as *Stratum NC*.[12] Ad we discuss in Section 6, the breakdown of results in these two strata sheds light on the nature of the responses to the rental subsidy.

3.4 Data collection

Our data collection strategy includes an owner baseline survey at the end of the 2019 Long Rains (season 0), a renter baseline survey at the beginning of the 2019 Short Rains (season 1), and follow-up data collection at the end of four crop seasons: 2019 Short Rains (season 1), 2020 Long Rains (season 2), 2020 Short Rains (season 3), and 2021 Long Rains (season 4). We have described the owner baseline survey in Section 3.1. Here, we describe the other surveys.

[12] Appendix Table A.2 presents a comparison of stratum C versus Stratum NC on two sets of outcomes: owner demographics and socio-economic variables and Target Plot baseline characteristics.

Baseline renter survey. At the beginning of the first experimental season (2019 Short Rains), we collected information on all rentals of the Target Plots. In this season, 212 Target Plots were rented out. We then conducted a survey with each renter, which included similar questions to the baseline owner survey: demographics, agricultural activities by plot for the previous two crop seasons for each plot owned or managed by the respondent in those seasons, non-agricultural activities, food security, assets, and financial markets.

Follow-up surveys. At the end of each of seasons 1-4, we conducted a round of follow-up surveys. We asked questions about agricultural activities on the Target Plot to the managers of the Target Plot during that season: the owner if the plot was not rented out and the renter if it was rented out. In addition, regardless of whether they were managing the Target Plot, we asked the owners questions about their other plots, non-agricultural activities, food security, assets, and household finances. Since large shares of output are not sold, and sizable shares of inputs are not purchased, we compute median prices of each crop and input by crop-season-county and compute the values by multiplying the price by the relevant quantity. Concerning labor, following Agness et al. (2022), in our preferred specification we price household labor at 60% of the wage of hired labor, but we also present robustness to alternative valuations.[13] We also collected information on whether the Target Plot was rented out in crop season 5 (2021 Short Rains), even though we did not conduct a full follow-up survey. Due to COVID-19, we conducted phone interviews for the second half of follow-up round 1 and the entire follow-up round 2.[14]

Soil samples. At the end of crop season 1 (2019 Short Rains) and crop season 4 (2021 Long Rains), we collected soil samples from the Target Plots. Kenyan laboratories analyzed the samples to measure several soil nutrients (nitrogen, phosphorous, potassium, organic matter, and the pH level of the soil). Following Burchardi et al. (2019), we constructed a soil quality index by first standardizing each measurement into a z-score, taking the mean of each plot's z-scores and then standardizing again against the control group. The soil index has predictive power: a one s.d.

[13] In about 12% of the follow-up surveys, the harvesting on the Target Plot was not completed when the enumerator visited. In this case, we recorded the planned harvest amount and then in many cases verified the actual amount, either with ad hoc phone calls or in subsequent survey rounds. After this verification process, we are left with approximately 6% of plots with a non-verified planned harvest amount. In Section 6.3, we show that results on harvest value and value added are robust to controlling for a dummy for non-verified planned harvest (Appendix Tables E.5 and E.6). Finally, if the crop cycle does not match the standard rain season cycle, we divide the harvest amount evenly across all the rain seasons in which cultivation occurred.

[14] In season 1, we also attempted to conduct a crop-cutting exercise for the most common major crops in our setting: maize, beans, groundnuts. We aimed to conduct the field visits shortly before harvesting. However, the harvesting time of various crops was quite spread out across multiple weeks, or even months. As a result, we ended up running the crop cutting only for maize. Even for maize, however, we missed approximately 25% of the plots growing this crop, due to early harvesting. For more plots, harvesting was already partially ongoing by the time of our visit. We ended up collecting (often incomplete) crop-cutting data in only 227 of the 521 plots in the sample. In addition, many other implementation challenges emerged when implementing this task in the field. Therefore, we opted not to continue this exercise in subsequent seasons.

increase in the soil index is associated with an increase in harvest value of 18.5% of the mean.

3.5 Randomization balance

Appendix Table A.3 presents the balance of baseline covariates on owner demographic and socio-economic variables, as well as Target Plot and non-Target plot characteristics. While most baseline variables are balanced, a few variables are unbalanced. The control group appears to have higher likelihood of erosion and lower value of inputs and hired labor compared to both the rental subsidy and the cash transfer groups. As we discuss in Section 3.7, in the main results we use ANCOVA specifications that control for the value of baseline outcomes and all the results are also robust to post-double selection of controls (Belloni et al., 2014). Finally, the unbalance of a few baseline variables in the control group is not a concern for the comparisons between the rental subsidy and the unconditional cash transfer, as the two treatment groups are overall well balanced.

3.6 Attrition

Overall, attrition rates are low, with survey completion rates of at least 91% in all surveys and above 95% for most rounds (Appendix Table E.8). However, there is some differential attrition by group: the unconditional cash transfer group has a significantly higher completion rate than control (+3 p.p.) in the follow-up surveys with Target Plot managers (Panel B) and the rental subsidy group has a significantly lower completion rate (-5 p.p.) than control for soil tests (Panel C) and owner follow-up surveys (Panel D). Given the low rates of attrition, any bias induced by differential attrition is unlikely to dramatically influence our results. To examine the extent of any bias in our results, we follow Lee (2009) and construct bounded treatment estimates for attrition. We present these bounds when we discuss the experimental results.

3.7 Empirical Strategy

The experimental analysis focuses on treatment effects on rental of the Target Plots, agricultural production on the Target Plots and on owners' other plots, and finally on owners' non-agricultural outcomes. Here, we provide an overview of our empirical strategy. Appendix B presents details, including all the estimating equations.

First, in Section 4, we use data from follow-up surveys to document the effect of treatments on the likelihood that the Target Plot is rented out:

$$y_{is}^t = \beta_0 + \beta_1 RentalSubsidy_i + \beta_2 CashTransfer_i + \delta x_i^0 + \eta_s + \eta^t + \epsilon_i^t, \quad (1)$$

where the outcome is a dummy for whether the Target Plot i is rented out in crop season $t = 1, 2, 3, 4, 5$, η^t is a vector of crop-season fixed effects, η_s is a vector of strata fixed effects, x_i^0 is a vector of baseline controls that includes the size of the Target Plot and the baseline values of the outcome variable in the 2018 Long Rains and 2019 Short Rains.[15]

Second, in Section 5, we look at the distributional effects of rentals on land access, comparing the characteristics of owners to renters for the experimentally-induced rentals. The analysis uses baseline data from the owner baseline and the renter baseline surveys. We study how the treatment changes baseline characteristics of the plot manager, including demographics (e.g., age, gender, education), wealth (agricultural land owned, non-land wealth), baseline use of agricultural inputs, and agricultural productivity. We present both Intent to Treat (ITT) and Local Average Treatment Effect (LATE) estimates, where we instrument whether a Target Plot is rented out with the rental subsidy treatment.

Third, in Section 6, we examine how the treatments affect agricultural production, including cultivation rates, crop choice, input value, harvest value, value added, and soil quality —first for the Target Plot (using follow-up surveys with the Target Plot Manager), and then for owners' other plots (using follow-up surveys with the Target Plot Owner, reshaped by plot). We present these results pooling across seasons (clustering standard errors by Target Plot and owners, respectively) and then by season (in Appendix B). For continuous outcomes, we focus on winsorized (1%) outcomes in levels and on the inverse hyperbolic sine (IHS) transformation of the total outcome across rounds.[16] We estimate ITT, which follows closely Equation (1), and Treatment on Treated (TOT), where we instrument whether the respondent received any rental subsidy or unconditional cash transfer payment during the study with the treatment assignment dummies:

$$y_{is}^t = \gamma_0 + \gamma_1 Rental\widehat{Subsidy}Paid_i + \gamma_2 CashT\widehat{ransf}erPaid_i + \delta x_i^0 + \eta_s + \eta^t + \epsilon_i^t. \tag{2}$$

There are two different questions we can answer directly through these TOT coefficients. First, what is the effect of offsetting the rental frictions through the payment of the rental subsidy to the owners? This is given by γ_1 and includes *i)* the effect of the induced rentals, *ii)* an income effect of the subsidy to owners (which may be partly passed through to renters), for both marginal and inframarginal rentals. We note that, since we require the chief to confirm the rental (see Section 3.2), the estimates capture the effects of rentals verified by the chief. Second, as a policy question, how does the effect of a dollar spent on rental subsidies compare to the effect of a dollar spent on unconditional cash transfers to owners? This is simply the comparison of γ_1 to γ_2. When comparing these two coefficients, one should keep in mind that the set of compliers differs between

[15] We collected data on rentals for the upcoming season 5 in the follow-up survey at the end of season 4.

[16] Season-specific outcomes contain sizable shares of zeros (e.g., mostly because some plots are not cultivated in certain seasons) and, thus, we cannot use IHS in that case (Bellemare and Wichman, 2020).

the two treatments. However, as there is essentially perfect compliance in the unconditional cash transfer, the comparison of γ_1 to γ_2 is a lower bound of the effect of the rental subsidy on compliers in this group *controlling for the income effect*, under the plausible assumptions that the income effect on the outcome of interest (e.g., inputs): i) is (weakly) stronger when the owner, who receives the payment, does not rent out the plot; ii) goes in the same direction for those who do not take up the rental subsidy as for those who do.[17]

Finally, in Section 7, we use owner surveys to study treatment effects on owners' non-agricultural outcomes, including food security, non-agricultural activities, assets, and household finances.

4 Do subsidies increase land rentals?

In this section, we first discuss selection into the experimental sample: which owners are interested in the subsidy, and which plots they choose as Target Plot. We then present, for our sample, take up of the treatments and their effects on the likelihood of renting out the Target Plot.

4.1 Interest in the rental subsidy and selection of rented plots

In the listing exercise, 879 of the 5,425 eligible owners (16.2%) expressed interest in the rental subsidy. The main reasons for their interest were needing cash (78%), not having sufficient inputs to cultivate the plot (16%) and being unable to hire sufficient labor to cultivate the plot (15%).

Selection of farmers: interested vs non-interested owners. Table 1 – Panel A shows that, compared to those who did not express interest for the rental subsidy, interested owners owned more land and were more likely to both rent out their plots and leave them fallow. The results are based on data from the listing exercise, which only collected limited information on demographics and agricultural plots. Interested owners were also more likely to be male and own a phone. There was a small difference in experience of cultivating commercial crops, with interested owners having less experience than their non-interested counterparts.

Selection of plots: Target Plots vs. non-Target Plots. Interested owners were asked to choose the plot for which the rental subsidy would apply — the Target Plot — during the listing exercise. Table 1 – Panel B presents a comparison of baseline characteristics for Target versus non-Target plots. Overall, Target Plots are similar to non-Target Plots in terms of observable characteristics: size, location in the same village as where the respondent lives, and likelihood of

[17] A third question of interest would be what is the effect of the rentals induced by the subsidy, absent any income effects the subsidy induces? As is common in conditional subsidy designs, we cannot estimate the LATE of the actual rental status of the Target Plot, because the exclusion restriction fails: the rental subsidy may affect the Target Plot outcomes not only by inducing rentals, but also because of an income effect, on both marginal and inframarginal rentals. In Appendix B.3, we discuss how we can bound this effect and we use these bounds for the back-of-the-envelope exercises in Section 8.

being irrigated. The only observable difference is that Target plots are somewhat less likely to have a sandy-clay type of soil. From the perspective of baseline agricultural use, Target Plots are significantly less likely to be cultivated and more likely to be rented out, both in the 2019 Long Rains and in the 2018 Short Rains. These effects are significant at 1% and are consistent with the fact that owners were asked to identify a plot that they would be interested in renting out, conditional on receiving the subsidy. Finally, Target Plots are also slightly less likely to be cultivated with commercial crops at baseline and the average value of hired labor employed on these plots is higher compared to non-Target plots (both differences significant at 1%).

4.2 Take up of the treatments

Rental subsidy take up. 70.3% of Target Plot owners eligible for the rental subsidy took it up. Of those who took it up, 76% received the rental subsidy for three seasons, 18.2% for two seasons, and 5.8% for one season. The two main reasons for incomplete take up were that the owner either could not find a renter (87%) or that they decided to cultivate the Target Plot themselves (11%). Appendix Table C.1 compares baseline characteristics of treatment owners who took up the subsidy vs those who did not. Compliers have more education and training, they supply more labor for agricultural and non-agricultural work, own larger Target Plots, use more inputs, and have more access to savings and credit.

There was little churn in renters, with most owners who took up the rental subsidy for multiple seasons having the same rental agreement throughout. A handful of owners first rented out the plot for one or two seasons (and received the subsidy for those seasons) and then completed a different rental agreement for one or two additional seasons, for which they received additional rental subsidies. Since those renters who rented for multiple seasons typically paid the entire rent at the beginning of the first season, we then also paid the rental subsidy for multiple seasons at the beginning of the first season.

Unconditional cash transfer take up. The take up of the unconditional transfer was nearly universal (99%). To determine the number of seasons for which the household received the transfer, we matched the distribution of the number of seasons for the rental subsidy, randomizing the allocation within each county-cultivation-plan stratum. As a result, of the owners who took up the unconditional transfer, 82% received it for three seasons, 12.8% for two seasons, and 5.2% for one season.[18] Since we needed to perform this matching after observing the realization of rentals in the rental subsidy group, we typically made payments in the unconditional cash transfer group

[18] We also match the timing of payment (upfront or by season) for those eligible for multiple seasons with the analog distribution in the rental subsidy. The small discrepancy from the rental subsidy distribution arises because some rental subsidy owners found a renter and took up the subsidy after we had performed the matching.

a few days after the disbursement of rental subsidy payments.

4.3 Treatment effects on rentals

Target Plot rentals in the intervention seasons. The rental subsidy treatment led to a large increase in the likelihood that the Target Plot was rented out. Figure 2 demonstrates that in the three seasons in which treatment households where eligible for the rental subsidy, the likelihood of rentals increased by 45-47 percentage points, from a control mean of 0.22-0.24. The unconditional cash transfer also had a positive effect on rentals, but this is much smaller (5-8 p.p.) and non-significant. Appendix Table C.2 presents regression results and also shows that the impact of rentals was similar in strata C and NC.[19]

While we conducted our sampling to identify potential compliers (see Section 3.1), our intervention still exhibits imperfect compliance. This arises for two reasons. First, some treatment owners did not take up the rental subsidy (see Section 4.2), either because they turned out not to be interested or because they could not find a renter. Second, some control owners ended up renting out the Target Plot, even if in the listing they had mentioned they were not going to rent it out.[20] It is nevertheless crucial for the rest of our analysis that we induced a sizable difference across treatment groups in the likelihood of renting out the Target Plot.

Persistence of Target Plot rentals after the rental subsidies end. The treatment effect of rental subsidies on Target Plot rentals persisted after the intervention ended (seasons 4 and 5). The treatment effect is still very large, 34-38 p.p., though smaller than in the intervention seasons. Almost all of these rentals (94%) were with the same renters who managed the plot in seasons 1 to 3. This persistence suggests that the subsidy may have helped foster long-term relationships between owners and renters by covering fixed search costs or by fostering experimentation, an issue we delve into in Section 8. Most owners who rented out the Target Plot reported being willing to do so again and having not had problems with the renters, ruling out the alternative hypothesis that the persistence in rentals reflects difficulties in evicting tenants.

Rentals of non-Target Plots. Increased rentals of Target Plots did not crowd out renting out of other plots owned by the treatment households. Table 5 shows that the rental treatment did not affect the likelihood of renting out non-Target Plots (see Section 6 for further details).

[19] By combining information on planned rentals in the listing survey, on rentals of study owners in the control and treatment groups, and on the value of the subsidy, we estimate an elasticity of land rental supply of about 3. We observe, however, that this may be an upper bound if, as was the case for the owners interested in the subsidy that enter our study, other owners also ended up renting out a higher share of plots than they reported in the listing.

[20] There are many potential reasons why some owners in the control group rented out the Target Plot. For example, if the plan reported in the listing was preliminary or if owners began exploring rental opportunities (paying search costs) upon hearing about the possibility of rental subsidies. We cannot disentangle among such explanations but doing so does not matter for our results, nor their interpretation.

Comparing rentals in treatment vs. control group. Appendix Table C.3 compares season-one rentals in the rental subsidy group (i.e., rentals that were mostly induced by the experiment) with those in other groups (and thus not induced by the experiment). 212 Target Plots were rented out, 57% of which were in the rental subsidy group. Due to the small number of rentals in each of the Cash Drop and Control groups, and their similar rental rates, we pool them together to gain power in the comparison. Overall, the Target Plots rented out were similar across the two groups, except that those rented out in the rental subsidy group were significantly less likely to have been previously rented out. Regarding the contracts, rentals induced by the subsidy had a similar duration and a similar rental price to those in the other groups.[21] The relationships between owners and renters were similar across both groups, with about one third of Target Plots being rented out to family members, and a fifth of renters residing in a different village than the Target Plot, across both groups. However, renters in the rental subsidy group were significantly less likely to have rented in the Target Plot before (significant at 5%). Overall, these results suggest that the rental subsidies successfully induced new rentals and that the rental contracts were comparable to those naturally occurring in this context, both in terms of characteristics of the plots rented out and of features of the contracts.

5 Distributional effects on access to land

In the next three sections we present results corresponding to three research questions central to the debate on the effects of land market frictions: *i)* the distributional effects; *ii)* the effects on agricultural production; *iii)* the effects on non-agricultural activities and other owner outcomes.

We begin by studying the effects of the experimentally-induced marginal rentals on land distribution across farmers. We compare the baseline characteristics of the manager of the Target Plot in the first season across treatment groups, both in the ITT and in a LATE where we instrument for the plot being rented out with the rental subsidy treatment (see Appendix B.2). We consider effects on: (i) demographics and education, (ii) agricultural land and practices, and (iii) food security, wealth and finance.

5.1 Demographics and education

Households renting in are the same size as households renting out, on average, but the heads of their households are younger, much more likely to be male, and more educated. Column 1

[21] We cannot experimentally identify pass-through of the subsidy to the renter via a reduction of the rental price: that would require observing the rental price of an equivalent set of rented plots with and without the subsidy, but the subsidy treatment induced many more plots to be rented out.

of Panel A, Table 2 shows no meaningful effect of renting out on household size. Column 2 shows that, among complier households, heads of households renting in were much younger than those renting out – 7.8 years younger, on average. Column 3 shows that the vast majority of household heads renting in under the subsidy were male – the local average treatment effect is 26 p.p., on a control mean of 69% of households. Thus, rental markets in this setting appear to redistribute agricultural land from women to men; however, we cannot say whether this increases or decreases gender equality, in terms of wellbeing – for example, if female headed households are labor constrained, renting out the Target Plot may benefit them. Lastly, consistent with their being younger, among complier households, those renting in are substantially more likely to have finished high school, with a 14 p.p. effect on a control mean of 0.24.[22]

5.2 Agricultural land and practices

Target plot renters own substantially less land than owners renting out and are more likely to already be renting in other plots. Column 1 of Panel B, Table 2, shows that among the rentals induced by the rental subsidy, those renting in own 1.9 fewer plots on average than those renting out, compared with a control group mean of 3.2 plots owned on average. In Appendix Table D.2 we show that this corresponds to 1.5 fewer acres, from a control mean of 2.1. This is a large effect, and together with the null effect on household size, shows that households renting in have a substantially higher labor-to-land ratio than those renting out. Consistent with this, Column 2 shows that those renting in during our experiment are 30 p.p. more likely to already be renting in a plot, compared to a control mean of 7% of owners, although the majority of the renters are new renters, in that they did not rent in a plot the previous year.

In terms of what they do with their land, Column 3 shows a higher, but insignificant, share of plots cultivated with cash crops among renters. The local average treatment effect on the share of cultivated plots having cash crops is 5 p.p., an almost 50 percent increase from the control mean of 11%, suggesting that those renting in have higher prevalence of cash crop cultivation, but this result is noisy and should be interpreted with caution.

Households renting in are more likely than owner households to live in a different village than the Target Plot (Col. 4). In the control group, the Target Plot is outside the owner household's village in just 5% of cases. The local average treatment effect shows that, among induced rentals, the Target Plot is 19 p.p. more likely to be outside of the village of the household renting in the Target Plot than the village of the household renting it out. This is consistent with average

[22] These findings, on the gender and education of renters vs. owners, are qualitatively similar if instead of focusing on the head of the household, we focus on the person reported to be "in charge of taking agricultural decisions for the target plot" (which coincides with the HH head in 84% of cases for the manager survey).

walking time to the target plot being higher among those renting in than those renting out, as reported in Appendix Table D.2. We elaborate on the implications of these findings in Section 8.

5.3 Food security, wealth, and finance

One hypothesis for why people may rent in agricultural land is that they are food insecure and wish to cultivate more land for personal consumption. Column 1 of Panel C, Table 2, does not support this hypothesis in our setting – if anything, those renting in were *less* likely to have experienced a hunger period in the last 12 months than those renting out, with a local average treatment effect of -13 p.p. on a control mean of 33%.

We also investigate differences in non-land wealth between owners and renters. Column 2 reports treatment effects on a wealth index, reflecting (non-land) assets and household amenities. The index is the principal component of a vector of 16 common assets and 6 common household amenities, standardized in the control group. The point estimate is -.25 st.dev. of the control group mean, showing that those renting in are somewhat poorer on average (p-value =0.13).

Another hypothesis for why renters may increase productivity relative to owners, and hence wish to rent land from them, is that they may be more familiar with finance and hence more willing to take loans for the purpose of cultivation. Column 3 shows that those induced by the rental subsidy to rent in are 24 p.p. more likely to have borrowed in the last 12 months than those they rent from, compared to a control mean of 62%. These figures do not include loans to explicitly rent and cultivate the Target Plot: we find that about 20% of the renters got such loans throughout the four experimental seasons. Column 4 shows there is no significant difference between those renting in and renting out in whether they would be able to finance a 5k Ksh ($50) emergency expenditure from their own savings. The estimate however is relatively imprecise, such that we can only reject a difference of more than 23 p.p., compared to a point estimate of 9 p.p.

The results on manager characteristics are mostly robust to Lee Bounds, (Appendix Table D.1). Appendix Table D.2 presents additional results: we highlight that renters were less likely than owners to have completed agricultural training, but reported marginally significant higher output per acre on the plots they cultivated at baseline.

5.4 Distributional effects on access to land: discussion

The marginal transactions induced by the experiment led to a reallocation of land away from households who own lots of land, towards those who own little, with no difference in average household size. Thus, the reduced land rental market frictions increased equity in land use and reduced dispersion in labor-land ratios, consistent with one strand of the observational literature (see, e.g., Jin and Jayne, 2013; Chamberlin and Ricker-Gilbert, 2016; Deininger et al., 2017).

The analysis of other characteristics reveals a more nuanced picture of their distributional effects. Renter households were substantially younger, almost exclusively male headed, and more educated than owner households (in line with some other studies, e.g., Ali et al., 2015). Renters were also potentially more market-oriented, devoting a greater fraction of their plots to cash crops and being more likely to have borrowed. These findings suggest that the (land) wealth gap may partially reflect the fact that renters were at a different point of their wealth life cycle.

From a comparison of their baseline characteristics, it is unclear whether renters would be more productive cultivating the Target Plot than owners. On the one hand, renters have a higher labor-land ratio and are younger and more educated. On the other hand, they are more likely to live in a different village than the Target Plot, and they may have less experience, both in farming in general (being younger) and especially in cultivating the Target Plot. In the next section, we harness the experimental design to answer this question and in Section 8.3 we compare our experimental result to a simple prediction based on baseline productivity.

6 Treatment effects on agricultural production

In this section, we study the treatment effects on agricultural outcomes. We begin with outcomes on the Target Plot, for which we present results on four groups of outcomes: *i)* the plot manager's decision to cultivate the plot (vs leaving it idle); *ii)* crop choice; *iii)* value of inputs, harvest, and value added; *iv)* soil quality. We then present results on owners' other plots.

6.1 Cultivation

As we discussed in Section 2, a sizable share of plots are left uncultivated. Over the four experimental seasons, 18% of the Target Plots were idle on average. Short Rains crop seasons have a higher rate of fallowing than Long Rains (24% vs 12%). Column 1 in Table 3 shows that both the rental subsidy and the unconditional cash transfer increased the likelihood of cultivation: the TOT coefficients were 8 p.p. and 6 p.p., respectively (from a control mean of 82%). The two treatment effects are statistically indistinguishable. Appendix Table E.11 shows that the treatment effect on cultivation rates was nil in the stratum where owners reported they were planning to cultivate the Target Plot in the first experimental season (Stratum C), while it was very large in the stratum where owners reported they were not planning to cultivate it (Stratum NC).

These two facts – that a sizable share of Target Plots was uncultivated and that the interventions affected cultivation rates – matter for the interpretation of the treatment effects on other

Target Plot outcomes, like crop choice, inputs, and output. To avoid selection concerns, we present treatment effects on *unconditional* outcomes, i.e., taking a value of zero, as opposed to missing, if the Target Plot was not cultivated in that season. However, we will also discuss several additional results which strongly suggest that changes in Target Plot outcomes are driven in part by intensive-margin adjustments, not just by the extensive margin of cultivation.

6.2 Crop choice

Columns 2 and 3 in Table 3 show that the treatments altered the Target Plot's crop portfolio. We focus on two dummies capturing cultivation of maize, the most important consumption crop in the study areas, and cultivation of any of the most important commercial crops (groundnuts, sugarcane, tobacco). Across the four follow-up surveys, the control mean was 0.69 for maize and 0.09 for commercial crops. The rental subsidy increased commercial crop cultivation significantly by 0.07 in the ITT and 0.1 in the TOT, while it had no effect on maize cultivation. The cash drop, in contrast, increased the likelihood of maize cultivation (0.05), but not of commercial crops. T-tests suggest that the difference in the treatment effects between the two groups is significant.[23] Appendix Table E.1 shows that these results are robust to alternative specifications where we vary the list of baseline controls, including specifications without any baseline controls or with controls selected from all Target Plot variables via post-double-selection (Belloni et al., 2014).

The patterns of substitution from maize to commercial crops are particularly transparent in Stratum C, where, as we discussed above, there was no treatment effect on cultivation rates relative to control. Appendix Table E.11 shows that, in this stratum, the rental subsidy reduced cultivation of maize and increased cultivation of commercial crops, while the unconditional cash transfer had no impact. In the NC stratum, both treatments increased the unconditional likelihoods of both maize and commercial crop cultivation, reflecting the increase in cultivation rates of the Target Plot. These patterns appear to be quite similar in the four follow-up seasons, except for the fact that in the first season the TOT of the rental subsidy had a larger treatment effect on cultivation rates (Appendix Figure E.2, panels (a)-(c)).

6.3 Inputs, output, and value added

We examine treatment effects on the total value of inputs (seeds, fertilizer and chemicals), household and hired labor, harvest, and value added (i.e., harvest value minus all the previously mentioned production costs) on the Target Plot. Table 4 presents the main results, first with

[23]We also study treatment effects on beans, the second most common consumption crop in the study area (grown by 17% of households in the control group), and find small and insignificant coefficients.

pooled season-level observations (odd columns) and then with the IHS of the total value of the variable across seasons (even columns, 2-8).

Inputs and Labor. The rental subsidy significantly increased the value of agricultural inputs on the Target Plot, while the cash drop had a smaller and noisy effect (Table 4, cols 1-2). The TOT effect of the rental subsidy is \$13.9 (s.e.=4.5), from a control mean of \$33, in the level specification and 0.34 (s.e.=0.13) in the IHS specification. The TOT coefficients are significantly different between the two treatments (p=0.01-0.08), which assuages concerns from the baseline imbalances in the control group vs the two treatment groups (see Section 3.5). Treatment effects on the value of hired and household labor are small and insignificant (cols 3-6).

In Appendix Table E.10, we also include results on the value and use of individual non-labor inputs used on the Target Plot. While both rental subsidy and cash drop increased the use of seeds, the TOT effect of rental subsidy is significantly higher than that of cash drop (\$10.5 vs \$3.6, p-value=0.01). Rental subsidy participants also used more inorganic fertilizers and pesticides, and less compost (on net, the combined value of fertilizer increases in the rental subsidy group).[24]

Harvest value and productivity. Treatment effects for harvest value on the Target Plot follow the same patterns as agricultural inputs. The rental subsidy significantly increased harvest value, while the cash drop had a smaller and noisy positive effect (cols 7-8). The TOT of the rental subsidy is \$44.3 (s.e.=13.7) in the level specification, from a control mean of \$96.3, and 0.39 (s.e.=0.15) in the IHS specification. The TOT coefficients are significantly different between the two treatments (p=0.01-0.04 in the TOT).

In turn, there is a significant treatment effect of the rental subsidy on value added (col. 9). The TOT is \$21.4 (s.e.=10.7), from a control mean of -\$6.4.[25] The treatment effect of unconditional cash transfer is small (TOT -\$0.9), insignificant, and differs significantly from the rental subsidy effect (p=0.03). The coefficients are stable when using alternative valuations of household labor (Appendix Figure E.1), though the control mean of value added depends heavily on the valuation choice, in line with the literature (see, e.g., Anagol et al., 2017; Bold et al., 2021).

Robustness Appendix Tables E.2-E.6 show that the results on inputs, harvest, and value added are robust to alternative specifications where we vary the list of baseline controls, including specifications that have no controls other than strata dummies, that only include plot size as an additional control, and that select controls via post-double-selection (Belloni et al., 2014). TOT coefficients

[24]Appendix Table E.10 also includes results on use of farm equipment. There is a small increase in ox-plough usage for both rental subsidy and cash drop, however, both estimates are noisy and not significantly different from each other. There is no effect on tractor use.

[25]Since value added takes negative values, we do not report the IHS specification for this outcome. However, we discuss distributional implications when presenting quantile treatment effects below.

of the rental subsidies remain large and significant across specifications, and consistently higher than cash drop TOT coefficients, further assuaging concerns about baseline imbalances that we discussed in Section 3.5. Additionally, results for harvest value and value added are robust to controlling for a dummy capturing non-verified planned harvests (see discussion in Section 3.4). The results are also robust to Lee Bounds (Appendix Table E.9), though the cash drop coefficient on value added (Column 8) becomes larger in the lower bound specification, making the difference between rental subsidy and cash drop noisier. Finally, Appendix Figure E.2 suggest that the rental subsidy TOT coefficients on non-labor inputs decreased over the four follow-up seasons (panel (d)), and the effects on labor inputs, harvest value and value added appear to increase (panels (e)-(h)), though the coefficients for individual seasons are somewhat noisy. We speculate this may be due to renters learning how to better cultivate the Target Plot over time, although other factors, like crop choices, could also be responsible for these trends.

Appendix Table E.14 also examines treatment effects on another measure of productivity on the Target Plot, total factor productivity (TFP). Consistent with Gollin and Udry (2021), we assume that Target Plot net revenues (harvest value minus the value of non-labor inputs) follow a Cobb-Douglas production function in land and labor.[26] In the TOT, the rental subsidy increased TFP by 36% of the mean (+6.1 from a control mean of 16.9). The results are robust when restricting the sample to Stratum C (where there was no treatment effect on the likelihood of cultivation) and when using alternative calibrations from Tanzania (Gollin and Udry, 2021), Malawi (Chen et al., 2021) and the U.S. (Valentinyi and Herrendorf, 2008).

Extensive and intensive margin responses. Our analysis has focused on treatment effects on unconditional outcomes, because restricting analysis to cultivated plots would have introduced selection concerns. However, two observations suggest that changes in Target Plot outcomes are driven in part by intensive-margin adjustments, not just by the extensive margin of cultivation. First, the rental subsidy and unconditional cash drop treatments have similar effects on cultivation rates, but different effects on agricultural inputs, harvest, and value added. Second, we find similar treatment effects on these outcomes, though noisier, in Stratum C, even if there is no treatment effect on cultivation rates in this stratum (see Appendix Table E.12 for details).

Quantile regressions. The consistency between the specifications in levels and with IHS suggests that the previous results are not driven by outliers. We use quantile regressions to shed

[26] We highlight several important caveats in our study of TFP: i) The assumption of a common production function is particularly problematic in our setting, given that treatment changed crop portfolios; ii) the TFP is defined only when the plot is cultivated, which is potentially problematic, given that treatments affect selection into cultivation; iii) since our data does not include credible instruments for input use, we cannot estimate the production function and we instead calibrate it using factor shares that Gollin and Udry (2021) estimate in Uganda.

further light on the distributional impact of the treatments. We again instrument whether the respondent received any rental subsidy or unconditional cash transfer payment during the experiment with the treatment assignment dummies. Appendix E.3.6 presents the results. For input value, harvest value, and value added, the rental subsidy TOT coefficients become positive and significant around the median and are mostly increasing in percentiles. For example, the treatment effect on harvest value is $22 at the median and $144 at the 95th percentile (for comparison, the TOT in Table 4 is $44). The rental subsidy appears to have a negative effect in the lowest (5th-10th) percentiles. Finally, the difference between rental subsidy and cash drop is large and sizable above the median, though we are somewhat underpowered to detect it. The value of hired labor exhibits a similar pattern, though less pronounced. The treatment effect on household labor is instead quite flat and not significant throughout the distribution.

Measurement. Before concluding this section, we discuss several points related to outcome measurement. In a recent paper, Aragón et al. (2022) suggests that using the plot as a unit of analysis, as opposed to the farm, may lead to excess measurement error. This may inflate the extent of measured dispersion and measured misallocation. However, our analysis does not rely on measures of dispersion. For our purpose, the presence of excess measurement error at the plot-level relative to the farm-level may increase standard errors, thus reducing the precision of our estimates, but it would not affect estimation of the treatment effect coefficients.

Concerns related to measurement may nevertheless arise if renters were more likely than owners to over-report input use and harvest on the Target Plot. Several considerations mitigate this concern. First, renters had no financial incentive to misreport outcomes at endline, since the rental subsidy was not contingent on plot use. Second, treatment effects on cultivation and crop choice (e.g., on commercial crop cultivation) are unlikely to suffer from this problem. Third, a final concern is that farmers for whom the Target Plot represents a smaller portion of their farm may underreport quantities on it. If this were the case, Target Plot owners (who have more plots) may underreport quantities on the Target Plot compared to renters. To explore this concern, we examine the relation between the farm size and the reported input use and output on the Target Plot at baseline, controlling for Target Plot size. Contrary to the concern discussed above, owners for whom the Target Plot covers a smaller portion of the farm report *higher* values of inputs and output on the Target Plot.[27] This would suggest that reporting errors would result, if anything,

[27]The average value of agricultural inputs per acre is 54.3 USD and the coefficient on the number of cultivated plots is 12.8, significant at the 1% level, a 24% increase for an extra plot cultivated. The average value of harvest per acre is 106 USD and the coefficient on the number of cultivated plots is 41.2, significant at the 1% level, a 39% for an extra plot cultivated. We obtain similar results when we measure the size of the farm with total acreage rather than the number of plots.

in a downward bias in the treatment effects of rental subsidies on inputs and harvest.

6.4 Soil quality

As discussed in Section 3.4, we collected soil results for each Target Plot at the end of seasons 1 and 4. Following Burchardi et al. (2019), we constructed a soil quality index based on the results of the soil tests. This index combines measurements of nitrogen, phosphorous, potassium, organic matter, and the pH level of the soil by first standardizing each measurement into a z-score, taking the mean of the plot's z-scores and then standardizing again against the control group.

The results in Column 10 of Table 4, which pool together the two seasons, indicate no significant soil quality differences across the treatment and control groups. While there is a slight deterioration in measured soil quality under the rental subsidy, the TOT coefficient of -0.02 is not significant. Appendix Figure E.2 panel (i) possibly suggests a more negative impact of rental subsidy on soil quality at the end of the season 4, but the coefficient is noisy. Table E.7 shows robustness and Table E.13 presents results for each nutrient of the index.

6.5 Treatment effects on owners' other plots

Although the rental subsidy was specific to the Target Plot, both treatments could have affected agricultural outcomes on other plots. Our data collection strategy enables us to study outcomes on most, but not all, of owners' non-Target Plots: we only measure agricultural outcomes of non-Target Plots if the owner manages them, not if she rents them out (because, unlike in the Target Plot, we do not interview the renters of non-Target Plots). Therefore, we first report treatment effects on the likelihood that the non-Target Plot is rented out and then we report treatment effects on other plot outcomes, conditional on the plot not being rented out in that season.

Rentals of non-Target Plots. Column 1 of Table 5 shows that neither of the treatments affect the likelihood that the owner rents out a non-Target Plot: the treatment coefficients are 0.01 (s.e.=0.01) from a control mean of 0.05. This result has two implications. First, as we discussed in Section 4.3, the increased rentals of Target Plots in the rental subsidy treatment does not displace rentals of other plots. Second, the fact that rental rates of non-Target Plots are similar across treatment groups mitigates selection concerns in the analysis of other non-Target Plot outcomes, which we observe only when the plot is not rented out.

Cultivation, crop, inputs, and output on non-Target Plots. Columns 2-9 of Table 5 show treatment effects on other non-Target Plot outcomes, conditional on the owner managing the plot (i.e., not renting it out). There is some suggestive evidence that households may use some of the unconditional cash transfer to increase inputs in non-Target Plots and that owners in the rental subsidy group may reallocate labor from the Target Plot to their non-Target Plots. However, these effects are only marginally significant and, overall, the treatments do not have sizable effects on cultivation, crop choices, investments, and output in non-Target Plots.

6.6 Treatment effects on agricultural production: discussion

The rental subsidy and the unconditional cash transfer have similar treatment effects on the likelihood that the Target Plot is cultivated. However, only the rental subsidy induces a shift toward commercial crops. Rental subsidies also have larger treatment effects than unconditional cash transfers on inputs and output on the Target Plot. This suggests that, for owners on the margin, inducing plot rentals may be a stronger push towards market participation and agricultural productivity than supporting plot owners, in line with some of the reallocation arguments highlighted by recent literature (e.g., Adamopoulos et al., 2022b).

The treatment effects on Target Plot outcomes differ from the results of longitudinal studies in Kenya by Yamano et al. (2010) and Muraoka et al. (2018), which find that land productivity and input use are lower in rented parcels, possibly due to worse unobservable land quality in rented plots.[28] Our results are in line with recent papers showing that reforms in land rights (Chari et al., 2021; Chen et al., 2022) or in the administration of land records (Beg, 2022) improved agricultural efficiency, arguably because they increased the volume of rentals.

We see little to no spillovers of the Target Plot rentals on owners' other plots —neither on their decision to rent out or cultivate, nor on their input usage. If owners faced constraints in input use, this suggests that they typically addressed them by reducing inputs on the Target Plot, rather than across all plots. The increase in cultivation rates on the Target Plot (and not other plots) in the cash drop group is also consistent with this observation. Finally, as discussed in Section 3.2, our experimental design does not allow an analysis of spillovers on renters' other plots.

7 Treatment effects on owner outcomes

In this section, we study treatment effects on owners' non-agricultural outcomes. Our follow-up surveys collected data on owners regardless of whether they were managing the Target Plot or renting it out. We consider four families of outcomes: *i)* labor supply outside of the farm, *ii)* household assets, *iii)* food security, and *iv)* household finance. Results are reported in Table 6.

Labor supply. When households rent out the Target Plot, there are two main mechanisms

[28]Other studies suggest that renters are more likely to be high-ability farmers (see, e.g., Deininger and Mpuga, 2003; Jin and Jayne, 2013; Chamberlin and Ricker-Gilbert, 2016), but do not attempt to measure the effects of the change from owner- to renter- management on parcel outcomes.

which could affect their labor supply outside the household farm. First, they may have lower labor demand on their farm, freeing up labor which could then be used elsewhere, potentially acting as a push factor towards structural transformation. Second, there is a potential income effect, since the household receives more income early in the season, from rent, and then less income at harvest time, from a lower yield. The combined effect of these two mechanisms is thus ambiguous.

We consider effects on both agricultural and non-agricultural labor supply. Both treatments have little effect on agricultural labor supply outside of the household farm, as shown in Column 1 of Table 6. Point estimates are less than one person-day over the agricultural season, and standard errors are small, ruling out an economically meaningful effect. However, the rental subsidy has a meaningful effect on non-agricultural labor supply, shown in Column 2, with the TOT point estimate of 9.5 fewer person-days, on a control mean of 38.7 person-days. The TOT point estimate of the cash drop is also negative but smaller and insignificant, at 4.7 fewer person days.[29] These results suggest that the income effect dominates any labor supply effect, such that overall labor supply falls, with no evidence of any effect on structural transformation out of agriculture. Consistent with this, Column 3 shows no meaningful effect on working outside of the village, with a similar null result for migration reported in the appendix.

Household assets. Columns 4 and 5 report treatment effects on two sets of assets. First, whether the household owns any livestock (oxen, cow, or bull). Second, the principal component of a standard list of household assets (excluding animals) and amenities, such as radios, televisions, motorbikes, metal roofs, and improved walls. We observe no meaningful effect of the rental subsidy nor the cash drop on either asset measure, suggesting that renting out did not have a transformative effect on household wealth.[30]

Food security. The rental subsidy induced farmers to rent out one of their plots, in turn reducing their total cultivated land and in particular the amount used to cultivate staple crops. Did this affect their food stocks and food security? Columns 6 and 7 report effects on whether the household had stocks of maize from their own production in the last 6 months, in Season 1 and Seasons 2-4, respectively. During seasons 2-4, the rental subsidy led to a reduction in maize stocks from own production in the last 6 months, consistent with a reduction in production. In Season 1, in contrast, the rental subsidy led to more households holding maize stocks from their own production. Two possible explanations are that receiving income (rent plus subsidy) early in

[29] Splitting these results by different parts of the season, into harvest time, slack season, and planting time (not reported), shows no obvious pattern on the seasonality of labor supply outside of the household farm.

[30] We did not collect consumption data. The change in the seasonality of income induced by renting out would have made interpreting consumption results from any one point in time difficult, and it was infeasible to collect consumption data at high frequency.

the season may have reduced the need to sell maize straight away, enabling households to benefit from seasonal price fluctuations (Burke et al., 2018), or that households anticipated having less maize from subsequent harvests, when renting out, and stored more maize for this future need.

We find no effect on whether the household experienced hunger episodes in the last six months, as reported in Column 8. The control mean is relatively low, 0.16, and we can reject an effect size of 6 p.p., suggesting that the treatments had minimal effect on food security, perhaps unsurprisingly given owners' relatively large landholdings and the subsidy being limited to one plot.

Household finance. Despite the rental subsidy changing the timing of agricultural income, we do not see meaningful effects either on whether the household would have 5k Ksh to cover an emergency, Column 9, nor on whether it has borrowed in the last 6 months, Column 10.

7.1 Treatment effects on owners: discussion

We see no substantial effects on measures of owners' well-being, including food security, assets, and household finances. Land market participation did not seem to induce structural transformation by untying people from their land (Gottlieb and Grobovšek, 2019; Fernando, 2022): rental subsidies led to no change in working outside of the village or migration, and a *decrease* in non-agricultural labor. This contrasts results from papers on land markets and structural transformation in China (see, e.g., Jin and Deininger, 2009, for panel data estimation and Adamopoulos et al., 2022a, for quantitative evaluation) and suggests that, in our setting, marginal owners who rent out land may have more limited opportunities in the non-agricultural sector. In addition, our intervention only subsidized the rental of one plot, for farmers who owned at least two; larger-scale interventions may of course have more transformative effects on owners' livelihoods.

8 Understanding and quantifying land market frictions

In this section, we discuss how the experimental results shed light on the nature and magnitude of market frictions in agriculture. We first discuss the sources and duration of land frictions, and their interactions with frictions in other input markets. We then quantify land rental frictions and compare the experimental treatment effects to predictions from a misallocation quantification based on productivity dispersion (Hsieh and Klenow, 2009; Adamopoulos et al., 2022b).

8.1 Understanding land rental frictions

While the source of land market frictions is not the focus of the experimental design —indeed, our intervention is agnostic on which frictions it addresses and could offset several at once —our results are informative of which frictions bind at the margin. The first relevant result is that most induced rentals persisted beyond the subsidy period. This suggests that frictions had a substantial fixed cost component, in addition to any per period component, such as search costs, initial transaction costs, or learning about plot quality and optimal inputs.

Turning to individual sources of frictions, during the listing exercise farmers emphasized search costs, the risk of soil exploitation and land disputes, and asymmetric information over soil quality. Search costs were the most reported constraint and were said to be substantial on both sides of the market: finding owners willing to rent out and renters willing to rent in. Consistent with this, 30% of the owners offered the subsidy did not rent out, despite initially expressing interest in it, with 87% of these reporting not being able to find a renter as the reason. Even among those who did rent out, finding a renter often took several weeks.

Regarding perceived risks, while soil exploitation was a concern at baseline, we find little evidence of degradation in soil quality from the rental subsidy. Similarly, there is little evidence of land disputes occurring in the experimental rentals, despite it being a concern at baseline. These apparent differences between perceived risks and what subsequently happened may suggest a misperception of risks and a corresponding benefit from inducing experimentation, but they may also reflect a very high cost of disputes or soil exploitation, even if rare. Regardless, learning about these risks could explain the observed persistence of rentals, if perceived risks reduce once rental relationships become established. Alternatively, if the risks are match specific, they may entail high search costs, with owners needing to find renters who are *both* interested and trusted.

Finally, regarding renters learning about their productivity on the Target Plots, and asymmetric information about soil quality, in Appendix Table C.4 we compare the characteristics of rentals which terminate after at most three seasons (the subsidy period) to those which persist for four or more. Revenue and value added on the Target Plot are substantially lower for rentals which terminate, yet this is not reflected in the rental price, which is the same across the two groups, pointing to the role of learning about productivity. Does this reflect asymmetric information about plot quality, or learning about match quality? Turning to baseline outcomes, there is no difference in Target Plot revenue achieved by owners across the two groups, although soil quality is possibly marginally better on Target Plots where rentals subsequently persist. This (non-experimental) analysis thus suggests that renters decide whether to continue renting the Target Plot after learning their productivity on it, with a seemingly limited role for asymmetric information about plot quality.

8.2 Interactions between land rental frictions and other markets

Land market frictions only matter if farmers also face constraints in other markets. A first candidate is *labor* markets. Renters had similar household size than owners but much less land, leading to a higher labor-land ratio. Nevertheless, they did not spend more labor days on the target plot. If anything, renters reduced the amount of household labor (in Stratum C, see Appendix Table E.12). These patterns suggest that renters were not facing a lower shadow wage; if anything, the fact they could achieve higher harvest value without increasing labor is suggestive of surplus labor among owners (Lewis et al., 1954; Breza et al., 2021). Renters may have better *management* skills: at baseline they were younger, more educated, and more market-oriented; and analysis of the experimental seasons suggests that they make better crop choices and obtain higher returns on their investment (higher value added and TFP on the Target Plot). The results are also consistent with the importance of *capital* constraints: owners increase cultivation rates in response to the unconditional cash transfer; and renters have more access to finance at baseline, take loans to rent the Target Plot, and invest more in seeds and fertilizer. Consistent with this result, the unconditional cash drop also increased cultivation of the Target Plot. Finally, we consider *economies of scale*. The rental subsidy treatment improved outcomes despite not inducing land consolidation (Foster and Rosenzweig, 2022; Bryan et al., 2019): renters were more likely to reside in different villages and very few were managing other plots contiguous to the Target Plots. This suggests our findings are not driven by increasing returns to scale in crop production.

8.3 Quantifying land rental frictions

Even with a 30% rental subsidy, only 16% of farmers were interested in the rental subsidy. Therefore, a relatively small set of owners was on the margin of land market participation —for most owners, land rental frictions appear to be large (e.g., a perceived large risk of expropriation) relative to any potential gains from trade.

The size and incidence of frictions among marginal rentals. We use the simple framework from Section 3.2.1 to quantify land market frictions for the marginal rentals induced by the experiment, through three back-of-the-envelope exercises. First, did the subsidy induce trades which would have positive gains in a frictionless world? In principle, the experiment could have induced inefficient trades (if $s > \tau$), but the fact that the treatment on treated is positive in Table 4 implies that the subsidy induced rentals that should occur absent frictions (i.e., for which $\Delta > 0$). The TOT is also larger than the value of the subsidy ($11.5 per acre per season, paid for three seasons), but this is not sufficient to argue that the subsidy induced welfare gains, since the parties may incur (unobserved) friction costs when transacting. Indeed, in our second back-of-the-envelope exercise, we bound the value of the average land frictions among marginal rentals: using estimates of the local average treatment effects of marginal rentals on per-acre value added, we obtain $\tau \in (45.1, 53.7]$, or 133%-159% of the average rental price ($33.7 per acre).[31] Third, we compare value added among marginal renters ($40 per acre) to the average rent they pay per season ($33.7 per acre). Plugging these numbers into the owner's and renter's participation constraints suggests that owners bear the majority of any friction costs (i.e., $\alpha > 0.9$).[32]

These back-of-the-envelope exercises are speculative at best given the simplicity of the framework. We are ignoring dynamics and uncertainty, which are likely to be important. Drawing firmer conclusions would also require measuring data in subsequent seasons, given that the relationships from the rental agreements persist beyond the four seasons in which we collected data; the positive effects on value added may continue, or alternatively the additional activity on Target Plots may begin to impair soil quality.

Comparison to predicted gains from full reallocation, based on productivity dispersion. A popular method to quantify the cost of misallocation from land frictions is to estimate the dispersion in productivity across farms, and then to predict the hypothetical gains from reallocating land across them until productivity is equalized (Adamopoulos et al., 2022b). While a fundamentally different exercise from our experiment, it is informative to compare such a model-based prediction, using our baseline data, to our estimated treatment effects. We perform this comparison in two steps, outlined in more detail in Appendix F.

First, based on baseline measures of productivity, we compare the predicted effects of full reallocation among our sample to the predicted effects of the actual trades induced by the experiment.[33] Simply plotting baseline productivity dispersion, Appendix Figure F.1 shows wide dispersion among both owners and renters, with the renters' distribution shifted to the right, suggesting gains from both full reallocation and from the experimental rentals. To quantify these predicted gains, we fit a production function to baseline data and use it to predict treatment

[31] While the discussion earlier in this section suggests a large fixed (vs per-season) component of the friction, we present per-season values of τ for ease of exposition. We use the (lower-bound) estimate of the local average treatment effect of marginal rentals on value added, controlling for the income effect of the subsidy: $45.1 per acre per season, over four seasons (see discussion in Section 3.7 and Appendix B). To make the subsidy value comparable to the value-added results, we compute a measure of subsidy paid per season over four seasons ($11.5*3/4=$8.6).

[32] The insights from this exercise depend heavily on the valuation of household labor. We use the benchmark valuation of the household labor at 60% of hired labor (Agness et al., 2022). While the estimated treatment effect is quite stable to alternative valuations, average value added varies substantially (see Appendix Figure E.1). For instance, when valuing household labor at zero, value added among renters is $91 per acre, instead of $40.

[33] We would like to have the universe of farmers, and compare gains from reallocation in that universe to gains from the induced trades, but we only have baseline data for the interested owners in our sample, and the farmers that they rent out to. Hence we benchmark our induced trades to the potential gains within the control group.

effects of land reallocations. We predict that the rentals induced by the subsidy would increase total revenue by 1.6% (arising from approximately 9% of total land changing management, and consistent with our experimental treatment effects, as detailed in the next paragraph), while full reallocation would increase it by 205%, in line with predicted gains from other settings (e.g., Chen et al. 2021). Given data limitations, the latter is likely biased upwards by measurement error. However, it demonstrates the gulf between the two exercises, driven both by constraints on which rentals our experiment can induce (only owners can rent out, and only up to one plot) and by the induced rentals not being those with the largest predicted gains, for instance due to search costs.[34]

Second, for the induced rentals, we compare their predicted effects on average Target Plot revenue to the experimental treatment effects. The experimental effects (a 28% increase, based on the ITT in Table 4, col. 8) are consistent with, and, if anything, slightly larger than, the predicted effects (a 13 to 32% increase, depending on how we translate the farm-level prediction into a prediction for the Target Plot).[35] To summarize, our induced rentals increase productivity, but by substantially less than the predicted effect of full reallocation. This difference appears to arise from a difference in the set of rentals which occur, rather than a difference between their predicted and actual effects on productivity.

9 Conclusion

Across much of sub-Saharan Africa, agriculture is the main source of livelihood for the majority of the poor, yet markets for its key input, land, feature many imperfections. Limited land market participation is argued to have important implications for the efficiency and equity of agriculture and is also central to many other economic aspects of rural life in developing countries. However, experimental evidence on the effects of land market participation is virtually non-existent.

In this paper, we study the effects of incentivizing land rentals in Western Kenya. Approximately 16% of landowners expressed an interest to rent out an extra plot if receiving a subsidy worth around 30% of the average rental rate. Interested farmers owned more land and left a higher share of their plots unused, often due to lack of inputs. For this subset of owners, which constitutes our main study sample, the rental subsidy led to a large increase in the likelihood of renting out a plot, which persisted after the end of the incentive. Consistent with the argument that land rental

[34] They cannot be those with the largest predicted gains: full reallocation reallocates at most one order of magnitude more land than the induced rentals (which reallocate 9% of land), while the predicted gains are two orders of magnitude larger.

[35] The latter correspond to the 1.6% increase in total revenue detailed above —for example, if we assume the treatment effect was confined to the Target Plot (i.e., no spillovers to other plots) then, since the Target Plots account for approximately an eighth of total revenue at baseline, the treatment effect on Target Plot revenue is $\approx 8 * 1.6\% = 12.8\%$.

markets can equalize access to land, renters owned fewer plots on average than owners while they had similar household sizes. They were also younger and more educated. The renters increased commercial crop cultivation and non-labor inputs on the rented plot, achieving higher yields and ultimately higher value added. These effects were larger than those of an unconditional cash transfer to plot owners. Finally, participating in land rental markets had no meaningful effect on the food security of landowners and, if anything, reduced their non-agricultural work.

While the subsidy may overcome multiple frictions at once, the experimental results shed light on the nature and magnitude of land frictions. The persistence of rentals after the subsidy ends suggests that land frictions have a large fixed-cost (vs. per-season) component, plausibly driven by search costs in thin markets. The subsidy may have also fostered experimentation (for both owners and renters) and renter's learning about productivity on the rented plot. The shift in plot management from owners to renters appears to improve agricultural outcomes due to renters' better access to capital and, possibly, higher management skills. Other explanations based on labor constraints and economies of scale do not find support in the data. Turning to the magnitude of the frictions, simple back-of-the-envelope-exercises suggests that the rentals induced by the subsidy had gains from trade that were positive (and larger than the value of the subsidy), and hence should have taken place in a frictionless market, although they do not appear to be those trades with the highest potential revenue gains. A bounding approach suggests that frictions are at least as large as the observed rental price and that owners bear most of the friction costs.

We conclude by highlighting three areas for future work. First, our goal was to induce marginal rentals and study their effects; a subsidy was the natural tool to achieve it. There may of course be more cost-effective ways to improve the functioning of land markets. Further work may aim to study the impact of addressing specific frictions, including search costs (as in ongoing work in Rwanda by Karpe et al., 2019), asymmetric information over land characteristics, renter moral hazard, and the risk of land disputes. Second, interventions at scale may have different results, for example through general equilibrium effects. Our experiment is unable to capture such effects (e.g., on rental prices), nor spillovers to renters, and so care should be taken in drawing policy conclusions from it. Future research may aim to measure these effects; doing so would require a substantially different design and larger budget, if addressed experimentally. Third, our experiment induces rentals that are limited in size (<1 hectare on average) and duration (1-3 years, at least initially). While such rentals, which are in line with other rentals occurring in the study area, are shown to have sizable effects on agricultural production, the experimental exploration of large-scale and long-term leases remains an important area for future research.

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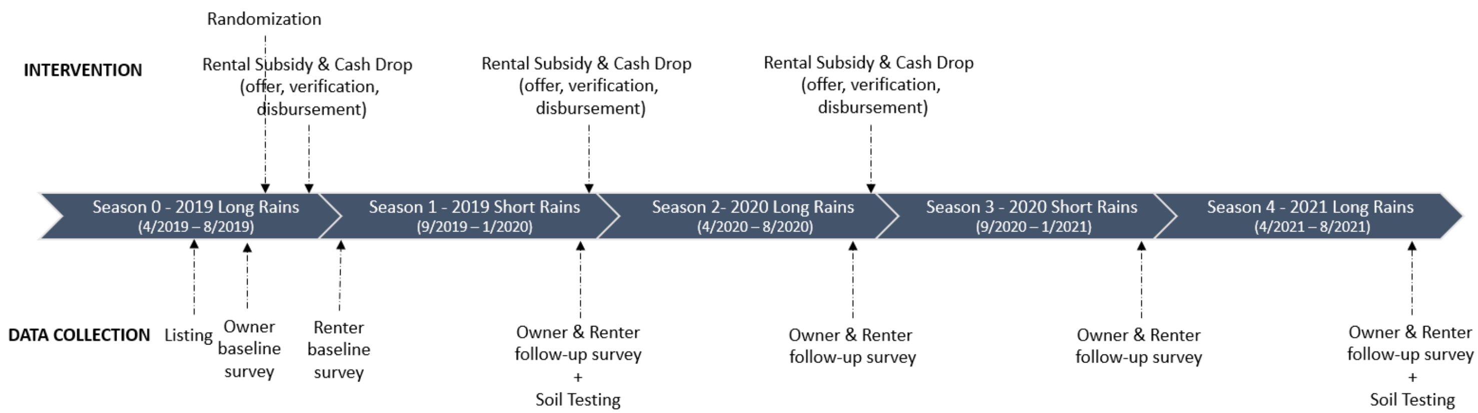
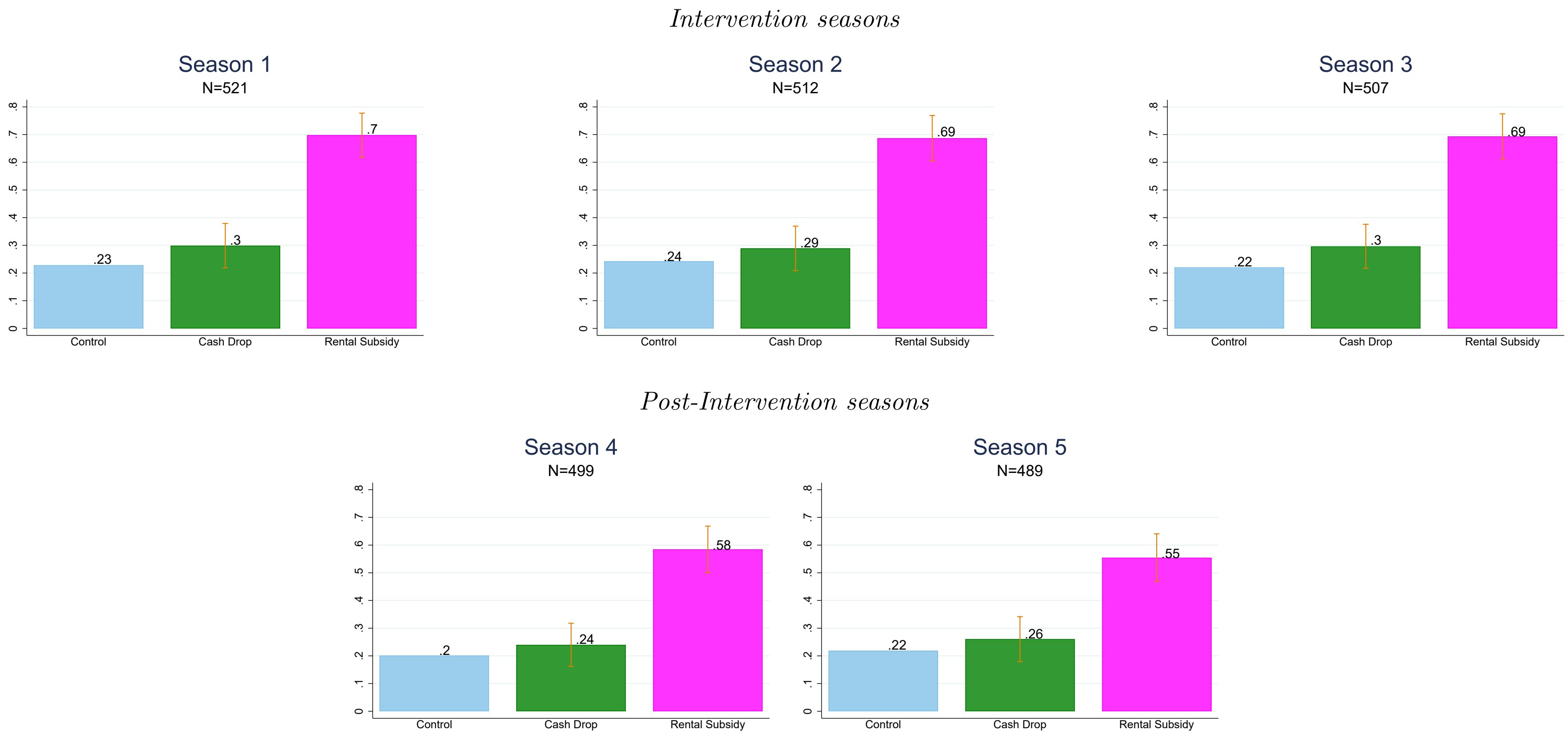
Figures

Figure 1: Timeline of intervention and data collection



Notes: This figure details the key intervention and data collection activities of the study. Activities were conducted within two annual agricultural seasons: the Long Rains and the Short Rains. Season 0 refers to the baseline period, while Seasons 1-4 are the seasons in which we collected follow-up data. We offered the treatments in Seasons 1-3. The dotted arrows show the sequence and timing of activities.

Figure 2: Target Plot Rentals



Notes: The figure reports the proportion of Target Plots rented out by treatment group by agricultural season. The rental subsidy was offered for up to three seasons (seasons 1 to 3). The data comes from the follow-up surveys we ran at the end of seasons 1 to 4 with the manager of the Target Plot. In the last survey round (season 4), we also asked about rentals for the upcoming season 5. In a handful of cases, we gathered information on the rental status of the Target Plot even if we could not complete the full follow-up survey. The number of observations varies by round due to attrition (see Appendix Table E.8) or because information on the rental status in the upcoming season was not available at the time of the survey (for season 5). The bars report 95 percent confidence intervals from a regression of an indicator equal to one if the Target Plot is rented out on dummies for the treatment groups.

Tables

Table 1: Selection of farmers and plots into renting out

Panel A: Selection of Farmers	Interested in rental subsidy [I]	Not interested [NI]	[I-NI]	N
Male	0.68	0.61	0.07	5,425
	(0.47)	(0.49)	(.01)***	
Age	50.76	49.37	1.39	5,418
	(14.98)	(15.81)	(.55)**	
Has a Phone	0.91	0.84	0.07	5,425
	(0.29)	(0.37)	(.01)***	
No. Plots Owned	3.50	2.87	0.64	5,425
	(1.33)	(1.05)	(.04)***	
Acres Owned	4.09	3.56	0.53	5,425
	(3.60)	(3.86)	(.13)***	
Renting out at least one plot	0.09	0.03	0.06	5,425
	(0.28)	(0.16)	(.00)***	
No. Plots Rented Out	0.10	0.03	0.07	5,425
	(0.35)	(0.20)	(.01)***	
Share of plots fallowed	0.08	0.02	0.06	5,425
	(0.16)	(0.10)	(.00)***	
Share of plots cultivated with cash crops	0.07	0.07	-0.01	5,425
	(0.10)	(0.10)	(.00)**	
Panel B: Selection of Plots	Target Plot [T]	Non-Target Plots [NT]	[T-NT]	N
Plot Size	0.79	0.75	0.02	1,898
	(0.55)	(1.04)	(0.04)	
Respondent's homestead in different village than plot	0.01	0.03	-0.01	1,898
	(0.12)	(0.17)	(0.01)	
Sandy loam soil	0.54	0.53	0.01	1,898
	(0.50)	(0.50)	(0.01)	
Sandy clay soil	0.27	0.31	-0.03	1,898
	(0.44)	(0.46)	(0.01)***	
Irrigation dummy	0.06	0.05	0.01	1,898
	(0.23)	(0.22)	(0.01)	
Cultivated in 2019 Long Rains	0.60	0.79	-0.19	1,898
	(0.49)	(0.41)	(0.02)***	
Rented out in 2019 Long Rains	0.12	0.06	0.06	1,898
	(0.32)	(0.24)	(0.01)***	
Cultivated with maize in 2019 Long Rains	0.49	0.45	0.01	1,898
	(0.50)	(0.50)	(0.03)	
Cultivated with commercial crops in 2019 Long Rains	0.04	0.09	-0.04	1,898
	(0.20)	(0.29)	(0.01)***	
Value of agricultural inputs in 2019 Long Rains	34.5	46.3	-9.4	1,883
	(71.7)	(284.9)	(7.4)	
Value of household labor in 2019 Long Rains	29.27	27.30	1.97	1,042
	(42.64)	(39.90)	(2.42)	
Value of hired labor in 2019 Long Rains	13.0	9.0	4.0	1,041
	(26.6)	(18.5)	(1.2)***	
Cultivated in 2018 Short Rains	0.54	0.69	-0.15	1,898
	(0.50)	(0.46)	(0.02)***	
Rented out in 2018 Short Rains	0.10	0.06	0.04	1,898
	(0.29)	(0.24)	(0.01)***	
Harvest value in 2018 Short Rains	70.5	106.0	-22.8	1,898
	(185.2)	(743.3)	(16.1)	

Notes: **Panel A** compares the plot owners interested in the rental subsidy against the plot owners who were not interested. The data comes from the listing survey. We report statistics for the owners who owned at least two plots and could thus become eligible for the subsidy if interested (5,425 out of 7,545). *Male* is a binary indicator equal to one if the owner was male. We winsorize *Acres Owned* at the top 1%. *Share of plots cultivated with cash crops* is the share of plots on which the owner is cultivating groundnuts, tobacco or sugarcane. The *[I-NI]* columns are generated by a regression of each outcome on an interested dummy with robust standard errors. *p<0.1, **p<0.05, ***p<0.01. **Panel B** compares plot characteristics for Target Plots against non-Target plots for the 521 study owners. The data comes from the owner baseline survey: in the study sample, there are 521 Target Plots and 1,377 non-Target plots. *Plot Size* is the reported plot size in acres. *Cultivated with commercial crops in 2019 long rains* is a binary indicator equal to one if the plot was cultivated with groundnuts, tobacco or sugarcane during the long rains 2019. Value of agricultural inputs, household labor, hired labor and harvest are expressed in USD (1 USD = 100 KSh) and winsorized at the top 1%. Since we needed to conduct the baseline survey while the 2019 Long Rains harvesting was ongoing, we do not have information on harvest amount for that season for most of the sample. *Value of agricultural inputs* is the value of any seeds, compost, chemical fertilizer, and pesticides used on the Target Plot. At baseline, we only collect labor variables for one non-Target Plot, hence the lower number of observations. *Value of hired labor* is the number of hired-work days valued at the median reported wage. *Value of household labor* is the number of household-member-work days, valued at 60% of the median reported wage. The difference *[T-NT]* is the coefficient from a regression of each outcome on a binary indicator equal to one if the plot is the Target Plot, including owner fixed effects. *p<0.1, **p<0.05, ***p<0.01.

Table 2: Distributional effects of rentals: manager characteristics

	(1)	(2)	(3)	(4)
Panel A: Demographics and Education	Household Size	Age	Male	High School Educated
ITT				
Rental Subsidy	0.10	-3.83***	0.12***	0.07*
	[0.25]	[1.24]	[0.04]	[0.04]
Cash Drop	-0.27	-1.32	0.09***	0.04
	[0.19]	[1.02]	[0.03]	[0.03]
p-value Rent = Cash	*0.11*	*0.05*	*0.32*	*0.50*
LATE				
Plot Rented	0.21	-7.83***	0.26***	0.14**
	[0.44]	[2.15]	[0.07]	[0.07]
Mean Y in Control Group	5.75	48.98	0.69	0.24
Observations	508	508	508	508
Panel B: Agricultural Land and Practices	N. Plots Owned	Rent In Plot(s)	S. Plots Cash Crops	Target Plot in Diff. Village
ITT				
Rental Subsidy	-0.91***	0.15***	0.02	0.09***
	[0.15]	[0.04]	[0.03]	[0.03]
Cash Drop	-0.13	0.04	-0.01	0.03
	[0.14]	[0.03]	[0.02]	[0.03]
p-value Rent = Cash	*0.00*	*0.01*	*0.15*	*0.05*
LATE				
Plot Rented	-1.87***	0.30***	0.05	0.19***
	[0.21]	[0.06]	[0.05]	[0.05]
Mean Y in Control Group	3.21	0.07	0.11	0.05
Observations	508	508	467	506
Panel C: Food Security, Wealth and Finance	Experienced Hunger	Wealth Index	Borrowed	Emergency Savings
ITT				
Rental Subsidy	-0.06*	-0.12	0.12***	0.04
	[0.03]	[0.09]	[0.04]	[0.04]
Cash Drop	-0.02	0.07	0.02	0.05
	[0.03]	[0.09]	[0.04]	[0.04]
p-value Rent = Cash	*0.29*	*0.05*	*0.02*	*0.94*
LATE				
Plot Rented	-0.13**	-0.25	0.24***	0.09
	[0.06]	[0.17]	[0.07]	[0.07]
Mean Y in Control Group	0.33	-0.01	0.62	0.40
Observations	508	504	508	508

Notes: The table reports treatment effects on the characteristics of the target plot manager. The dependent variables correspond to the *baseline* characteristics of whomever is managing the Target Plot in the first endline season (2019 Short Rains): the owner if the plot is not rented out, in which case the data comes from the owner baseline survey, and the renter if the plot is rented out, in which case the data comes from the renter baseline survey (which was performed approximately one month later than the owner baseline survey). **Panel A** reports demographic and education characteristics: the number of household members (col. 1), and for the household head their age (col. 2), gender (indicator function for male) (col. 3) and whether they are high school educated (col. 4). **Panel B** reports agricultural characteristics: the number of plots owned (col. 1), an indicator variable equal to one if the manager rents in any plots (col. 2), the share of cultivated plots which are cultivated with cash crops (col. 3, set to missing if the number of plots cultivated is 0), and an indicator for whether the Target Plot was in a different village to their house (col. 4). **Panel C** reports food security, wealth, and finance: an indicator variable equal to one if they experienced a hunger period in the last 12 months (col. 1), the standardized principal component of a vector of assets and amenities (excluding land and livestock) (col. 2), and indicator variables equal to one if they have borrowed in the last 12 months (col. 3) and if they had enough savings to cover an emergency expenditure of 5,000 Ksh (\$50) (col. 4). In the *ITT* sub-panels, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome (noting that these will be equal to the outcome itself when the Target Plot is not rented out), plot size and strata dummies (see Equation (B.2) in the Appendix). In the *LATE* sub-panels, we run an ANCOVA regression with the same controls, but we instrument the dummies for whether the Target Plot was rented out with the Rental Subsidy treatment, while controlling for the Cash Drop treatment (see Equation (B.3) in the Appendix). *$p<0.1$, **$p<0.05$, ***$p<0.01$.

Table 3: Target Plot outcomes: plot use and crop choice

	Cultivated	Maize	Commercial
	(1)	(2)	(3)
ITT			
Rental Subsidy	0.06***	-0.01	0.07***
	[0.02]	[0.03]	[0.02]
Cash Drop	0.06***	0.05	0.02
	[0.02]	[0.03]	[0.02]
p-value Rent = Cash	*0.90*	*0.05*	*0.02*
TOT			
Rental Subsidy Paid	0.08***	-0.01	0.10***
	[0.03]	[0.04]	[0.03]
Cash Drop Paid	0.06***	0.05	0.02
	[0.02]	[0.03]	[0.02]
p-value Rent = Cash	*0.47*	*0.07*	*0.00*
Mean Y in Control Group	0.82	0.69	0.09
Observations	1,957	1,956	1,956

Notes: The table reports treatment effects on indicators equal to one if the Target Plot is cultivated (col. 1), cultivated with maize (col. 2), cultivated with commercial crops, i.e., groundnuts, sugarcane, tobacco (col. 3). The data comes from follow-up surveys we run at the end of seasons 1 to 4 with the manager of the Target Plot. We pool observations from the four rounds of surveys. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome in the 2019 Long Rains and 2018 Short Rains, plot size, survey-round dummies, and strata dummies (see Equation (1) in the paper). In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the plot owner took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

Table 4: Target Plot outcomes: inputs, output and soil quality

	Value of Inputs		Value of Household Labor		Value of Hired Labor		Harvest Value		Value Added	Soil Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ITT										
Rental Subsidy	10.1***	0.24**	-2.1	-0.02	3.0	0.08	32.4***	0.28**	15.6*	-0.02
	[3.4]	[0.11]	[2.5]	[0.11]	[2.1]	[0.16]	[10.4]	[0.12]	[8.1]	[0.06]
Cash Drop	3.5	0.14	3.2	0.07	1.8	0.06	12.7	0.10	-0.9	0.02
	[2.9]	[0.11]	[2.6]	[0.12]	[2.1]	[0.15]	[9.4]	[0.13]	[7.1]	[0.06]
p-value Rent = Cash	*0.05*	*0.34*	*0.05*	*0.45*	*0.60*	*0.89*	*0.06*	*0.17*	*0.05*	*0.46*
TOT										
Rental Subsidy Paid	13.9***	0.34**	-2.9	-0.03	4.1	0.11	44.3***	0.39***	21.4**	-0.02
	[4.5]	[0.13]	[3.3]	[0.14]	[2.7]	[0.19]	[13.7]	[0.15]	[10.7]	[0.07]
Cash Drop Paid	3.6	0.14	3.2	0.07	1.8	0.06	12.7	0.10	-0.9	0.02
	[2.8]	[0.10]	[2.6]	[0.11]	[2.0]	[0.13]	[9.1]	[0.11]	[6.9]	[0.05]
p-value Rent = Cash	*0.01*	*0.08*	*0.05*	*0.43*	*0.38*	*0.77*	*0.01*	*0.04*	*0.03*	*0.46*
Mean Y in Control Group	33.0	IHS	46.07	IHS	22.7	IHS	96.3	IHS	-6.4	-0.02
Observations	1,957	509	1,957	509	1,957	509	1,957	509	1,957	967

Notes: The table reports treatment effects on agricultural outcomes on the Target Plot. The dependent variables in cols. (1)-(9) come from follow-up surveys we run at the end of seasons 1 to 4 with the manager of the Target Plot. They are measured in USD are equal to zero if the Target Plot is not cultivated. Inputs in columns (1-2) include seeds, fertilizer, and chemicals. We obtain their total value by multiplying the quantity of each input used on the Target Plot by its county-round median price and then summing up across inputs. The harvest value (cols. 7-8) is obtained in a similar way, summing across crops. We obtain the value of household labor (cols. 3-4) by multiplying the quantity of household labor used for each agricultural task by the county-round median wage for hired labor in that task, then adjusting by a factor of 0.6. The soil index in col. (10) comes from two rounds of soil testing conducted at the end of seasons 1 and 4. The index combines four nutrients (nitrogen, potassium, phosphorus and organic carbon) and pH. In the odd columns, we pool observations from the four rounds of follow-up surveys. In columns (1), (3), (5), (7) we winsorize the top 1%. In columns (9) and (10), we winsorize the top and bottom 1%. In columns (2), (4), (6), (8), the dependent variable is the inverse hyperbolic sine transformation (IHS) of the sum by Target Plot across the four rounds of the values of the variable. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome, plot size, survey-round dummies, and strata dummies (see Equation (1) in the paper). For inputs and labor, baseline values are from the 2019 Long Rains; for harvest, baseline values are from the 2018 Short Rains; for value added, we control for inputs and labor from the 2019 Long Rains and harvest value from the 2018 Short Rains. For the soil index (col. 10), we control for the baseline self-reported soil quality index, as we do not have baseline soil tests, and also for laboratory fixed effect. In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

Table 5: Owner outcomes: non-Target Plots

	Rented out	Cultivated	Maize	Commercial crops	Inputs	HH labor	Hired labor	Harvest	Value added
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ITT									
Rental Subsidy	0.01	0.01	0.02	-0.01	1.02	-0.17	1.69	5.78	1.75
	[0.01]	[0.02]	[0.02]	[0.01]	[1.77]	[1.92]	[1.19]	[8.05]	[7.11]
Cash Drop	0.00	-0.00	-0.00	0.00	3.33*	0.38	1.15	-1.69	-8.05
	[0.01]	[0.02]	[0.02]	[0.01]	[1.93]	[2.00]	[1.10]	[8.21]	[7.28]
p-value Rent = Cash	*0.63*	*0.60*	*0.34*	*0.23*	*0.24*	*0.78*	*0.66*	*0.40*	*0.21*
TOT									
Rental Subsidy Paid	0.01	0.01	0.02	-0.01	1.36	-0.22	2.25	7.72	2.33
	[0.01]	[0.02]	[0.03]	[0.01]	[2.32]	[2.53]	[1.55]	[10.56]	[9.37]
Cash Drop Paid	0.00	-0.00	-0.00	0.00	3.33*	0.38	1.14	-1.72	-8.08
	[0.01]	[0.02]	[0.02]	[0.01]	[1.90]	[1.98]	[1.09]	[8.10]	[7.19]
p-value Rent = Cash	*0.51*	*0.59*	*0.29*	*0.20*	*0.39*	*0.79*	*0.45*	*0.36*	*0.26*
Mean Y in Control Group	0.05	0.75	0.47	0.09	25.06	36.21	12.04	94.36	19.35
Observations	5,229	4,955	4,955	4,955	4,955	4,955	4,955	4,955	4,955

Notes: The table reports treatment effects on agricultural outcomes on the non-Target Plots, using a reshaped plot-level panel. Observations differ between Column (1) and Columns (2-9) as the rented out analysis is unconditional, while columns (2-9) only include plots that were not rented out. Details on the data sources and construction of the variables are included in the notes of Table 3 and Table 4. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome, plot size, survey-round dummies, and strata dummies (see Equation (B.6) in the Appendix). In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (B.7) in the Appendix). We cluster standard errors by Target Plot owner. *p<0.1, **p<0.05, ***p<0.01.

Table 6: Owner outcomes

	Labor Supply			Assets		Food Security			Finance	
	Other Farms	Non - Agricultural	Worked Outside Village	Owns Livestock	Wealth Index	Maize (S1)	Maize (S2 - S4)	Experienced Hunger	Emergency Liquidity	Borrowed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ITT										
Rental Subsidy	-0.54	-7.00*	-0.02	-0.04	0.04	0.10**	-0.06***	0.02	-0.02	-0.02
	[1.58]	[3.83]	[0.02]	[0.03]	[0.08]	[0.05]	[0.02]	[0.02]	[0.03]	[0.03]
Cash Drop	0.84	-4.66	-0.03	-0.02	0.07	0.05	-0.03*	0.02	-0.01	-0.05*
	[1.44]	[3.73]	[0.02]	[0.03]	[0.08]	[0.05]	[0.02]	[0.02]	[0.03]	[0.03]
p-value Rent = Cash	0.37	0.52	0.45	0.40	0.74	0.34	0.23	0.87	0.82	0.37
TOT										
Rental Subsidy Paid	-0.74	-9.52*	-0.02	-0.06	0.05	0.13**	-0.08***	0.03	-0.03	-0.03
	[2.08]	[5.07]	[0.03]	[0.04]	[0.11]	[0.06]	[0.03]	[0.03]	[0.04]	[0.04]
Cash Drop Paid	0.84	-4.66	-0.03	-0.02	0.07	0.05	-0.03*	0.02	-0.01	-0.05*
	[1.40]	[3.63]	[0.02]	[0.03]	[0.08]	[0.04]	[0.02]	[0.02]	[0.03]	[0.03]
p-value Rent = Cash	0.40	0.27	0.67	0.26	0.88	0.11	0.07	0.90	0.70	0.60
Mean Y in Control Group	9.16	38.71	0.18	0.64	-0.00	0.71	0.91	0.16	0.31	0.61
Observations	1,985	1,965	1,967	1,985	1,979	503	1,482	1,984	1,985	1,985

Notes: The table reports treatment effects on the original owners of the Target Plot, for outcomes not relating to the household farm. The dependent variables come from follow-up surveys we ran at the end of seasons 1 to 4 with the original owner of the Target Plot, pooling observations across seasons unless otherwise specified. Columns (1) and (2) are the number of person-days worked in the past season, summed across household members, on non-household agricultural work and non-agricultural work respectively. Column (3) is an indicator variable for whether any member of the household worked outside the village. Columns (4) and (5) are measures of wealth, with (4) being an indicator variable equal to one if the household owns any cows, bulls, or oxen, and (5) the standardized principal component of a vector of household assets and amenities (excluding land and livestock). Columns (6) through (8) pertain to food security: (6) and (7) are whether the household had any maize stocks from their own production in the last 6 months, in the first season and in the subsequent seasons, respectively (point estimates for by-season treatment effects are similar in seasons 2-4, and opposite in sign to season 1); (8) is a dummy variable for whether the household experienced a hunger period in any of the last six months. Columns (9) and (10) are household finance variables. (9) is a dummy variable for whether the household would have enough savings to cover an emergency expenditure of 5,000 Ksh ($50), while (10) is a dummy variable for whether they have borrowed in the last 6 months. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome, survey round dummies, and strata dummies (see Equation (1 in the paper), with the index i now refering to Target Plot owners, not to the Target Plot) . In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by Target Plot owner. *p<0.1, **p<0.05, ***p<0.01.

Appendix - For Online Publication

A Listing and baseline analysis

This appendix presents additional statistics using listing and baseline survey data. First, we use listing data to compare characteristics of surveyed vs non-surveyed plot owners, among those expressing interest in the rental subsidy in the listing (see Section 3.1). Second, we compare owner and Target Plot characteristics in the stratum where, in the listing, owners said they were planning to cultivate the Target Plot in the first experimental season vs those who said they would not. Third, we present balance by treatment group, focusing on characteristics of owners, Target Plots, and other plots.

A.1 Surveyed vs non-surveyed plot owners, among those expressing interest in the rental subsidy in the listing

Table A.1: Comparison of surveyed vs non-surveyed owners

	Surveyed [S]	Not Surveyed [NS]	[S-NS]	N
Male	0.68	0.69	-0.01	878
	(0.47)	(0.46)	(.03)	
Age	50.05	51.77	-1.71	876
	(14.87)	(15.10)	(1.0)*	
Has a Phone	0.90	0.91	-0.01	878
	(0.29)	(0.28)	(.01)	
No. Plots Owned	3.53	3.46	0.07	878
	(1.30)	(1.38)	(.09)	
Acres Owned (wins. 1%)	4.12	4.04	0.08	878
	(3.63)	(3.56)	(.24)	
Renting out at least one plot	0.06	0.13	-0.07	878
	(0.24)	(0.33)	(.02)***	
No. Plots Rented Out	0.07	0.14	-0.07	878
	(0.31)	(0.41)	(.02)***	
Share of plots fallowed	0.08	0.08	0.01	878
	(0.16)	(0.17)	(.01)	
Share of plots cultivated with cash crops	0.07	0.06	0.01	878
	(0.10)	(0.09)	(.00)*	

Notes: The sample in the table includes plot owners who expressed interest for the rental subsidy in the listing (N=878). Within this sample, we compare those owners who were surveyed at baseline and eventually included in the study (N=521) to those who were not surveyed (N=357). The data comes from the listing survey. *Male* is a binary indicator equal to one if the respondent was male. *Age* is missing two observations relative to all other included variables. We winsorize only *Acres Owned* at the top 1%. *Share of plots cultivated with cash crops* is the share of plots each owner is cultivating with groundnuts, tobacco or sugarcane. The *[S-NS]* columns are generated by a regression of each outcome on a surveyed dummy with robust standard errors. *p<0.1, **p<0.05, ***p<0.01.

A.2 Stratum C vs Stratum NC

Table A.2: Comparison of Stratum C versus Stratum NC

	Plan to Cultivate [C]	Plan to Fallow [NC]	[C-NC]	N
Owner characteristics				
Age	50.08	51.34	-1.25	521
	(14.35)	(15.98)	(1.42)	
Male	0.70	0.70	0.00	521
	(0.46)	(0.46)	(0.04)	
Family Size	5.86	5.35	0.51	521
	(2.72)	(2.70)	(0.25)**	
High School Educated	0.24	0.22	0.01	521
	(0.43)	(0.42)	(0.04)	
Agricultural Training	0.29	0.32	-0.04	521
	(0.45)	(0.47)	(0.04)	
Total plots: total acres owned in 2019 Long Rains	2.52	2.64	-0.11	520
	(1.92)	(2.03)	(0.18)	
Have maize stocks from own production, last 12 months	0.70	0.68	0.01	521
	(0.46)	(0.47)	(0.04)	
Number person-days spent working on other farms, last 7 months	25.41	20.07	5.33	521
	(73.77)	(69.17)	(6.53)	
Number person-days spent on non-ag work, last 12 months	21.25	24.23	-2.98	521
	(31.73)	(34.48)	(3.09)	
Taken a loan in last 12 months	0.63	0.61	0.01	521
	(0.48)	(0.49)	(0.04)	
5k Ksh in emergency savings	0.34	0.45	-0.11	521
	(0.48)	(0.50)	(0.05)**	
Wealth index, assets- and amenities-based PCA	-0.05	0.09	-0.14	520
	(1.72)	(2.07)	(0.18)	
Target Plot characteristics				
Plot Size	0.71	0.73	-0.01	521
	(0.46)	(0.47)	(0.04)	
Sandy clay soil	0.29	0.22	0.07	521
	(0.46)	(0.41)	(0.04)*	
Erosion dummy	0.26	0.19	0.07	521
	(0.44)	(0.39)	(0.04)*	
Cultivated in 2019 Long Rains	0.73	0.36	0.36	521
	(0.45)	(0.48)	(0.04)***	
Rented out in 2019 Long Rains	0.13	0.08	0.05	521
	(0.34)	(0.28)	(0.03)*	
Cultivated with maize in 2019 Long Rains	0.60	0.29	0.31	521
	(0.49)	(0.46)	(0.04)***	
Cultivated with commercial crops in 2019 Long Rains	0.05	0.02	0.03	521
	(0.22)	(0.15)	(0.02)*	
Value of agricultural inputs in 2019 Long Rains	40.3	23.4	16.90	517
	(74.1)	(65.5)	(6.30)***	
Value of household labor in 2019 Long Rains	36.04	16.34	19.69	521
	(44.50)	(35.54)	(3.58)***	
Value of hired labor in 2019 Long Rains	13.4	12.1	1.30	521
	(26.5)	(26.9)	(2.50)	
Cultivated in 2018 Short Rains	0.63	0.37	0.25	521
	(0.48)	(0.49)	(0.04)***	
Rented out in 2018 Short Rains	0.10	0.08	0.02	521
	(0.30)	(0.28)	(0.03)	
Harvest value in 2018 Short Rains	87.3	38.3	49.00	521
	(208.1)	(124.9)	(14.60)***	

Notes: The table presents a comparison of owner and Target Plot characteristics for owners that, in the listing, reported they were planning to cultivate the Target Plot for the first experimental agricultural season, i.e., the Short Rains 2019, (*Stratum C*, N=342) against those who were either planning to leave it fallow or still undecided (*Stratum NC*, N=179). The data comes from the owner baseline survey. *Male* is a binary indicator equal to one if the household head is male. *High School Educ household head* is a binary indicator equal to one if the highest level of education completed by the household head is high school or higher. *Agri Training household head* is a binary indicator equal to one if the household head received specific agricultural training in the past 3 years. *Total plots: total acres owned in 2019 long rains* is the sum of plot sizes across all plots owned at baseline, winsorized at the top 1%. *5k Ksh in emergency savings* is a binary indicator equal to one if the household had enough savings to cover an emergency expenditure of 5,000 Ksh ($50). *Wealth index, assets- and amenities-based PCA* is the standardized principal component of a vector of assets and amenities (excluding land and livestock). *Cultivated with commercial crops in 2019 long rains* is a binary indicator equal to one if the Target Plot was cultivated with groundnuts, tobacco or sugarcane during the long rains 2019. Value of agricultural inputs, household labor, hired labor and harvest are expressed in USD (1 USD = 100 KSh) and winsorized at the top 1%. *Value of agricultural inputs in 2019 long rains* is the value of any seeds, compost, chemical fertilizer, and pesticides used on the Target Plot. *Value of hired labor in 2019 long rains* is the number of hired-work days valued at the median reported wage. *Value of household labor in 2019 long rains* is the number of household-member-work days valued at 60% of the median reported wage. Since we conducted the baseline survey while the 2019 Long Rains harvesting was ongoing, we do not have information on harvest amount for that season for most of the sample. The difference *[C-NC]* is the coefficient from a regression of each outcome on a binary indicator equal to one if the household was planning to cultivate the Target Plot in the short rains 2019. P-values are reported in parentheses: *p<0.1, **p<0.05, ***p<0.01.

A.3 Balance

Table A.3: Balance

	Rental Subsidy [RS]	Cash Drop [CD]	Control [C]	[RS-CD]	[RS-C]	[CD-C]	N
A. Owners							
Age	49.38	51.81	50.34	-2.22	-0.95	1.40	521
	(15.19)	(15.19)	(14.38)	(1.60)	(1.64)	(1.61)	
Male	0.69	0.74	0.69	-0.06	-0.01	0.07	521
	(0.47)	(0.44)	(0.47)	(0.05)	(0.05)	(0.05)	
Family Size	5.37	5.83	5.85	-0.46	-0.42	0.06	521
	(2.83)	(2.71)	(2.61)	(0.30)	(0.30)	(0.28)	
High School Educated	0.26	0.21	0.23	0.05	0.01	-0.01	521
	(0.44)	(0.41)	(0.42)	(0.04)	(0.05)	(0.05)	
Agricultural Training	0.32	0.25	0.33	0.07	0.01	-0.06	521
	(0.47)	(0.44)	(0.47)	(0.05)	(0.05)	(0.05)	
Compare agricultural experience to avg. farmer (1-5)	2.84	2.78	2.89	0.04	-0.03	-0.10	521
	(0.89)	(0.82)	(0.92)	(0.09)	(0.09)	(0.09)	
No. plots owned in 2019 Long Rains	3.49	3.53	3.65	-0.05	-0.21	-0.15	521
	(1.28)	(1.34)	(1.29)	(0.14)	(0.14)	(0.14)	
Total plots: total acres owned in 2019 Long Rains	2.48	2.64	2.57	-0.17	-0.09	0.08	520
	(1.87)	(2.07)	(1.95)	(0.18)	(0.17)	(0.20)	
Have maize stocks from own production, last 12 months	0.69	0.70	0.68	0.00	0.01	0.01	521
	(0.46)	(0.46)	(0.47)	(0.04)	(0.04)	(0.05)	
Experienced a hunger period, last 12 months	0.34	0.36	0.37	-0.02	-0.04	-0.01	521
	(0.48)	(0.48)	(0.48)	(0.05)	(0.05)	(0.05)	
Own oxen or cow	0.69	0.67	0.61	0.02	0.07	0.05	521
	(0.46)	(0.47)	(0.49)	(0.05)	(0.05)	(0.05)	
Number person-days spent working on other farms, last 7 months	20.04	20.14	30.46	-1.62	-10.26	-8.90	521
	(70.39)	(56.06)	(86.67)	(6.68)	(8.78)	(6.98)	
Number person-days spent on non-ag work, last 12 months	20.90	20.21	25.68	1.06	-6.58	-6.76	521
	(31.16)	(31.62)	(35.05)	(3.22)	(3.53)*	(3.63)*	
Taken a loan in last 12 months	0.66	0.57	0.63	0.10	0.03	-0.06	521
	(0.48)	(0.50)	(0.48)	(0.05)*	(0.05)	(0.05)	
Total borrowed, last 12 months	53.0	88.8	69.5	-32.8	-23.1	14.9	521
	(123.6)	(233.4)	(145.9)	(19.1)*	(14.7)	(21.1)	
Participate in ROSCA	0.48	0.45	0.52	0.01	-0.04	-0.06	521
	(0.50)	(0.50)	(0.50)	(0.05)	(0.05)	(0.06)	
Have bank account	0.25	0.26	0.28	0.00	-0.03	-0.02	521
	(0.43)	(0.44)	(0.45)	(0.05)	(0.05)	(0.05)	
Total amount saved	64.3	74.1	78.7	-5.1	-16.8	-4.4	521
	(155.5)	(170.2)	(175.0)	(17.9)	(17.4)	(18.8)	
5k Ksh in emergency savings	0.38	0.34	0.41	0.03	-0.03	-0.06	521
	(0.49)	(0.48)	(0.49)	(0.05)	(0.05)	(0.05)	
Wealth index, assets- and amenities-based PCA	0.17	0.01	-0.18	0.15	0.33	0.21	520
	(2.07)	(1.79)	(1.65)	(0.22)	(0.19)*	(0.18)	
B. Target Plots							
Plot Size	0.71	0.76	0.69	-0.04	0.02	0.07	521
	(0.44)	(0.52)	(0.43)	(0.03)	(0.03)	(0.03)**	
Inherited	0.91	0.91	0.93	0.01	-0.02	-0.02	521
	(0.28)	(0.29)	(0.26)	(0.03)	(0.03)	(0.03)	
Certificate of title/customary ownership	0.76	0.67	0.67	0.10	0.10	0.00	521
	(0.43)	(0.47)	(0.47)	(0.05)**	(0.05)**	(0.05)	
Respondent's homestead in different village than plot	0.02	0.02	0.01	0.00	0.01	0.01	521
	(0.13)	(0.13)	(0.08)	(0.01)	(0.01)	(0.01)	
Sandy loam soil	0.53	0.53	0.55	-0.01	0.00	0.00	521
	(0.50)	(0.50)	(0.50)	(0.05)	(0.05)	(0.05)	
Sandy clay soil	0.27	0.26	0.26	0.02	0.01	-0.02	521
	(0.45)	(0.44)	(0.44)	(0.05)	(0.05)	(0.05)	
Soil quality index (1-3)	2.56	2.56	2.64	-0.01	-0.08	-0.07	521
	(0.54)	(0.53)	(0.53)	(0.06)	(0.06)	(0.05)	
Swampy/dry index (1-3)	2.42	2.39	2.41	0.03	-0.02	0.01	521
	(0.60)	(0.61)	(0.60)	(0.07)	(0.07)	(0.07)	
Erosion dummy	0.21	0.21	0.29	0.00	-0.07	-0.09	521
	(0.41)	(0.41)	(0.46)	(0.04)	(0.04)*	(0.04)**	
Irrigation dummy	0.05	0.05	0.07	0.00	-0.02	-0.01	521
	(0.21)	(0.22)	(0.26)	(0.02)	(0.02)	(0.03)	
Cultivated in 2019 Long Rains	0.63	0.60	0.57	0.04	0.06	0.04	521
	(0.48)	(0.49)	(0.50)	(0.05)	(0.05)	(0.05)	

	Rental Subsidy [RS]	Cash Drop [CD]	Control [C]	[RS-CD]	[RS-C]	[CD-C]	N
Rented out in 2019 Long Rains	0.13	0.10	0.12	0.03	0.01	-0.02	521
	(0.33)	(0.31)	(0.33)	(0.03)	(0.04)	(0.03)	
Cultivated with maize in 2019 Long Rains	0.53	0.49	0.46	0.05	0.07	0.03	521
	(0.50)	(0.50)	(0.50)	(0.05)	(0.05)	(0.05)	
Cultivated with commercial crops in 2019 Long Rains	0.04	0.05	0.04	-0.01	0.00	0.01	521
	(0.20)	(0.21)	(0.20)	(0.02)	(0.02)	(0.02)	
Value of agricultural inputs in 2019 Long Rains	41.1	39.2	23.1	2.6	19.1	19.6	517
	(84.5)	(75.9)	(48.8)	(8.1)	(7.5)**	(6.7)***	
Value of household labor in 2019 Long Rains	32.10	26.28	29.47	6.82	4.70	-1.28	521
	(45.58)	(35.33)	(46.20)	(4.36)	(4.88)	(4.31)	
Value of hired labor in 2019 Long Rains	16.2	11.7	11.1	4.3	5.8	1.8	521
	(30.3)	(24.7)	(24.4)	(3.0)	(2.8)**	(2.7)	
Cultivated in 2018 Short Rains	0.53	0.56	0.53	-0.02	0.00	0.04	521
	(0.50)	(0.50)	(0.50)	(0.05)	(0.05)	(0.05)	
Rented out in 2018 Short Rains	0.09	0.09	0.10	0.01	-0.01	-0.01	521
	(0.29)	(0.29)	(0.30)	(0.03)	(0.03)	(0.03)	
Harvest value in 2018 Short Rains	72.5	86.4	52.8	-10.0	16.1	27.0	521
	(169.5)	(232.3)	(141.4)	(20.9)	(16.8)	(21.0)	
C. Non-target Plots							
Owned in 2019 Long Rains	2.49	2.53	2.65	-0.05	-0.21	-0.15	521
	(1.28)	(1.34)	(1.29)	(0.14)	(0.14)	(0.14)	
Total acres owned in 2019 Long Rains	1.77	1.88	1.89	-0.12	-0.11	0.00	520
	(1.69)	(1.83)	(1.75)	(0.18)	(0.17)	(0.19)	
Rented out in 2019 Long Rains	0.10	0.15	0.22	-0.05	-0.12	-0.06	521
	(0.34)	(0.44)	(0.53)	(0.04)	(0.05)**	(0.05)	
Cultivated in 2019 Long Rains	2.10	1.94	2.18	0.17	-0.10	-0.27	521
	(1.33)	(1.21)	(1.25)	(0.13)	(0.14)	(0.13)**	
Cultivated with maize in 2019 Long Rains	1.15	1.16	1.26	-0.03	-0.13	-0.12	521
	(0.97)	(0.88)	(0.97)	(0.10)	(0.10)	(0.09)	
Cultivated with commercial crops in 2019 Long Rains	0.27	0.20	0.23	0.07	0.04	-0.01	521
	(0.52)	(0.44)	(0.55)	(0.05)	(0.06)	(0.06)	
Value of agricultural inputs	140.0	102.7	96.7	39.0	45.9	11.5	518
	(294.6)	(249.5)	(188.6)	(26.6)	(26.2)*	(23.3)	
Value of household labor	28.90	24.53	28.48	3.59	2.57	-3.85	521
	(44.86)	(32.44)	(41.50)	(4.34)	(4.80)	(4.11)	
Value of hired labor	8.8	9.6	8.8	-1.7	-0.2	1.5	520
	(17.2)	(19.8)	(18.5)	(2.2)	(1.9)	(2.1)	
Cultivated in 2018 Short Rains	1.85	1.71	1.87	0.16	-0.05	-0.20	521
	(1.32)	(1.23)	(1.31)	(0.13)	(0.14)	(0.14)	
Harvest value in 2018 Short Rains	231.9	295.7	281.3	-50.2	-32.3	3.2	521
	(603.1)	(842.8)	(825.8)	(83.4)	(70.6)	(89.8)	

Notes: The table presents the baseline balance for owners' socio-demographic characteristics and non-agricultural outcomes (Panel A), Target Plots (Panel B) and Non-target plots (Panel C). The data comes from the owner baseline survey. **Panel A:** *Male* is a binary indicator equal to one if the household head is male. *High School Educated* is a binary indicator equal to one if the highest level of education completed by the household head is high school or higher. *Agricultural Training* is a binary indicator equal to one if the household head received specific agricultural training in the past 3 years. *Compare agricultural experience to avg. farmer* comes from a question asking owners to assess their experience relative to the average farmer in their village on a 5-point scale, from "much less experience" to "much more experience". *Own oxen or cow* is a binary indicator equal to one if the household owns any cows or oxen. *5k Ksh in emergency savings* is a binary indicator equal to one if the household had enough savings to cover an emergency expenditure of 5,000 Ksh ($50). *Wealth index, assets- and amenities-based PCA* is the standardized principal component of a vector of assets and amenities (excluding land and livestock). **Panel B:** *Plot size* is the average between plot size reported by the owner and plot size measured at baseline by enumerators using hand-held GPS devices. The unit is acres. *Certificate of title/customary ownership* is a binary indicator equal to one if the owner has either a certificate of title or of customary ownership for the Target Plot. *Soil quality index* is a soil quality index self-reported by the respondent and it could take values 1 = poor, 2 = fair, 3 = good. *Swampy/dry index* could take values of 1 = swampy, 2 = mix, 3 = dry. *Cultivated with commercial crops in 2019 long rains* is a binary indicator equal to one if the Target Plot was cultivated with groundnuts, tobacco or sugarcane during the long rains 2019. Value of agricultural inputs, household labor, hired labor and harvest are expressed in USD (1 USD = 100 KSh) and winsorized at the top 1%. *Value of agricultural inputs in 2019 long rains* is the value of any seeds, compost, chemical fertilizer, and pesticides used on the Target Plot. *Value of hired labor in 2019 long rains* is the number of hired-work days valued at the median reported wage. *Value of household labor in 2019 long rains* is the number of household-member-work days valued at 60% of the median reported wage. Since we conducted the baseline survey while the 2019 Long Rains harvesting was ongoing, we do not have information on harvest amount for that season for most of the sample. **Panel C:** *Owned in 2019 long rains* and *Rented out in 2019 long rains* is the number of Non-target plots owned and rented out at baseline, respectively. *Total acres owned in 2019 long rains* is the sum of self-reported plot sizes across all Non-target plots and is winsorized at the top 1%. *Cultivated in 2019 long rains* and *Cultivated in 2018 short rains* are the total number of Non-target plots cultivated at baseline (2019 long rains) and in the previous agricultural season (2018 short rains), respectively. *Cultivated with commercial crops in 2019 long rains* is the total number of Non-target plots cultivated with groundnuts, tobacco or sugarcane during the long rains 2019. Value of agricultural inputs, household labor, hired labor, and harvest is the sum of the respective values across all Non-target plots. They are expressed in USD (1 USD = 100 KSh) and winsorized at the top 1%. P-values are based on specifications which include stratum fixed effects. *p<0.1, **p<0.05, ***p<0.01.

B Empirical strategy

The experimental analysis focuses on treatment effects on Target Plots and their owners, across four groups of outcomes. First, we document the effect of treatments on the likelihood that the Target Plot is rented out. Second, we then look at the distributional effects of land rental markets, comparing the characteristics of owners to renters for the rentals induced by the subsidy. Third, we examine how rental subsidies (and unconditional cash transfers) affect agricultural production, including crop choice, investment, output, and soil quality – first for the Target Plot, and then for owners' other plots. Finally, we study treatment effects on the owners' non-agricultural outcomes, such as food security and labor supply.

B.1 Target Plot: rentals

We examine the impact of the treatments on the likelihood that the Target Plot is rented out:

$$TargetPlotRentedOut_{is}^{t} = \beta_0 + \beta_1 RentalSubsidy_i + \beta_2 CashTransfer_i + \delta x_i^0 + \eta_s + \eta^t + \epsilon_i^t, \tag{B.1}$$

where the outcome is a dummy for whether the Target Plot i is rented out in crop season $t = 1, 2, 3, 4, 5$, η^t is a vector of crop-season fixed effects, η_s is a vector of strata fixed effects, x_i^0 is a vector of baseline controls that includes the size of the Target Plot and the value of the outcome variable in the two pre-experimental seasons for which we have data (2018 Long Rains and 2019 Short Rains). Data comes mostly from the follow-up surveys.[1] In a handful of cases, we collected information on the rental status even if we could not conduct a full follow-up survey for the plot.

We present these results both by season and pooling across seasons. Importantly, we have information on the rental status of the Target Plot in crop seasons 4 and 5, which enables us to test whether rental relationships induced by the treatment persisted after the rental subsidy intervention ended (in season 3). We also examine whether renting out the Target Plot may substitute for renting out other plots.

B.2 Target Plot: manager characteristics

The treatment may affect who manages the Target Plot, and thus the manager's observable characteristics. We are interested in whether rentals change manager characteristics such as demographics (e.g., age, gender, education), wealth (agricultural land owned, non-land wealth), baseline use of agricultural inputs, and agricultural productivity.

We study whether rentals induce changes in *baseline* characteristics of the Target Plot managers. For this purpose, we use two sources of data. If (in the first experimental season) the Target Plot manager is the owner, we use information from baseline owner survey, which we collected toward the end of the 2019 Long Rains (i.e., the last season before the intervention began); if the Target Plot manager is a renter, we use information from the baseline renter survey, conducted at the very beginning of the 2019 Short Rains, right after the rental began. Our analysis thus explores whether, by affecting rental probabilities, the rental subsidy may change baseline characteristics of managers of the Target Plot through a treatment effect on the identity of the manager.[2]

We examine the impact of the treatments on the baseline characteristics of the manager of the Target Plot in the first season. We present ITT and LATE results. The ITT regression model is:

$$x_{is}^{Manager} = \beta_0 + \beta_1 RentalSubsidy_i + \beta_2 CashTransfer_i + \delta x_i^0 + \eta_s + \epsilon_i, \tag{B.2}$$

where $x_{is}^{Manager}$ is the characteristic of the renter if the Target Plot is rented out and of the owner otherwise, x_i^0 is the value of the owner characteristic from the baseline owner survey (equal to the dependent variable $x_i^{Manager}$ if the Target Plot is not rented out), and the rest of the notation follows Equation B.1. We are

[1] We collect data on rentals for the upcoming season 5 in the follow-up survey we conduct at the end of season 4.

[2] While we conducted the owner baseline survey at the end of season 0, we could only run the renter baseline survey at the inception of season 1, as soon as the rentals were agreed. Most of the analysis of manager characteristics focuses on time-invariant characteristics or on production choices for season 0, which are unlikely to be affected by this difference in timing. Finally, since managing the Target Plot may have treatment effects on some of the characteristics of interest, we cannot conduct the same analysis for later experimental seasons.

primarily interested in β_1, the ITT effect of the rental subsidy on manager characteristics, but we also report ITT effects of the cash transfer for completeness, β_2.

We are also interested in the LATE of renting the plot on manager characteristics. For this purpose, we estimate:

$$x_{is}^{Manager} = \gamma_0 + \gamma_1 Target\widehat{PlotRented}Out_i + \gamma_2 Cash\widehat{Trans}fer_i + \delta x_i^0 + \eta_s + \epsilon_i, \tag{B.3}$$

where we instrument $TargetPlotRentedOut_i$ with the treatment assignment $RentalSubsidy_i$. The exclusion restriction is that the treatment changes the identity of the Target Plot manager (and thus her baseline characteristics) only by affecting the probability of a rental, which seems uncontroversial.

B.3 Target Plot: agricultural outcomes

We use information from the four rounds of follow-up surveys with the Target Plot managers to study the treatment effects on Target Plot outcomes: cultivation rates (vs leaving the plot uncultivated, crop choice, input value, harvest value, and value added. We also examine the impact on soil quality, using results from the soil sample laboratory analysis. The ITT regressions is thus:

$$y_{is}^t = \beta_0 + \beta_1 RentalSubsidy_i + \beta_2 CashTransfer_i + \delta x_i^0 + \eta_s + \eta^t + \epsilon_i^t, \tag{B.4}$$

where the notation follows Equation B.1, except that we have Target Plot outcomes for four seasons, not five. We cluster standard errors by Target Plot. For continuous outcomes, we focus on winsorized (1%) outcomes in levels and on the inverse hyperbolic sine (IHS) transformation of the total outcome across rounds.[3]

Since there is imperfect compliance in the rental subsidy treatment (see Section 4.2), we also estimate the Treatment-on-Treated (TOT). As paying a rental subsidy in season t may affect rental status and other plot outcomes in season $t+1$, we consider as endogenous variables dummies capturing whether the respondent received any rental subsidy or unconditional cash transfer payment during the study (as opposed to season-specific payment status), and we use the treatment assignment as an instrument. Section 4 provides more details on take up by crop season and thus on the interpretation of the TOT.

The estimating equation for the TOT is thus:

$$y_{is}^t = \gamma_0 + \gamma_1 Rental\widehat{Subsid}yPaid_i + \gamma_2 CashT\widehat{ransf}erPaid_i + \delta x_i^0 + \eta_s + \eta^t + \epsilon_i^t. \tag{B.5}$$

As we discussed in detail in Section 3.7, the TOT coefficient γ_1 measures the effects of offsetting the rental frictions through the payment of the conditional subsidy to the owners. In addition, under plausible assumptions, the comparison of γ_1 to γ_2 is a lower bound of the effect of the rental subsidy on compliers in this group controlling for the income effect

Another question of interest would be what is the effect of the rentals induced by the subsidy, absent any income effects the subsidy induces? As is common in conditional subsidy designs, we cannot estimate the LATE of the actual rental status of the Target Plot, because the exclusion restriction fails: the rental subsidy may affect the Target Plot outcomes not only by inducing rentals, but also because of an income effect, on both marginal and inframarginal rentals. However, we can bound the LATE of renting out the target plot, absent the income effect of the subsidy, as follows. First, comparing the rental subsidy group to the control group gives the effect of rentals on compliers, plus income effects on compliers and always takers. Second, comparing the rental subsidy group to the cash drop group gives the effect of rentals on compliers, minus the income effect on never takers (plus any effect of the income effect potentially being passed through to compliers in the rental subsidy group —a negative income effect on the owner and a positive one on the renter). Assuming that income effects have the same average sign in these three groups (always takers, compliers, and never takers), we therefore can partially identify the treatment effect of renting out as lying in the interval between the two LATEs, both of which instrument renting out by the rental subsidy: 1) in a comparison between rental subsidy and control groups, and 2) in a comparison between rental subsidy and cash drop groups. In practice, IV estimates when using a dummy for whether the Target Plot is rented as endogenous variable are about 40% larger than when using the dummy for whether the rental subsidy was paid (i.e., the TOT results we present in the paper).

[3]Season-specific outcomes contain sizable shares of zeros (e.g., mostly because some plots are not cultivated in certain seasons) and, thus, we cannot use IHS in that case (Bellemare and Wichman, 2020)

B.4 Owner outcomes

We use information from the four rounds of follow-up surveys to study the effect of the treatment on Target Plot owners. Regardless of whether they managed the Target Plot in a given season, we asked the owners questions on agricultural outcomes on their non-Target Plots, food security, non-agricultural activities, assets and amenities, and household finances.

Agricultural outcomes on Non-Target Plots. For the analysis of outcomes on non-Target Plots, we reshape our data at the plot level and run the following ITT regression:

$$y_{pis}^{t} = \beta_0 + \beta_1 RentalSubsidy_i + \beta_2 CashTransfer_i + \delta x_p^0 + \eta_s + \eta^t + \epsilon_p^t, \tag{B.6}$$

where we consider outcomes for non-Target Plot p of owner i in crop season t. The rest of the notation follows the previous equations. Standard errors are clustered at the owner level. We only measure outcomes of non-Target Plots if the owner manages them, not if she rents them out (because we do not interview the renters of non-Target Plots). Therefore, we first report treatment effects on the likelihood that the non-Target Plot is rented out and then we report treatment effects on other non-Target Plot outcomes (cultivation, crop choice, inputs, output, and value added) only if the plot is not rented out.

We also present TOT estimates, instrumenting whether the owner received any rental subsidy or cash transfer payment during the experiment with the treatment dummies:

$$y_{pis}^{t} = \beta_0 + \beta_1 Rental\widehat{Subsid}yPaid_i + \beta_2 CashT\widehat{rans}ferPaid_i + \delta x_p^0 + \eta_s + \eta^t + \epsilon_p^t, \tag{B.7}$$

Non-agricultural owner outcomes. For the analysis of non-agricultural owner outcomes, we present ITT and TOT estimates following Equation (1) and Equation (2) respectively, where the index i now refers to Target Plot's owners instead than to the Target Plot.

C Subsidy take up and Target Plot rentals

This appendix presents additional results on take up of the subsidy and rentals of the target plot (see Section 4 for the main results on these outcomes). First, we compare characteristics among owners in the rental subsidy treatment group who took up the rental subsidy (N=121) vs those who did not (N=51). Second, we present treatment effects on the likelihood that the Target Plot is rented out by season (1-5) and by stratum (C vs NC). Finally, we compare plot characteristics and rental terms among rentals in the rental subsidy group and those in the control and cash drop groups.

Table C.1: Comparison of Rental Subsidy compliers and non-compliers

	Complier [C]	Non-Complier [NC]	Difference [C-NC]	N
Age	49.24	49.65	-0.41	172
	(14.59)	(16.38)	(2.52)	
Male	0.67	0.72	-0.05	172
	(0.47)	(0.45)	(.07)	
Family Size	5.55	5.03	0.52	172
	(2.81)	(2.87)	(.46)	
High School Educated	0.30	0.17	0.14**	172
	(0.46)	(0.38)	(.07)	
Agricultural Training	0.41	0.15	0.26***	172
	(0.49)	(0.36)	(.07)	
Compare agricultural experience to avg. farmer (1-5)	2.91	2.70	0.21	172
	(0.93)	(0.81)	(.14)	
No. plots owned in 2019 Long Rains	3.48	3.52	-0.03	172
	(1.24)	(1.35)	(.21)	
Total plots: total acres owned in 2019 Long Rains	2.62	2.21	0.41	172
	(1.95)	(1.68)	(.28)	
Have maize stocks from own production, last 12 months	0.75	0.58	0.17**	172
	(0.43)	(0.50)	(.08)	
Experienced a hunger period, last 12 months	0.30	0.42	-0.11	172
	(0.46)	(0.50)	(.08)	
Own oxen or cow	0.72	0.63	0.09	172
	(0.45)	(0.49)	(.08)	
Number person-days spent working on other farms, last 7 months	25.33	10.17	15.16*	172
	(83.83)	(31.40)	(8.9)	
Number person-days spent on non-ag work, last 12 months	24.31	14.52	9.80**	172
	(32.87)	(26.78)	(4.64)	
Taken a loan in last 12 months	0.69	0.60	0.09	172
	(0.47)	(0.49)	(.08)	
Total borrowed, last 12 months	70.81	19.85	50.96***	172
	(148.85)	(29.66)	(14.59)	
Participate in ROSCA	0.52	0.40	0.12	172
	(0.50)	(0.49)	(.08)	
Have bank account	0.29	0.18	0.10	172
	(0.45)	(0.39)	(.07)	
Total amount saved	63.62	65.69	-2.07	172
	(129.33)	(196.38)	(28.09)	
5k Ksh in emergency savings	0.43	0.30	0.13*	172
	(0.50)	(0.46)	(.08)	
Wealth index, assets- and amenities-based PCA	0.34	-0.16	0.51*	171
	(2.24)	(1.67)	(.3)	
Plot size	0.78	0.57	0.22***	172
	(0.48)	(0.30)	(.06)	
Inherited	0.90	0.93	-0.03	172
	(0.30)	(0.25)	(.04)	
Certificate of title/customary ownership	0.77	0.75	0.02	172
	(0.42)	(0.44)	(.07)	
Respondent's homestead in different village than plot	0.03	0.00	0.03*	172
	(0.16)	(0.00)	(.02)	
Sandy loam soil	0.57	0.47	0.10	172
	(0.50)	(0.50)	(.08)	
Sandy clay soil	0.27	0.28	-0.02	172
	(0.44)	(0.45)	(.07)	
Soil quality index (1=poor, 2=fair, 3=good)	2.57	2.53	0.04	172
	(0.55)	(0.54)	(.09)	
Swampy/dry index (1=swampy, 2=mix, 3=dry)	2.43	2.42	0.01	170
	(0.61)	(0.59)	(.1)	
Erosion dummy	0.23	0.17	0.07	172

	(0.42)	(0.38)	(.06)	
Irrigation dummy	0.05	0.03	0.02	172
	(0.23)	(0.18)	(.03)	
Cultivated in 2019 Long Rains	0.63	0.65	-0.03	172
	(0.49)	(0.48)	(.08)	
Rented out in 2019 Long Rains	0.14	0.10	0.04	172
	(0.35)	(0.30)	(.05)	
Cultivated with maize in 2019 Long Rains	0.54	0.52	0.02	172
	(0.50)	(0.50)	(.08)	
Cultivated with commercial crops in 2019 Long Rains	0.04	0.03	0.01	172
	(0.21)	(0.18)	(.03)	
Value of agricultural inputs in 2019 Long Rains	47.13	29.92	17.21	172
	(93.63)	(63.22)	(12.03)	
Value of household labor in 2019 Long Rains	37.76	21.55	16.21**	172
	(49.74)	(34.52)	(6.47)	
Value of hired labor in 2019 Long Rains	16.74	15.09	1.65	172
	(30.16)	(30.88)	(4.89)	
Cultivated in 2018 Short Rains	0.54	0.53	0.00	172
	(0.50)	(0.50)	(.08)	
Rented out in 2018 Short Rains	0.11	0.07	0.04	172
	(0.31)	(0.25)	(.04)	
Plan cultivate in 2019 Long Rains (Listing)	0.65	0.67	-0.01	172
	(0.48)	(0.48)	(.08)	
Harvest value in 2018 Short Rains	77.00	64.12	12.88	172
	(169.05)	(171.50)	(27.27)	

Notes: The table presents a comparison of socio-demographic characteristics and non-agricultural outcomes and Target Plot characteristics of owners who took up the rental subsidy vs those who did not take up, among those owners randomly assigned to the rental subsidy treatment group. The data comes from the owner baseline survey and the listing survey. Details on the construction of the variables are included in the notes of Table A.3. The values in the column *Difference* are generated by a regression of each outcome on a dummy for whether the farmer took up the rental subsidy for any season of the sample. Robust standard errors are included in parantheses. *p<0.1, **p<0.05, ***p<0.01.

Table C.2: Target Plot Rented Out

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rental Subsidy	0.46***	0.44***	0.46***	0.37***	0.34***	0.41***	0.42***	0.43***
	[0.05]	[0.05]	[0.05]	[0.05]	[0.05]	[0.04]	[0.05]	[0.06]
Cash Drop	0.06	0.03	0.06	0.02	0.03	0.04	0.04	0.06
	[0.05]	[0.05]	[0.05]	[0.05]	[0.05]	[0.04]	[0.05]	[0.06]
p-value Rent = Cash	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Crop Season	1	2	3	4	5	All	All	All
Strata	All	All	All	All	All	All	C	NC
Mean Y in Control Group	0.23	0.24	0.22	0.20	0.22	0.22	0.21	0.24
Observations	521	512	507	499	489	2528	1660	868

Notes: The table reports the treatment effects on the likelihood the Target Plot is rented out. The data comes from follow-up surveys we run at the end of seasons 1 to 4 with the manager of the Target Plot. Data for Season 5 (col. 5) comes from the survey we ran at the end of Season 4. The stratum was created based on a Target Plot's likelihood to be Cultivated (C) or Not Cultivated (NC) during the first season of the study (see Section 3.3 in the Paper for more details). We run an ANCOVA regression of the rented out dummy on treatment dummies, controlling for baseline rental status and plot size, and including stratum dummies for all columns (See Equation 1 in the paper). Columns 6-8 also include survey-round dummies. We use robust standard errors for columns 1-5 and we cluster standard errors by the Target Plot for columns 6-8. *p<0.1, **p<0.05, ***p<0.01.

Table C.3: Comparison of rentals across treatment groups

	Rental Subsidy [RS]	Cash Drop & Control [CD&C]	[RS-(CD&C)]
Target Plot characteristics			
Plot size	0.77	0.78	-0.01
	(0.48)	(0.54)	(0.07)
Sandy loam soil	0.57	0.59	-0.01
	(0.50)	(0.50)	(0.07)
Sandy clay soil	0.25	0.22	0.03
	(0.43)	(0.41)	(0.06)
Soil quality index (1=poor, 2=fair, 3=good)	2.56	2.59	-0.03
	(0.56)	(0.54)	(0.08)
Swampy/dry index (1=swampy, 2=mix, 3=dry)	2.42	2.52	-0.10
	(0.62)	(0.58)	(0.08)
Erosion dummy	0.23	0.28	-0.06
	(0.42)	(0.45)	(0.06)
Irrigation dummy	0.05	0.07	-0.02
	(0.22)	(0.25)	(0.03)
Formal certificate available	0.82	0.77	0.05
	(0.38)	(0.42)	(0.06)
Rented out at any point in 2019	0.22	0.33	-0.11
	(0.41)	(0.47)	(0.06)*
Renters and rental contracts			
Rental contract duration (months)	20.63	21.29	-0.66
	(16.42)	(16.08)	(2.32)
Cash amount agreed for rental contract	93.3	95.7	-2.4
	(87.1)	(111.4)	(14.5)
Taken a loan to rent in	0.08	0.05	0.03
	(0.27)	(0.21)	(0.03)
Renter's homestead in different village than Target Plot	0.21	0.21	0.00
	(0.41)	(0.41)	(0.06)
Renter is a family member	0.35	0.27	0.08
	(0.48)	(0.45)	(0.07)
Rented in before from same owner	0.19	0.27	-0.08
	(0.39)	(0.45)	(0.06)
Rented the Target Plot before	0.16	0.29	-0.13
	(0.37)	(0.46)	(0.06)**
Renting in other plots at baseline (2019 long rains)	0.29	0.34	-0.04
	(0.46)	(0.48)	(0.07)
Observations	120	92	212

Notes: The table presents a comparison of Target Plot rentals that occurred in the *Rental Subsidy* (N=120) group against those that occurred in the *Cash Drop* and *Control* (N=92) group. Due to the small number of rentals in the Cash Drop and in the Control group and the similar rental rates in the two groups, we pool them together to gain power in the comparison. The sample is based on the subset of Target Plots which were rented out in the first experimental season, the short rains 2019. The data in the first panel comes from the owner baseline survey and reports average Target Plots characteristics for the rented plots. *Plot Size* is the average between the Target Plot size reported by the owner and the size measured at baseline by enumerators using hand-held GPS devices. The unit is acres. *Target Plot: formal certificate available* is a binary indicator equal to one if the owner has a formal certificate of ownership over the Target Plot. *Target Plot: rented out at any point in 2019* is a binary indicator equal to one if the Target Plot was rented out at baseline, at any point during 2019, before the first experimental season (the short rains 2019). The data in the second panel comes from the renter baseline survey and reports average renters and contract characteristics. Reported characteristics are for the rental contracts started or in place during the short rains 2019. The difference *[RS-(CD & C)]* is the coefficient from a regression of each outcome on a binary indicator equal to one if the owner belongs to the *Rental Subsidy* group. Robust standard errors are reported in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table C.4: Learning and persistence: comparing rentals that persist to those that do not

	Continued Rentals [CR]	Terminated Rentals [TR]	[TR-CR]	N
Baseline soil quality	1.47	1.39	-0.08	163
	(0.59)	(0.52)	(.08)	
Baseline Revenue	101.94	80.74	-21.20	163
	(283.62)	(191.88)	(37.80)	
Rental rate (per acre, per season)	45.95	42.40	-3.56	163
	(40.67)	(36.32)	(6.03)	
Revenue (Season 1)	196.19	108.04	-88.15	163
	(288.17)	(192.57)	(38.26)**	
Revenue (Seasons 1-3)	178.13	123.00	-55.13	486
	(252.32)	(181.92)	(24.09)**	
Value Added (Season 1)	42.60	-1.89	-44.49	163
	(245.09)	(154.76)	(31.98)	
Value Added (Seasons 1-3)	28.33	-2.03	-30.36	486
	(200.32)	(141.66)	(16.74)*	
Target Plot cultivated (Seasons 1-3)	0.96	0.94	-0.02	486
	(0.20)	(0.24)	(.02)	

Notes: The table compares outcomes for Target Plots that were rented out in Season 1 and where the initial renter-owner relationship continued for all four seasons vs Target Plots that were rented out in Season 1 and where the owner rented to a different renter or stopped renting before Season 4. The data comes from the owner baseline survey and the follow-up surveys we run at the end of seasons 1 to 4 with the manager of the Target Plot. All variables, apart from *Baseline soil quality* and *Target Plot cultivated*, are in per-acre terms and are winsorized at the top 1% level and all *Value Added* variables are also winsorized at the bottom 1%. *Baseline soil quality* is a self-reported index of soil quality. The values in the column *Difference* are generated by a regression of each outcome on a dummy for whether the renter-owner relationship did not continue for all four seasons. Standard errors, included in parantheses, are robust for variables with only observation for one season. Otherwise, we cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

D Distributional effects of rentals: Manager characteristics

This appendix presents additional results on the treatment effects on Target Plot baseline manager characteristics (see Section 5 for the main results on these outcomes). First, we show Lee bounds for attrition. Second, we present results on additional manager characteristics not included in the main text.

Table D.1: Manager Characteristics: Lee Bounds

	Households Size	Age	Gender	High School Educated	N. Plots Owned	Rent In Plot(s)	S. Plots Cash Crops	Target Plot in Diff. Village	Experienced Hunger	Non-Land Wealth	Borrowed	Emergency Savings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ITT												
Rental Subsidy	0.10 [0.25] 0.08 [0.25] 0.12 [0.25]	-3.83*** [1.24] -3.90*** [1.24] -3.72*** [1.24]	0.12*** [0.04] 0.12*** [0.04] 0.12*** [0.04]	0.07* [0.04] 0.07* [0.04] 0.07* [0.04]	-0.91*** [0.15] -0.93*** [0.15] -0.90*** [0.15]	0.15*** [0.04] 0.15*** [0.04] 0.15*** [0.04]	0.02 [0.03] 0.01 [0.02] 0.02 [0.03]	0.09*** [0.03] 0.09*** [0.03] 0.09*** [0.03]	-0.06* [0.03] -0.06* [0.03] -0.06* [0.03]	-0.12 [0.09] -0.13 [0.09] -0.12 [0.09]	0.12*** [0.04] 0.12*** [0.04] 0.12*** [0.04]	0.04 [0.04] 0.04 [0.04] 0.05 [0.04]
Cash Drop	-0.27 [0.19] -0.35* [0.19] -0.25 [0.19]	-1.32 [1.02] -1.69 [1.03] -0.96 [1.03]	0.09*** [0.03] 0.08*** [0.03] 0.09*** [0.03]	0.04 [0.03] 0.03 [0.03] 0.04 [0.03]	-0.13 [0.14] -0.21 [0.14] -0.06 [0.14]	0.04 [0.03] 0.02 [0.03] 0.04 [0.03]	-0.01 [0.02] -0.03 [0.02] -0.01 [0.02]	0.03 [0.03] 0.01 [0.02] 0.03 [0.03]	-0.02 [0.03] -0.04 [0.03] -0.02 [0.03]	0.07 [0.09] 0.01 [0.08] 0.08 [0.09]	0.02 [0.04] 0.01 [0.04] 0.03 [0.04]	0.05 [0.04] 0.04 [0.04] 0.06* [0.04]
p-value Rent = Cash	*0.11* *0.07* *0.12*	*0.05* *0.09* *0.03*	*0.32* *0.31* *0.46*	*0.50* *0.41* *0.54*	*0.00* *0.00* *0.00*	*0.01* *0.00* *0.01*	*0.15* *0.08* *0.19*	*0.05* *0.01* *0.05*	*0.29* *0.47* *0.25*	*0.05* *0.11* *0.04*	*0.02* *0.01* *0.04*	*0.94* *0.99* *0.80*
LATE												
Plot Rented	0.21 [0.44] 0.15 [0.43] 0.23 [0.43]	-7.83*** [2.15] -7.92*** [2.14] -7.65*** [2.15]	0.26*** [0.07] 0.26*** [0.07] 0.27*** [0.07]	0.14** [0.07] 0.13* [0.07] 0.13** [0.07]	-1.87*** [0.21] -1.90*** [0.21] -1.85*** [0.22]	0.30*** [0.06] 0.29*** [0.06] 0.31*** [0.06]	0.05 [0.05] 0.04 [0.05] 0.05 [0.05]	0.19*** [0.05] 0.18*** [0.05] 0.19*** [0.06]	-0.13** [0.06] -0.13** [0.06] -0.13** [0.06]	-0.25 [0.17] -0.27* [0.16] -0.25 [0.17]	0.24*** [0.07] 0.24*** [0.07] 0.25*** [0.07]	0.09 [0.07] 0.08 [0.07] 0.09 [0.07]
Mean Y in Control Group	5.75 5.75 5.75	48.98 48.98 48.98	0.69 0.69 0.69	0.24 0.24 0.24	3.21 3.21 3.21	0.07 0.07 0.07	0.11 0.11 0.11	0.05 0.05 0.05	0.33 0.33 0.33	-0.01 -0.01 -0.01	0.62 0.62 0.62	0.40 0.40 0.40
Observations	508 503 503	508 503 503	508 503 503	508 503 503	508 503 503	508 503 503	467 463 462	506 501 501	508 503 503	504 499 499	508 503 503	508 503 503

Notes: The table reports the bounded treatment effects following Lee (2009), with bounds created for each variable by trimming the top and bottom of the Rental Subsidy and Cash Drop group, as these groups had the lowest attrition. For each cell in the table, results are ordered as following: unbounded, lower bound and upper bound. Details on the data sources and construction of the variables are included in the notes of Table 2. In the *ITT* sub-panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome (noting that these will be equal to the outcome itself when the Target Plot is not rented out), plot size and stratum dummies (see Equation (B.2) in the Appendix). In the *LATE* sub-panel, we run an ANCOVA regression with the same controls, but we instrument the dummies for whether the Target Plot was rented out with the Rental Subsidy treatment, while controlling for the Cash Drop treatment (see Equation (B.3) in the Appendix). *p<0.1, **p<0.05, ***p<0.01.

Table D.2: Manager characteristics: Additional results

	Agricultural Training	Agricultural Experience	Acres Owned	Input per Acre	Output per Acre	Maize Stock	Owns Livestock	Total Savings	Lent Money	Emergency Savings	Walking distance
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
ITT											
Rental Subsidy	-0.07	0.02	-0.70***	31.27	9.27	-0.01	-0.08*	17.66	0.00	0.03	4.52***
	[0.04]	[0.08]	[0.15]	[175.74]	[51.09]	[0.04]	[0.05]	[21.72]	[0.04]	[0.03]	[1.09]
Cash Drop	0.05	0.10	0.09	-103.51	-45.58	0.01	-0.02	13.22	-0.01	-0.00	1.30
	[0.04]	[0.07]	[0.15]	[123.13]	[49.13]	[0.03]	[0.04]	[23.63]	[0.03]	[0.03]	[0.91]
p-value Rent = Cash	*0.01*	*0.37*	*0.00*	*0.38*	*0.08*	*0.66*	*0.18*	*0.87*	*0.73*	*0.34*	*0.00*
LATE											
Plot Rented	-0.14*	0.05	-1.45***	87.98	21.59	-0.01	-0.15*	36.00	0.00	0.05	9.29***
	[0.08]	[0.15]	[0.24]	[409.30]	[99.96]	[0.07]	[0.08]	[38.29]	[0.08]	[0.05]	[1.81]
Mean Y in Control Group	0.27	2.83	2.13	167.72	150.35	0.70	0.63	96.12	0.40	0.85	7.66
Observations	508	508	496	386	422	508	508	508	508	508	506

Notes: The table reports treatment effects on additional characteristics of the Target Plot managers, which are not included in Table 2 in the main text. The dependent variables correspond to the *baseline* characteristics of whomever is managing the Target Plot in the first endline season (2019 Short Rains). Column (1) is an indicator variable for whether the household head has received any agricultural training. Column (2) is a categorical variable for the level of agricultural experience they have compared to the average farmer in their village, with 1 being *much less experience* and 5 being *much more experience*. Column (3) is the total acres of land owned by the household. Columns (4) and (5) measure the total input and output per acre of land, with (4) the total input per acre measured for the long rains season in 2019, and with (5) the total output per acres measured for the short rains season in 2018. Note that the measures correspond to different seasons due to the surveys being conducted at different stages. Column (6) pertains to food security constructed as an indicator variable for whether the household had any maize stocks from their own production in the last 6 months. Column (7) is a measure of wealth constructed as an indicator variable equal to one if the household owns any cows, bulls, or oxen. Column (8) measures the total savings of the household, while (9) is an indicator variable for whether the household has lent money. Column (10) is a dummy variable for whether the household would have 5,000 Ksh (50$) in savings available if an emergency expense arose, and (11) is a measure of walking time (mins) to the Target Plot. In the *ITT* sub-panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome (noting that these will be equal to the outcome itself when the Target Plot is not rented out), plot size and stratum dummies (see Equation (B.2) in the Appendix). In the *LATE* sub-panel, we run an ANCOVA regression with the same controls, but we instrument the dummies for whether the Target Plot was rented out with the Rental Subsidy treatment, while controlling for the Cash Drop treatment (see Equation (B.3) in the Appendix). *p<0.1, **p<0.05, ***p<0.01.

E Target Plot outcomes

This appendix presents additional results on treatment effects on Target Plot outcomes (see Section 6 for the main results on these outcomes). Appendix E.1 presents robustness to alternative specifications and to alternative valuations of household labor. In Appendix E.2, we show statistics about attition by survey and by treatment group and present Lee bounds. Appendix E.3 presents results on additional outcomes, including: the breakdown of treatment effects by input type, by stratum, by crop season, and by soil test nutrients; treatment effects on calbrated TFP; quantile regressions; and learning and persistence.

E.1 Target plot outcomes: Robustness

Table E.1: Robustness: Target Plot Cultivation and Crop Choice

	Cultivated				Maize				Commercial			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ITT												
Rental Subsidy	0.06***	0.07***	0.07***	0.05**	-0.01	0.00	0.00	-0.01	0.07***	0.07***	0.07***	0.07***
	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.03]	[0.03]	[0.03]	[0.02]	[0.02]	[0.02]	[0.02]
Cash Drop	0.06***	0.07***	0.07***	0.06***	0.05*	0.05*	0.05*	0.05*	0.02	0.02	0.03	0.02
	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.03]	[0.03]	[0.03]	[0.02]	[0.02]	[0.02]	[0.02]
p-value Rent = Cash	*0.89*	*0.96*	*0.98*	*0.55*	*0.05*	*0.10*	*0.10*	*0.04*	*0.02*	*0.05*	*0.06*	*0.04*
TOT												
Rental Subsidy Paid	0.08***	0.09***	0.09***	0.06**	-0.01	0.00	0.00	-0.02	0.10***	0.09***	0.10***	0.09***
	[0.03]	[0.03]	[0.03]	[0.03]	[0.04]	[0.04]	[0.04]	[0.04]	[0.03]	[0.03]	[0.03]	[0.03]
Cash Drop Paid	0.06***	0.07***	0.07***	0.06***	0.05*	0.05*	0.05*	0.04	0.02	0.02	0.03	0.02
	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.03]	[0.03]	[0.03]	[0.02]	[0.02]	[0.02]	[0.02]
p-value Rent = Cash	*0.47*	*0.37*	*0.36*	*0.83*	*0.07*	*0.16*	*0.16*	*0.08*	*0.00*	*0.01*	*0.02*	*0.01*
Mean Y in Control Group	0.82	0.82	0.82	0.82	0.69	0.69	0.69	0.69	0.09	0.09	0.09	0.09
Controls	Main	Size	None	PDS	Main	Size	None	PDS	Main	Size	None	PDS
Observations	1,957	1,957	1,957	1,957	1,956	1,956	1,956	1,956	1,956	1,956	1,956	1,956

Notes: The table reports treatment effects on the likelihood the Target Plot is cultivated (col. 1-4), cultivated with maize (col. 5-8), cultivated with commercial crops, i.e., groundnuts, sugarcane, tobacco (col. 9-12). Along with results under the core specification (col. 1, 5 and 9), the table includes results when, in addition to controlling for survey-round dummies and stratum dummies, only plot size is controlled for (col. 2, 6 and 10), when no other variables are controlled for (col. 3, 7 and 11), and when, following Belloni et al. (2014), we control for Target Plot variables selected via post-double-selection (PDS) (col. 4, 8 and 12). Details on the data sources are included in the notes of Table 4. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies (see Equation (1) in the paper). In the *TOT* Panel, we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

Table E.2: Robustness: Value of Inputs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ITT								
Rental Subsidy	10.1***	0.24**	11.1***	0.29**	12.4***	0.32***	10.7***	0.21**
	[3.4]	[0.11]	[3.4]	[0.12]	[3.6]	[0.11]	[3.3]	[0.10]
Cash Drop	3.5	0.14	4.7	0.17	7.8**	0.23*	4.6	0.11
	[2.9]	[0.11]	[3.0]	[0.12]	[3.3]	[0.12]	[2.8]	[0.10]
p-value Rent = Cash	*0.05*	*0.34*	*0.05*	*0.26*	*0.20*	*0.42*	*0.05*	*0.28*
TOT								
Rental Subsidy Paid	13.9***	0.34**	15.2***	0.41***	16.9***	0.45***	14.7***	0.30**
	[4.5]	[0.13]	[4.4]	[0.14]	[4.6]	[0.14]	[4.3]	[0.14]
Cash Drop Paid	3.6	0.14	4.7	0.17*	7.8**	0.23**	4.5	0.11
	[2.8]	[0.10]	[2.9]	[0.10]	[3.2]	[0.10]	[2.8]	[0.10]
p-value Rent = Cash	*0.01*	*0.08*	*0.01*	*0.04*	*0.03*	*0.07*	*0.01*	*0.11*
Mean Y in Control Group	33.0	IHS	33.0	IHS	33.0	IHS	33.0	IHS
Controls	Main	Main	Size	Size	None	None	PDS	PDS
Observations	1,957	509	1,957	509	1,957	509	1,957	509

Notes: The table reports treatment effects on the value of inputs used on the Target Plot, as well as several robustness tests. Along with results under the core specification (col. 1 and 2), the table includes results when, in addition to controlling for survey-round dummies and stratum dummies, only plot size is controlled for (col. 3 and 4), when no other variables are controlled for (col. 5 and 6), and when, following Belloni et al. (2014), we control for Target Plot variables selected via post-double-selection (PDS) (col. 7 and 8). Details on the data sources are included in the notes of Table 4. This table also includes results using the inverse hyperbolic sine transformation (IHS) of the sum of the input values used on each Target Plot for each robustness test (even columns). In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies (see Equation (1) in the paper). In the *TOT* Panel, we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

Table E.3: Robustness: Value of Household Labor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ITT								
Rental Subsidy	-2.1	-0.02	-1.4	0.00	-1.3	-0.00	-1.9	-0.02
	[2.5]	[0.11]	[2.5]	[0.11]	[2.5]	[0.11]	[2.4]	[0.10]
Cash Drop	3.2	0.07	3.1	0.08	3.4	0.08	3.6	0.05
	[2.6]	[0.12]	[2.7]	[0.12]	[2.7]	[0.12]	[2.6]	[0.10]
p-value Rent = Cash	*0.05*	*0.44*	*0.09*	*0.52*	*0.08*	*0.52*	*0.03*	*0.49*
TOT								
Rental Subsidy Paid	-2.9	-0.03	-2.0	0.00	-1.8	-0.00	-3.0	-0.03
	[3.3]	[0.14]	[3.4]	[0.14]	[3.4]	[0.14]	[3.3]	[0.13]
Cash Drop Paid	3.2	0.07	3.1	0.08	3.5	0.08	3.0	0.05
	[2.6]	[0.11]	[2.6]	[0.11]	[2.6]	[0.11]	[2.5]	[0.10]
p-value Rent = Cash	*0.05*	*0.43*	*0.10*	*0.54*	*0.09*	*0.54*	*0.05*	*0.54*
Mean Y in Control Group	46.1	IHS	46.1	IHS	46.1	IHS	46.1	IHS
Controls	Main	Main	Size	Size	None	None	PDS	PDS
Observations	1,957	509	1,957	509	1,957	509	1,957	509

Notes: The table reports treatment effects on the value of household labor used on the Target Plot, as well as several robustness tests. Along with results under the core specification (col. 1 and 2), the table includes results when, in addition to controlling for survey-round dummies and stratum dummies, only plot size is controlled for (col. 3 and 4), when no other variables are controlled for (col. 5 and 6), and when, following Belloni et al. (2014), we control for Target Plot variables selected via post-double-selection (PDS) (col. 7 and 8). Details on the data sources are included in the notes of Table 4. This table also includes results using the inverse hyperbolic sine transformation (IHS) of the sum of the value of household labor used on each Target Plot for each robustness test (even columns). In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies (see Equation (1) in the paper). In the *TOT* Panel, we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

Table E.4: Robustness: Value of Hired Labor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ITT								
Rental Subsidy	3.0	0.08	3.3	0.17	3.7*	0.19	3.7	0.13
	[2.1]	[0.16]	[2.1]	[0.16]	[2.1]	[0.16]	[2.1]	[0.14]
Cash Drop	1.8	0.06	1.9	0.08	3.0	0.13	3.0	0.07
	[2.1]	[0.15]	[2.1]	[0.15]	[2.1]	[0.15]	[2.0]	[0.13]
p-value Rent = Cash	*0.60*	*0.88*	*0.53*	*0.57*	*0.75*	*0.69*	*0.75*	*0.64*
TOT								
Rental Subsidy Paid	4.1	0.11	4.5	0.23	5.1*	0.27	4.4	0.16
	[2.7]	[0.19]	[2.8]	[0.20]	[2.8]	[0.20]	[2.8]	[0.19]
Cash Drop Paid	1.8	0.06	1.9	0.08	3.0	0.13	2.0	0.03
	[2.0]	[0.13]	[2.0]	[0.13]	[2.0]	[0.13]	[2.0]	[0.13]
p-value Rent = Cash	*0.38*	*0.75*	*0.32*	*0.37*	*0.43*	*0.44*	*0.34*	*0.44*
Mean Y in Control Group	22.7	IHS	22.7	IHS	22.7	IHS	22.7	IHS
Controls	Main	Main	Size	Size	None	None	PDS	PDS
Observations	1,957	509	1,957	509	1,957	509	1,957	509

Notes: The table reports treatment effects on the value of hired labor used on the Target Plot, as well as several robustness tests. Along with results under the core specification (col. 1 and 2), the table includes results when, in addition to controlling for survey-round dummies and stratum dummies, only plot size is controlled for (col. 3 and 4), when no other variables are controlled for (col. 5 and 6), and when, following Belloni et al. (2014), we control for Target Plot variables selected via post-double-selection (PDS) (col. 7 and 8). Details on the data sources are included in the notes of Table 4. This table also includes results using the inverse hyperbolic sine transformation (IHS) of the sum of the value of hired labor used on each Target Plot for each robustness test (even columns). In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies (see Equation (1) in the paper). In the *TOT* Panel, we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

Table E.5: Robustness: Harvest Value

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ITT										
Rental Subsidy	32.4***	0.28**	34.1***	0.29**	37.6***	0.32**	33.9***	0.24*	32.7***	0.25**
	[10.4]	[0.12]	[10.3]	[0.13]	[10.8]	[0.12]	[10.0]	[0.11]	[10.6]	[0.12]
Cash Drop	12.7	0.10	14.9	0.12	23.5**	0.18	15.13*	0.06	17.0*	0.07
	[9.4]	[0.13]	[9.4]	[0.13]	[10.2]	[0.13]	[9.0]	[0.11]	[9.9]	[0.13]
p-value Rent = Cash	*0.06*	*0.18*	*0.07*	*0.18*	*0.22*	*0.27*	*0.07*	*0.11*	*0.16*	*0.16*
TOT										
Rental Subsidy Paid	44.3***	0.39***	46.6***	0.41***	51.3***	0.44***	44.5***	0.33**	44.7***	0.35**
	[13.7]	[0.15]	[13.6]	[0.15]	[14.0]	[0.15]	[13.7]	[0.15]	[13.9]	[0.15]
Cash Drop Paid	12.7	0.10	14.9	0.12	23.3**	0.17	13.2	0.06	17.0*	0.07
	[9.1]	[0.11]	[9.1]	[0.12]	[9.8]	[0.12]	[9.0]	[0.11]	[9.6]	[0.11]
p-value Rent = Cash	*0.01*	*0.04*	*0.01*	*0.04*	*0.04*	*0.05*	*0.01*	*0.05*	*0.04*	*0.04*
Mean Y in Control Group	96.3	IHS	96.3	IHS	96.3	IHS	96.3	IHS	96.3	IHS
Controls	Main	Main	Size	Size	None	None	PDS	PDS	Planned	Planned
Observations	1,957	509	1,957	509	1,957	509	1,957	509	1,957	509

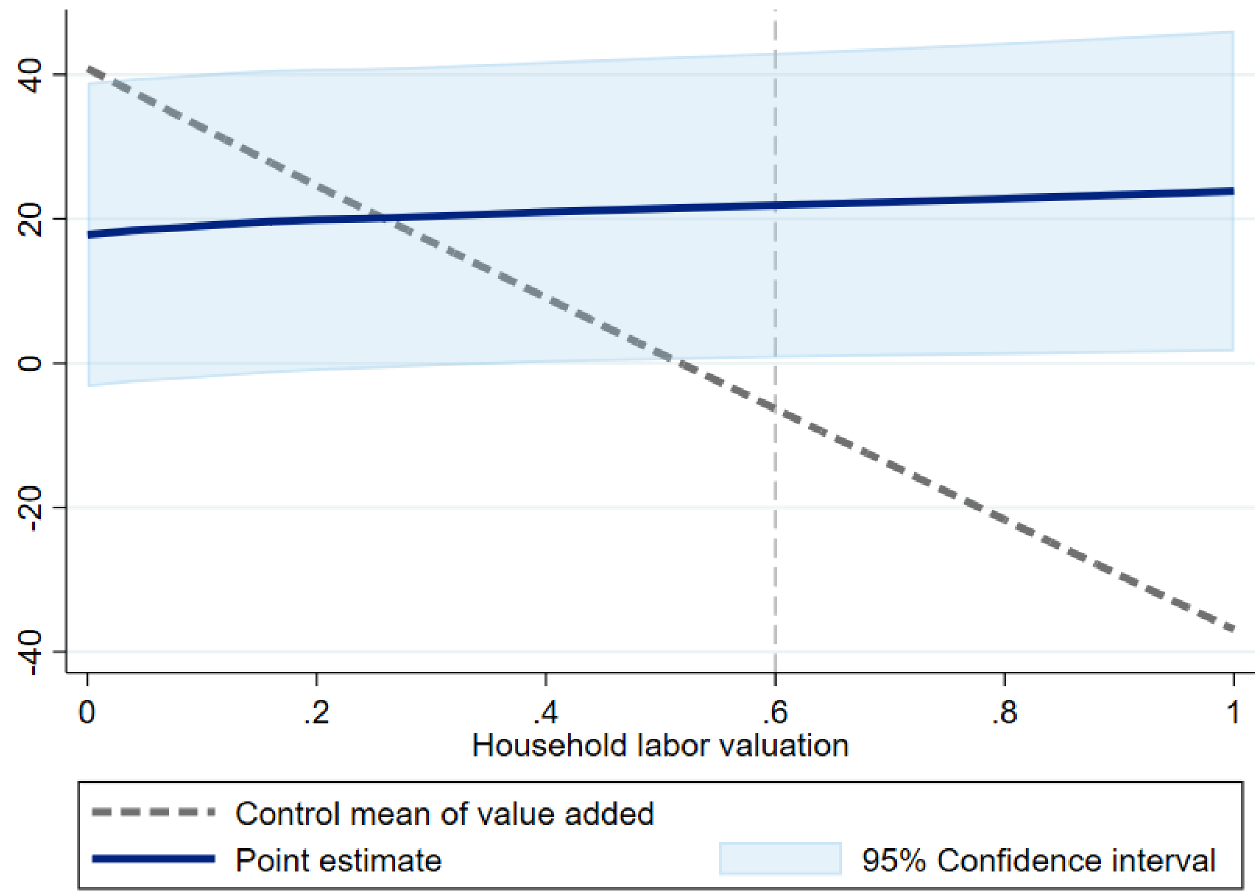
Notes: The table reports treatment effects on the value of harvest on the Target Plot, as well as several robustness tests. Along with results under the core specification (col. 1 and 2), the table includes results when, in addition to controlling for survey-round dummies and stratum dummies, only plot size is controlled for (col. 3 and 4), when no other variables are controlled for (col. 5 and 6), and when, following Belloni et al. (2014), we control for Target Plot variables selected via post-double-selection (PDS) (col. 7 and 8). In cols. (9) and (10), we control for a dummy capturing non-verified planned harvests (see discussion in Section 3.4). Details on the data sources are included in the notes of Table 4. Results using the inverse hyperbolic sine transformation (IHS) of the sum of the harvest values from each Target Plot for each robustness test are also included (even columns). In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies (see Equation (1) in the paper). In the *TOT* Panel, we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

Table E.6: Robustness: Value Added

	(1)	(2)	(3)	(4)	(5)
ITT					
Rental Subsidy	15.6**	16.2**	17.4**	17.4**	15.1*
	[8.1]	[7.8]	[7.9]	[7.7]	[7.8]
Cash Drop	-0.9	1.4	4.5	0.3	1.6
	[6.9]	[7.0]	[7.1]	[7.0]	[7.2]
p-value Rent = Cash	*0.03*	*0.04*	*0.06*	*0.04*	*0.11*
TOT					
Rental Subsidy Paid	21.4**	22.1**	23.8**	21.2**	20.7**
	[10.7]	[10.3]	[10.5]	[10.4]	[10.4]
Cash Drop Paid	-0.9	1.3	4.5	0.3	1.5
	[6.9]	[7.0]	[7.1]	[7.0]	[7.0]
p-value Rent = Cash	*0.03*	*0.04*	*0.06*	*0.04*	*0.06*
Mean Y in Control Group	-6.4	-6.4	-6.4	-6.4	-6.4
Controls	Main	Size	None	PDS	Planned
Observations	1,957	1,957	1,957	1,957	1,957

Notes: The table reports treatment effects on value added for the Target Plot, as well as several robustness tests. Along with results under the core specification (col. 1), the table includes results when, in addition to controlling for survey-round dummies and stratum dummies, only plot size is controlled for (col. 2), when no other variables are controlled for (col.3), and when, following Belloni et al. (2014), we control for Target Plot variables selected via post-double-selection (PDS) (col. 4). In col. (5), we control for a dummy capturing non-verified planned harvests (see discussion in Section 3.4). Details on the data sources are included in the notes of Table 4. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies (see Equation (1) in the paper). In the *TOT* Panel, we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

Figure E.1: Value Added TOT Coefficients by Household Labor Value



Notes: The figure includes the Rental Subsidy treatment effect on value added under different valuations of household labor. Valuation refers to how household labor is valued relative to hired labor. A valuation of 0 indicates that household labor is zero, while a valuation of 1 indicates household labor is valued the same as hired labor. The data used to construct the different variables comes from follow-up surveys we run at the end of seasons 1 to 4 with the manager of the Target Plot and are measured in USD. In the main results of the paper, we use a 60% value of household labor (based on Agness et al., 2022), the vertical line indicates results at this valuation. We winsorize the top and bottom 1% of the outcome variable. To generate the coefficients used in the graph, we run an ANCOVA regression controlling for baseline values of each variable, plot size, survey-round dummies and stratum dummies. We instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot.

Table E.7: Robustness: Soil Quality Index

	(1)	(2)	(3)	(4)
ITT				
Rental Subsidy	-0.02	-0.02	-0.02	-0.02
	[0.06]	[0.06]	[0.06]	[0.05]
Cash Drop	0.02	0.02	0.03	0.03
	[0.06]	[0.05]	[0.05]	[0.05]
p-value Rent = Cash	*0.46*	*0.46*	*0.43*	*0.40*
TOT				
Rental Subsidy Paid	-0.02	-0.02	-0.02	-0.03
	[0.07]	[0.07]	[0.07]	[0.07]
Cash Drop Paid	0.02	0.02	0.03	0.02
	[0.05]	[0.05]	[0.05]	[0.05]
p-value Rent = Cash	*0.46*	*0.46*	*0.44*	*0.46*
Mean Y in Control Group	-0.02	-0.02	-0.02	-0.02
Controls	Main	Size	None	PDS
Observations	967	967	967	967

Notes: The table reports treatment effects on the soil quality of the Target Plot, as well as several robustness tests. Along with results under the core specification (col. 1), the table includes results when, in addition to controlling for survey-round dummies and stratum dummies, only plot size is controlled for (col. 2), when no other variables are controlled for (col.3), and when, following Belloni et al. (2014), we control for Target Plot variables selected via post-double-selection (PDS) (col. 4). Details on the data sources are included in the notes of Table 4. Laboratory fixed effects are also included for the results in this table. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies (see Equation (1) in the paper). In the *TOT* Panel, we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

E.2 Target plot outcomes: Attrition

Table E.8: Attrition across surveys

	S0-2019 LR	S1-2019 SR	S2-2020 LR	S3-2020 SR	S4-2021 LR	S1-4
	(1)	(2)	(3)	(4)	(5)	(6)
A: Manager Characteristics						
Rental Subsidy	0.01					
	[0.02]					
Cash Drop	0.02					
	[0.01]					
p-value Rent=Cash	*0.26*					
Control Mean	*0.97*					
B: Target Plot Follow-up						
Rental Subsidy		-0.02	-0.01	-0.02	-0.02	-0.02
		[0.03]	[0.02]	[0.03]	[0.03]	[0.02]
Cash Drop		0.03	0.03	0.03	0.03	0.03*
		[0.02]	[0.02]	[0.02]	[0.03]	[0.02]
p-value Rent=Cash		*0.02*	*0.11*	*0.07*	*0.07*	*0.02*
Control Mean		*0.94*	*0.95*	*0.93*	*0.91*	*0.93*
C: Soil Samples						
Rental Subsidy		-0.05**			-0.05*	-0.05**
		[0.02]			[0.03]	[0.02]
Cash Drop		-0.00			0.02	0.01
		[0.02]			[0.02]	[0.02]
p-value Rent=Cash		*0.03*			*0.01*	*0.00*
Control Mean		*0.98*			*0.94*	*0.96*
D: Owner Follow-up						
Rental Subsidy		-0.03	-0.04*	-0.06**	-0.06**	-0.05**
		[0.02]	[0.02]	[0.03]	[0.03]	[0.02]
Cash Drop		-0.00	-0.00	0.02	0.02	0.01
		[0.02]	[0.02]	[0.02]	[0.02]	[0.01]
p-value Rent=Cash		*0.15*	*0.06*	*0.00*	*0.00*	*0.00*
Control Mean		*0.98*	*0.98*	*0.97*	*0.94*	*0.97*
Observations	521	521	521	521	521	2,084

Notes: The table reports completion rates across the different data collection activities included in the study. *Panel A* presents results from the baseline owner survey and the baseline renter survey. Data from the baseline owner survey is used where the Target Plot wasn't rented out in the first crop season. Where the Target Plot was rented out, data from the baseline renter survey is used. *Panel B* uses data from the follow-up surveys, asked at the end of each crop season, where we asked agricultural activity questions to each Target Plot manager: the owner if the plot was not rented out and the renter if it was rented out. Results in *Panel C* come from the two rounds of soil sampling completed during the first and the fourth crop seasons. *Panel D* presents the attrition results of each of the owner follow-up surveys where we asked owners, regardless of whether they rented out the Target Plot, questions concerning their other plots, non-agricultural activities, food security, assets and household finances. As soil samples were only collected in the first and fourth crop seasons, the pooled estimate in column 6 only includes 1,042 observations. We run an ANCOVA regression of a completion dummy on treatment dummies and include stratum dummies for all columns. Column (6) also includes survey-round dummies. We use robust standard errors for Columns (1)-(5) and we cluster standard errors by the Target Plot for column (6). *p<0.1, **p<0.05, ***p<0.01.

Table E.9: Target plot outcomes with Lee Bounds

	Cultivated	Maize	Commercial	Value of Inputs	Value of Household Labor	Value of Hired Labor	Harvest Value	Value Added	Soil Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ITT									
	0.06*** [0.02]	-0.01 [0.03]	0.07*** [0.02]	10.11*** [3.43]	-2.08 [2.48]	2.98 [2.06]	32.36*** [10.43]	15.56* [8.07]	-0.02 [0.06]
Rental Subsidy	0.05** [0.02]	-0.02 [0.03]	0.07*** [0.02]	9.70*** [3.45]	-2.84 [2.49]	2.64 [2.08]	31.07*** [10.52]	11.75 [7.73]	-0.03 [0.06]
	0.06*** [0.02]	-0.01 [0.03]	0.09*** [0.02]	10.92*** [2.81]	-0.55 [2.31]	4.39** [1.75]	34.44*** [8.18]	17.75*** [6.46]	0.01 [0.06]
	0.06*** [0.02]	0.05* [0.03]	0.02 [0.02]	3.53 [2.89]	3.19 [2.63]	1.84 [2.05]	12.69 [9.35]	-0.88 [7.07]	0.02 [0.06]
Cash Drop	0.09*** [0.02]	0.07** [0.03]	0.02 [0.02]	4.41 [2.97]	4.74* [2.66]	2.58 [2.10]	15.43 [9.72]	7.67 [6.78]	0.05 [0.06]
	0.06*** [0.02]	0.04 [0.03]	0.00 [0.02]	-0.01 [2.14]	-0.32 [2.30]	-0.86 [1.60]	-0.89 [5.94]	-11.79** [4.90]	-0.00 [0.05]
	0.90	*0.05*	*0.02*	*0.05*	*0.05*	*0.60*	*0.06*	*0.05*	*0.46*
p-value Rent = Cash	*0.06*	*0.00*	*0.04*	*0.12*	*0.00*	*0.98*	*0.15*	*0.61*	*0.17*
	0.88	*0.14*	*0.00*	*0.00*	*0.93*	*0.00*	*0.00*	*0.00*	*0.74*
TOT									
	0.08*** [0.03]	-0.01 [0.04]	0.10*** [0.03]	13.93*** [4.48]	-2.86 [3.31]	4.07 [2.71]	44.27*** [13.67]	21.40** [10.72]	-0.02 [0.07]
Rental Subsidy Paid	0.06** [0.03]	-0.03 [0.04]	0.10*** [0.03]	12.80*** [4.49]	-3.52 [3.31]	3.70 [2.72]	41.25*** [13.41]	16.17 [10.26]	-0.04 [0.07]
	0.08*** [0.03]	-0.01 [0.04]	0.12*** [0.03]	15.08*** [3.65]	-0.75 [3.07]	6.00*** [2.28]	47.21*** [10.69]	24.46*** [8.61]	0.02 [0.07]
	0.06*** [0.02]	0.05* [0.03]	0.02 [0.02]	3.57 [2.80]	3.21 [2.56]	1.84 [1.99]	12.65 [9.08]	-0.89 [6.89]	0.02 [0.05]
Cash Drop Paid	0.09*** [0.02]	0.08*** [0.03]	0.03 [0.02]	4.10 [2.86]	5.11** [2.58]	2.53 [2.02]	14.67 [9.18]	7.69 [6.59]	0.05 [0.05]
	0.06*** [0.02]	0.04 [0.03]	-0.00 [0.02]	0.02 [2.06]	-0.32 [2.23]	-0.90 [1.54]	-0.98 [5.74]	-11.83** [4.79]	-0.00 [0.05]
	0.47	*0.07*	*0.01*	*0.01*	*0.05*	*0.38*	*0.01*	*0.03*	*0.46*
p-value Rent = Cash	*0.23*	*0.00*	*0.01*	*0.03*	*0.01*	*0.65*	*0.04*	*0.39*	*0.18*
	0.25	*0.20*	*0.00*	*0.00*	*0.88*	*0.00*	*0.00*	*0.00*	*0.71*
	0.82	0.69	0.09	33.02	46.07	22.70	96.34	-6.38	-0.02
Mean Y in Control Group	0.83	0.71	0.09	33.58	46.86	23.09	97.99	-1.21	0.00
	0.82	0.69	0.07	29.40	43.53	19.95	83.66	-16.49	-0.07
	1,957	1,956	1,956	1,957	1,957	1,957	1,957	1,957	967
Observations	1,914	1,913	1,913	1,914	1,914	1,914	1,914	1,914	946
	1,914	1,913	1,913	1,914	1,914	1,914	1,914	1,914	946

Notes: The table reports the bounded treatment effects following Lee (2009), with bounds created for each variable by trimming the top and bottom of the control and cash drop group, as these groups had the lowest attrition. For each cell in the table, results are ordered as follows: unbounded, lower bound and upper bound. Details on the data sources and construction of the variables are included in the notes of Table 3. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome, plot size, survey-round dummies, and stratum dummies (see Equation (1) in the paper). In col. (10), we also control for laboratory fixed effect. In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (1) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

E.3 Target plot outcomes: Additional results

E.3.1 Individual inputs

Table E.10: Target plot outcomes: inputs

	Inputs		Seeds			Compost		Inorganic Fertilizer			Pesticide		Ox-Plough	Tractor
	Value	IHS	Use	Value	IHS	Use	Value	Use	Value	IHS	Use	Value	Use	Use
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
ITT														
Rental Subsidy	10.11***	0.24**	0.05**	7.63***	0.34***	-0.04**	-0.43	0.07**	2.07	0.12	0.02*	0.20	0.03	0.00
	[3.43]	[0.11]	[0.02]	[2.27]	[0.11]	[0.02]	[0.35]	[0.03]	[1.48]	[0.13]	[0.01]	[0.19]	[0.03]	[0.01]
Cash Drop	3.53	0.14	0.06**	3.59*	0.27**	0.00	0.29	0.02	0.29	0.02	0.01	-0.01	0.05*	-0.01
	[2.89]	[0.11]	[0.02]	[1.91]	[0.11]	[0.02]	[0.37]	[0.03]	[1.40]	[0.12]	[0.01]	[0.17]	[0.02]	[0.01]
p-value Rent = Cash	*0.05*	*0.33*	*0.68*	*0.07*	*0.52*	*0.02*	*0.06*	*0.06*	*0.21*	*0.40*	*0.23*	*0.24*	*0.61*	*0.61*
TOT														
Rental Subsidy Paid	13.93***	0.34**	0.06**	10.45***	0.47***	-0.05**	-0.59	0.10***	2.84	0.17	0.03*	0.27	0.04	0.00
	[4.48]	[0.13]	[0.03]	[2.97]	[0.13]	[0.02]	[0.47]	[0.04]	[1.94]	[0.15]	[0.02]	[0.25]	[0.03]	[0.02]
Cash Drop Paid	3.57	0.14	0.06**	3.59*	0.27***	0.00	0.29	0.02	0.29	0.02	0.01	-0.01	0.05*	-0.01
	[2.80]	[0.10]	[0.02]	[1.84]	[0.10]	[0.02]	[0.36]	[0.03]	[1.36]	[0.11]	[0.01]	[0.16]	[0.02]	[0.01]
p-value Rent = Cash	*0.01*	*0.08*	*0.79*	*0.01*	*0.08*	*0.01*	*0.05*	*0.01*	*0.13*	*0.26*	*0.12*	*0.19*	*0.96*	*0.65*
Mean Y in Control Group	33.02	IHS	0.81	13.07	IHS	0.14	2.18	0.63	16.06	IHS	0.06	0.59	0.45	0.05
Observations	1,957	509	1,957	1,957	509	1,957	1,957	1,957	1,957	509	1,957	1,957	1,957	1,957

Notes: The table reports treatment effects on the inputs used on the Target Plot. The inputs variable (used in cols. 1 and 2) is a composite of seeds, compost, inorganic fertilizer and pesticide. The value of each input, the inverse-hyperbolic-sine transformation of the value, and the dummy for use on the Target Plot are included in the table. IHS outcomes for compost and pesticides are not included as many plots did not use either input. Use of inputs is not included as results largely mirror cultivation rates, shown in Table 3. Details on the data sources are included in the notes of Table 4. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome, plot size, survey-round dummies, and stratum dummies (see Equation (1) in the paper). In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

E.3.2 Results by stratum

Table E.11: Target plot outcomes by stratum: plot use and crop choice

	Cultivated	Maize	Commercial
	(1)	(2)	(3)
Panel A: Stratum C			
ITT			
Rental Subsidy	0.01	-0.08**	0.07**
	[0.02]	[0.03]	[0.03]
Cash Drop	0.00	-0.03	0.01
	[0.02]	[0.04]	[0.03]
p-value Rent = Cash	*0.77*	*0.25*	*0.05*
TOT			
Rental Subsidy Paid	0.01	-0.11**	0.10**
	[0.03]	[0.05]	[0.04]
Cash Drop Paid	0.00	-0.04	0.01
	[0.02]	[0.04]	[0.03]
p-value Rent = Cash	*0.72*	*0.11*	*0.02*
Mean Y in Control Group	0.92	0.80	0.10
Observations	1,289	1,288	1,288
Panel B: Stratum NC			
ITT			
Rental Subsidy	0.14***	0.08	0.07*
	[0.05]	[0.05]	[0.03]
Cash Drop	0.16***	0.18***	0.04
	[0.05]	[0.05]	[0.03]
p-value Rent = Cash	*0.59*	*0.08*	*0.31*
TOT			
Rental Subsidy Paid	0.19***	0.12*	0.09**
	[0.06]	[0.06]	[0.04]
Cash Drop Paid	0.16***	0.18***	0.03
	[0.05]	[0.05]	[0.03]
p-value Rent = Cash	*0.60*	*0.35*	*0.12*
Mean Y in Control Group	0.62	0.50	0.07
Observations	668	668	668

Notes: The table reports treatment effects on agricultural outcomes on the Target Plot for plots that, according to the listing, the owner was planning to cultivate in the first study season (Stratum C, N=342) and plots that the owner was not planning to cultivate (Stratum NC, N=179) (see Section 3.3 in the paper for more details on the stratification). Details on the data sources and construction of the variables are included in the notes of Table 3. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome, plot size, survey-round dummies, and stratum dummies (see Equation (1) in the paper). In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

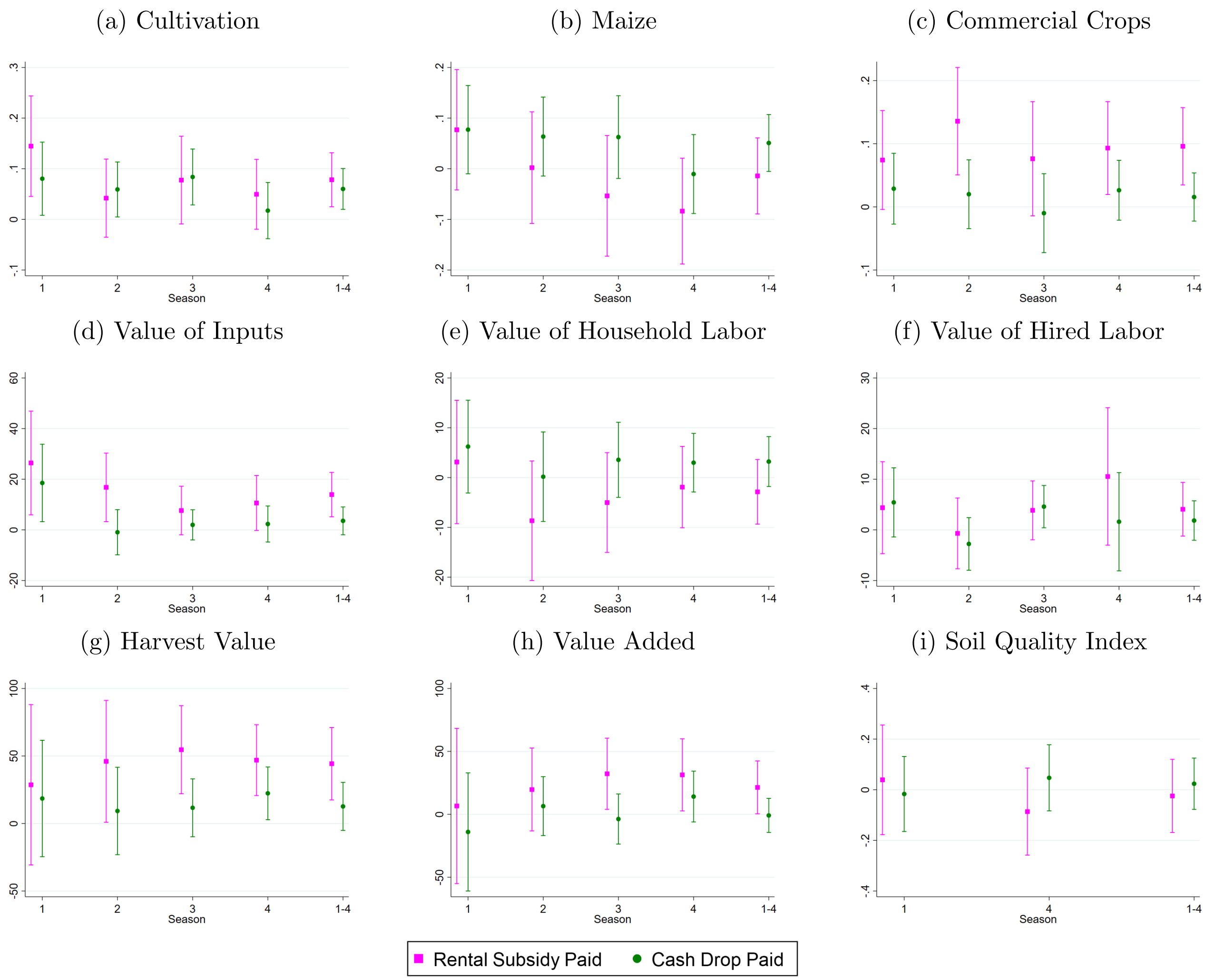
Table E.12: Target plot outcomes by stratum: inputs, output and soil quality

	Value of Inputs		Value of Household Labor		Value of Hired Labor		Harvest Value		Value Added	Soil Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Stratum C										
ITT										
Rental Subsidy	10.7**	0.16	-7.2**	-0.14	2.8	-0.02	30.9**	0.17	15.9	0.02
	[4.4]	[0.12]	[3.0]	[0.11]	[2.7]	[0.19]	[12.6]	[0.12]	[9.8]	[0.07]
Cash Drop	4.8	0.11	-3.6	-0.13	1.0	-0.05	17.6	0.01	9.7	0.06
	[3.3]	[0.12]	[3.0]	[0.13]	[2.6]	[0.17]	[11.2]	[0.13]	[9.1]	[0.07]
p-value Rent = Cash	*0.16*	*0.66*	*0.26*	*0.96*	*0.51*	*0.86*	*0.30*	*0.22*	*0.54*	*0.58*
TOT										
Rental Subsidy Paid	14.9***	0.22	-9.9**	-0.19	3.9	-0.03	42.4***	0.25*	22.0*	0.03
	[5.7]	[0.15]	[4.1]	[0.14]	[3.5]	[0.24]	[16.4]	[0.15]	[13.1]	[0.09]
Cash Drop Paid	5.0	0.11	-3.7	-0.13	1.0	-0.05	18.0	0.01	9.9	0.06
	[3.2]	[0.11]	[2.9]	[0.11]	[2.5]	[0.15]	[11.0]	[0.12]	[9.0]	[0.06]
p-value Rent = Cash	*0.06*	*0.37*	*0.11*	*0.65*	*0.39*	*0.90*	*0.11*	*0.10*	*0.33*	*0.69*
Mean Y in Control Group	34.5	IHS	53.1	IHS	23.7	IHS	103.0	IHS	-9.4	-0.01
Observations	1,289	335	1,289	335	1,289	335	1,289	335	1,289	640
Panel B: Stratum NC										
ITT										
Rental Subsidy	9.6*	0.38	6.7	0.18	3.5	0.18	43.1**	0.49	21.4	-0.10
	[5.3]	[0.25]	[4.6]	[0.28]	[3.3]	[0.31]	[19.3]	[0.30]	[14.6]	[0.11]
Cash Drop	1.8	0.18	15.5***	0.44	4.2	0.25	11.1	0.29	-16.7	-0.04
	[5.3]	[0.24]	[4.8]	[0.28]	[3.5]	[0.28]	[16.8]	[0.29]	[11.2]	[0.10]
p-value Rent = Cash	*0.13*	*0.40*	*0.09*	*0.36*	*0.84*	*0.81*	*0.10*	*0.51*	*0.01*	*0.57*
TOT										
Rental Subsidy Paid	13.3*	0.51*	9.5	0.25	4.9	0.24	59.1**	0.65**	31.0*	-0.13
	[7.0]	[0.28]	[6.3]	[0.32]	[4.4]	[0.35]	[25.3]	[0.33]	[20.0]	[0.13]
Cash Drop Paid	1.7	0.18	15.5***	0.44*	4.2	0.25	10.4	0.28	-17.1	-0.03
	[5.0]	[0.19]	[4.6]	[0.23]	[3.3]	[0.23]	[15.9]	[0.24]	[10.6]	[0.09]
p-value Rent = Cash	*0.06*	*0.16*	*0.33*	*0.50*	*0.86*	*0.98*	*0.04*	*0.22*	*0.01*	*0.43*
Mean Y in Control Group	30.1	IHS	32.8	IHS	20.8	IHS	83.7	IHS	-0.6	-0.05
Observations	668	174	668	174	668	174	668	174	668	327

Notes: The table reports treatment effects on agricultural outcomes on the Target Plot for plots that, in the listing, the owner was planning to cultivate in the first study season (Stratum C, N=342) and plots that the owner was not planning to cultivate (Stratum NC, N=179) (see Section 3.3 in the paper for more details on the stratification). Details on the data sources and construction of the variables are included in the notes of Table 4. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome, plot size, survey-round dummies, and stratum dummies (see Equation (1) in the paper). In col. (10), we also control for laboratory fixed effect. In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

E.3.3 Results by season

Figure E.2: TOT Coefficient Plots: plot use, crop choice, inputs, output and soil quality



Notes: These figures present the estimated TOT effects on the Target Plot. In each graph, the marker identifies each TOT coefficient with bars showing the 95% confidence interval around each coefficient. For details on how each estimate is generated, see Table 3 for the cultvation and crop choice results and Table 4 for the inputs and output results.

E.3.4 Soil sample analysis

Table E.13: Additional soil quality results

	Index	Nitrogen	Potassium	Phosphorus	Organic Carbon	pH
	(1)	(2)	(3)	(4)	(5)	(6)
ITT						
Rental Subsidy	-0.02	-0.01	-0.11	0.09	0.15	-0.03
	[0.06]	[0.05]	[0.21]	[1.27]	[0.66]	[0.04]
Cash Drop	0.02	-0.08*	0.22	1.55	-0.18	0.09**
	[0.06]	[0.04]	[0.22]	[1.32]	[0.60]	[0.04]
p-value Rent = Cash Paid	*0.46*	*0.15*	*0.13*	*0.26*	*0.60*	*0.00*
TOT						
Rental Subsidy Paid	-0.02	-0.02	-0.14	0.12	0.21	-0.04
	[0.07]	[0.06]	[0.27]	[1.60]	[0.83]	[0.05]
Cash Drop Paid	0.02	-0.08*	0.23	1.56	-0.18	0.09**
	[0.05]	[0.04]	[0.21]	[1.24]	[0.57]	[0.04]
p-value Rent = Cash Paid	*0.46*	*0.26*	*0.13*	*0.32*	*0.60*	*0.00*
Mean Y in Control Group	-0.02	1.39	5.89	21.56	22.51	5.60
Observations	967	967	967	967	967	967

Notes: The table reports treatment effects on agricultural outcomes on the Target Plot. The soil index in column (1) comes from two rounds of soil testing that we conducted at the end of seasons 1 and 4. The index combines the standardized versions of the 5 additional variables included in the table (nitrogen, potassium, phosphorus, organic carbon and pH value). The index is standardized against the control group. In columns (2)-(6) we winsorize the top 1%. In column (1) we winsorize the top and bottom 1%. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies. We control for plot size, baseline self-reported soil quality, laboratory fixed effects, survey-round dummies, and stratum dummies (see Equation (1) in the paper). In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

E.3.5 TFP

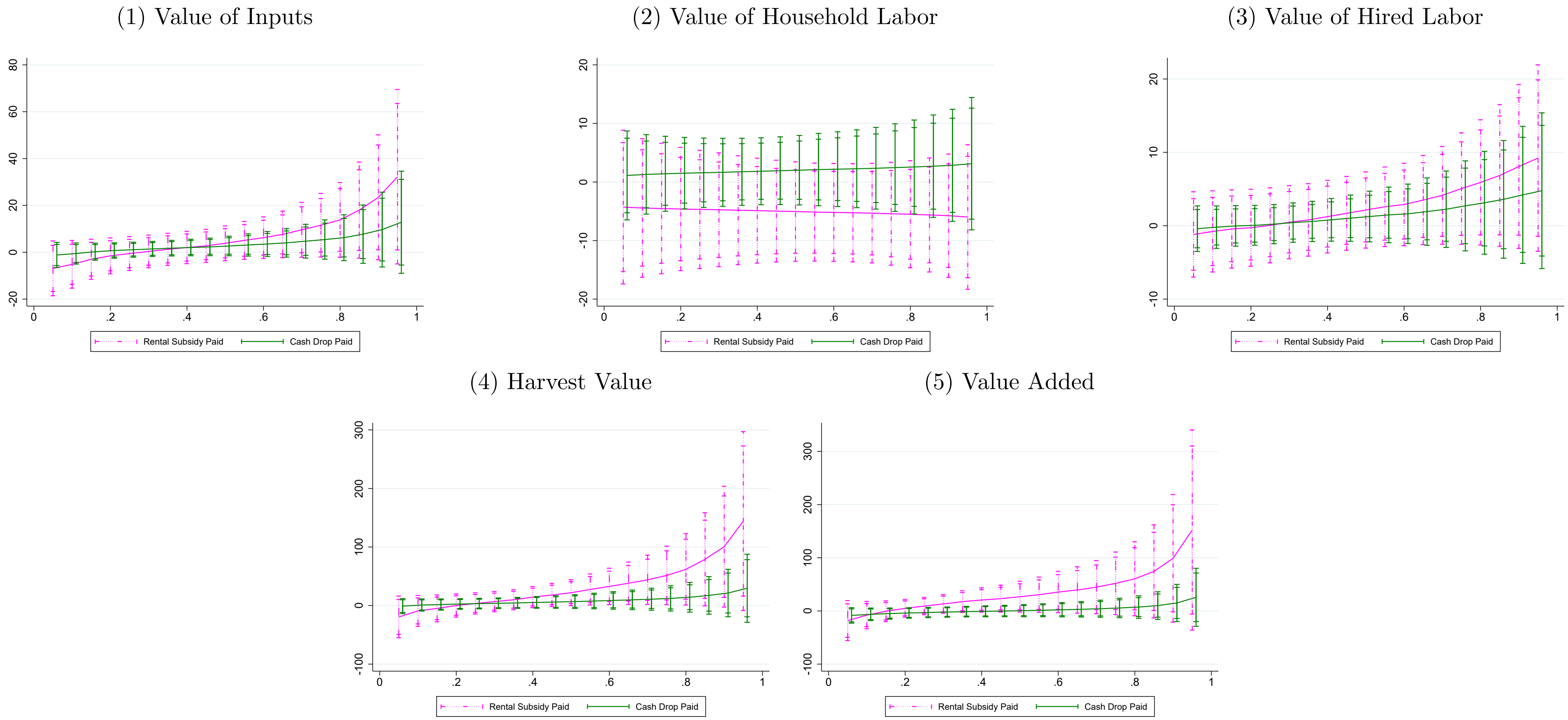
Table E.14: TFP results & robustness tests

	Core	Stratum C	Alternative Calibrations		
	(1)	(2)	(3)	(4)	(5)
ITT					
Rental Subsidy	4.69**	5.02*	7.59*	4.62**	4.19**
	[2.23]	[2.58]	[4.08]	[2.11]	[1.73]
Cash Drop	0.89	1.40	0.59	1.09	1.49
	[1.99]	[2.42]	[3.67]	[1.89]	[1.53]
p-value Rent = Cash	*0.10*	*0.18*	*0.10*	*0.11*	*0.14*
TOT					
Rental Subsidy Paid	6.08**	6.59**	9.83*	5.98**	5.43**
	[2.79]	[3.28]	[5.09]	[2.64]	[2.16]
Cash Drop Paid	0.91	1.44	0.62	1.11	1.51
	[1.93]	[2.37]	[3.56]	[1.83]	[1.48]
p-value Rent = Cash	*0.05*	*0.10*	*0.06*	*0.05*	*0.06*
Mean Y in Control Group	16.89	16.92	34.52	16.47	12.80
Land Share	.53	.53	.61	.391	.18
Labor Share	.43	.43	.26	.419	.46
Observations	1,621	1,142	1,621	1,621	1,621

Notes: The table reports treatment effects on the TFP of the Target Plot. The construction of the TFP variable is detailed in Section 6.3 of the paper. The table includes our core specification of TFP (col. 1), a specification restricted to stratum C (col. 2), and a range of alternatively calibrated TFP based on different factor shares (col. 3-5). Observations are restricted to farmers reporting a positive harvest value and labor quantity. TFP is calibrated against factor shares estimated in Gollin and Udry (2021) for Uganda (col. 1 and 2) and Tanzania (col. 3). Chen et al. (2021) include factor shares for Malawi and Valentinyi and Herrendorf (2008) for the U.S., which are used in column 4 and column 5, respectively. In the *ITT* Panel, we run an ANCOVA regression of the outcome on treatment dummies, controlling for baseline values of the outcome, plot size, survey-round dummies, and stratum dummies (see Equation (1) in the paper). In the *TOT* Panel, we run an ANCOVA regression with the same controls, but we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment (see Equation (2) in the paper). We cluster standard errors by the Target Plot. *p<0.1, **p<0.05, ***p<0.01.

E.3.6 Quantile regressions

Figure E.3: Quantile regression results



Notes: The figure reports TOT coefficients from instrumental variable quantile regressions of agricultural outcomes on the Target Plot. Each dependent variable is the average across four seasons, with one observation per Target Plot. Additional details on the construction of the variables are included in the notes of Table 4. We run an ANCOVA regression controlling for baseline values of the outcome and we instrument dummies for whether the respondent took up the treatment in any of the four seasons with the treatment assignment.

F Comparison of our treatment effects to the predictions of a misallocation exercise based on baseline productivity dispersion

A common misallocation exercise is to quantify the predicted effect of a *full* reallocation of land, until its marginal productivity is equalized across farmers, where the reallocations and their predicted effects are based on baseline estimates of productivity and a production function (Adamopoulos et al., 2022b). This is a different exercise, and potentially a very different set of land trades, from the *marginal* reallocation induced by our experiment. Nevertheless, it is instructive to compare our treatment effects to the predicted effects from full reallocation, to see where differences arise. We do so in two steps: (1) based on baseline measures of productivity, we compare the predicted effects on output from fully reallocating land among farmers (until the marginal product of land is equalized across farmers), as per the misallocation exercise, to the predicted effect of the actual rentals induced by the subsidy; (2) for the actual induced rentals, we compare their predicted effects on output on the Target Plot to their actual treatment effects on the Target Plot.

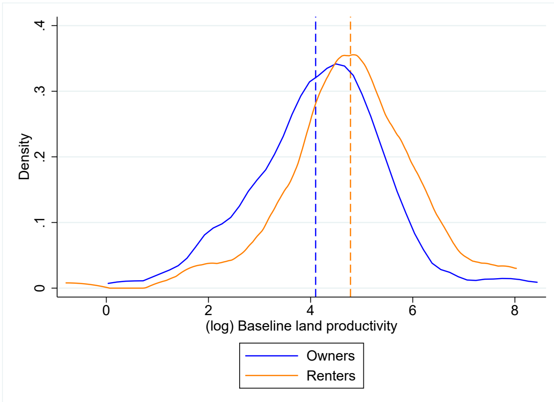
For the exercise, when necessary, we assume a Cobb-Douglas production function at the farm level, for farmer i: $Y_i = A_i L_i^{\alpha}$, where Y_i is total revenue, L_i is total land, A_i is TFP (estimated as a residual, using baseline data), and α, assumed constant across farmers, is the returns to scale.[4] Under full reallocation, land is reallocated across farmers until the marginal product of land, $\alpha Y_i/L_i$, is equalized across farmers. We calibrate $\alpha = 0.54$ based on Adamopoulos et al., 2022b; results are similar if we instead estimate α from the data ($\hat{\alpha} = 0.59$), but we do not have instruments for input use as in Gollin and Udry, 2021 and so are vulnerable to well-known biases in production function estimation when doing so.

F.1 Comparing predicted effects of induced trades vs. full reallocation

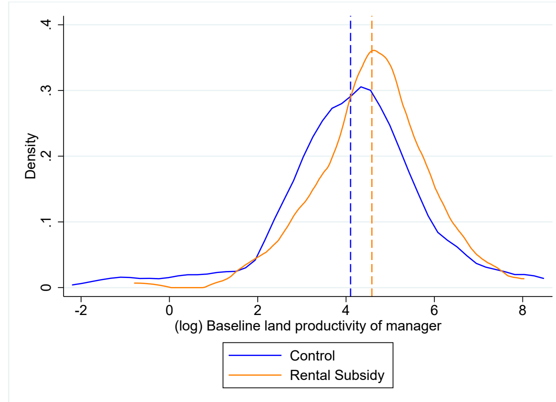
As a first step, in Figure F.1, we plot the distribution of the (log) marginal product of land across farmers, defined simply as $log(Y_i/L_i)$. Panel a) shows a comparison of the distribution of baseline land productivity among owners vs. renters whenever the Target Plot was rented out, pooling across the control and rental subsidy group. Renters have higher productivity than owners on average — the distribution is shifted to the right — showing that rentals are on average predicted to increase output and decrease misallocation. Panel b) shows a comparison of the distribution of baseline land productivity of managers of the Target Plot in the control group vs. the rental subsidy group. In this case, the shift to the right of the distribution shows that marginal rentals — those induced by our rental subsidy — are also predicted to decrease misallocation. However, from these figures alone, it is difficult to infer how much of the potential gains from full reallocation are predicted to be achieved by the rentals which occur.

To calculate the predicted gains from the reallocation induced by the rental subsidy, we calculate the predicted gains from rentals in the rental subsidy group and then net out the predicted

[4] As we explained in Section 3.1, we conducted the listing and the owner baseline while harvesting for the 2019 Long Rains was still ongoing and, thus, we are missing information on harvest amount for a large portion of the sample for that season. In this section, we thus use harvest amount in the previous season, i.e., the 2018 Short Rains crop season, but we do not have information on input values for that season. For these reasons, in this section, we use a one-input (land) production function. We also report results with Y_i as revenue minus the cost of monetary inputs, but this is a secondary specification, as we impute monetary inputs in the baseline data based on input use and plot size, rather than quantity used, because of limitations in the baseline data. We do not add labor in the production function, or normalize by it, due to similar limitations in baseline data. We highlight this is a different approach from the one in Section 6.3, where we computed *endline* TFP using also labor and non-labor inputs.



(a) Owners vs. Renters



(b) Managers, Control group vs. Rental subsidy group

Figure F.1: Baseline dispersion in (log) land productivity

gains from rentals in the control group.[5] The gains are reported in Column (2) of Table F.1. For comparison, Column (1) reports the total baseline farm revenue of owner and renter households in the rental subsidy group. The induced rentals are predicted to increase total revenue by $1,360, corresponding to a 1.6% increase. We note that this increase arises from approximately 9% of the total land in the rental subsidy group changing management.[6]

To calculate the predicted potential gains from full reallocation, we consider an output maximizing relocation of land, that which equalizes the marginal product of land across farmers. We take as the set of farmers across which the reallocation can occur to be all owners and renters in the rental subsidy group, the most inclusive set of farmers we have (ideally, for a full reallocation exercise, we would have the universe of farmers). Solving for the optimum gives the following allocation, based on baseline estimates of TFP:

$$L_i^* = \frac{\hat{A}_i^{\frac{1}{1-\alpha}}}{\sum_j \hat{A}_j^{\frac{1}{1-\alpha}}} \sum_j L_j$$

We then compare predicted total revenue under this allocation to predicted total revenue under the allocation in the control group, resulting in a predicted treatment effect which is comparable to that of the induced rentals. The predicted gain from full reallocation is reported in Column (3); total revenue of owners and renters in the rental subsidy group would increase by $175,000, a %205 increase. This is a very large increase, but not inconsistent with other estimates of gains

[5] We have to proceed in two stages, as we cannot separately identify compliers from always takers in the rental subsidy group – that is, we cannot identify those who were induced by the subsidy to rent from those who would have rented anyway. First, we calculate the predicted gain from the rentals which occur in the control group (the always takers). We do so by summing, across rentals, the predicted output gains for the renter minus the predicted output losses for the owner:

$$(A_r(L_r + L_{TP})^\alpha - A_r(L_r)^\alpha) - (A_o(L_r)^\alpha - A_r(L_r - L_{TP})^\alpha)$$

where r denotes renter, o owner, and TP target plot. We then calculate the predicted gains from rentals in the rental subsidy group in the same manner, and then the predicted treatment effect of the induced rentals is the difference of these two quantities.

[6] The average Target Plot size is 0.7 acres, while the average landholdings are 2.7 acres for owners and 1.4 acres for renters (whom we have for 70% of owners), so Target Plots account for approximately 19% of total land; and the predicted ITT gains come from the 45% of Target Plots which are marginal.

from full reallocation (e.g. Chen et al., 2021). The predicted gain from full reallocation is thus two orders of magnitude larger than the predicted gains from our induced rentals. Since full reallocation reallocates at most one order of magnitude more land than the induced rentals (which reallocate 9% of land), the difference shows that the trades which are induced are not those with the largest potential revenue gains (as is also suggested by Figure F.1). This is perhaps not surprising, especially given the constraints on that set of rentals which our experiment can induce (only owners can rent out, and only up to one plot per owner).

One substantial caveat of our full reallocation exercise is that we base our measures of farm productivity on data from one (baseline) season, and thus cannot do the steps to remove measurement error undertaken in related papers; the resulting measurement error will bias us towards overestimating the potential gains from full reallocation.

Table F.1: Predicted treatment effects on total revenue: full reallocation vs. induced reallocation

	No reallocation level (1)	Actual rentals treatment effect (2)	Full reallocation treatment effect (3)
Total revenue	85,200	1,360	175,000

F.2 Comparing predicted effects to experimental effects among induced rentals

We undertake this comparison for outcomes on the Target Plot, rather than at the farm level, because as explained in Section 3.2, the experimental design does not give a renter counterfactual (e.g., for renter's farms). We thus need to arrive at predictions for Target Plot outcomes using our farm-level production function, when calculating the predicted change in output for a given rental. We do so in three ways: 1) assuming owners and renters achieve their average output on the Target Plot:

$$A_r(L_r + L_{TP})^\alpha \frac{L_{TP}}{L_r + L_{TP}} - A_o(L_o)^\alpha \frac{L_{TP}}{L_o}$$

2) assuming that the Target Plot is marginal, in the sense that rentals induce no spillovers to outcomes on other plots of owners and renters (as we find empirically for owners), in which case the predicted farm-level treatment effect above is identical to the predicted treatment effect on the Target Plot:

$$(A_r(L_r + L_{TP})^\alpha - A_r L_r^\alpha) - (A_o L_o^\alpha - A_o(L_r - L_{TP})^\alpha)$$

3) using a first order approximation, based on the difference in the marginal product of land:

$$(\alpha\frac{Y_r}{L_r} - \alpha\frac{Y_o}{L_o})L_{TP}$$

These predicted treatment effects on average Target Plot revenue are reported in Table F.2, Columns (2)-(4) respectively. To benchmark them, Column (1) reports the average Target Plot revenue at baseline (short rains 2018). Predicted average treatment effects are estimated to be between \$9 and \$23, corresponding to a 13% to 32% increase. Column (5) reports the corresponding estimated average treatment effect based on our endline data— the ITT of the rental subsidy minus the ITT of the cash drop to control for the income effect — which is \$19.7 corresponding to a 28% increase. The predicted treatment effects on Target Plot revenue are thus consistent with, and if anything slightly smaller than, the estimated treatment effects.

Table F.2: Treatment effects of induced rentals on average Target Plot revenue: predicted vs. experimental

	Baseline mean	Predicted effect			Experimental effect
		Production function average productivity	Production function marginal productivity	First-order approximation	
	(1)	(2)	(3)	(4)	(5)
Revenue on Target Plot	70.5	10.5	8.87	22.9	19.7