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Unfair Incentives: A Behavioral Note on Sharecropping*

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Abstract

We conducted a field experiment with real-life tenants in Ethiopia to test the incentive effects of fixed-wage, sharecropping, fixed-rent, and ownership contracts. The experimental task resembles a common process in agricultural production. The sharecropping contract is essentially a piece-rate scheme framed as a profit sharing agreement. The sharecropping output was about 11 percent smaller than the fixed-rent output. Surprisingly, it is statistically indistinguishable from the fixed-wage output, despite substantial piece rates. This effect is driven by real-life sharecroppers. Their sharecropping output was significantly smaller than that of non-sharecroppers, and in one region, it was even 10 percent lower than sharecroppers' fixed-wage output. Based on qualitative interviews and historical accounts, we argue that our subjects dislike sharecropping contracts because of the unfair profit sharing and the controversial allocation of the land. The contractual performance may therefore depend on the perceived fairness of the incentive scheme.

Keywords: Agricultural contracts, Ethiopia, Fairness, Field Experiment

JEL classification codes: C93, J30, N50

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1 Introduction

There are four polar cases of how to contract land and labor, and hence the claim to the yields from the land: wage labor, sharecropping, fixed rent, and owner cultivation. The term sharecropping refers to a contractual relationship between a landlord and a tenant allowing the latter to use the land of the former in return for a share of the crops produced on this land. The discussion of the negative incentive effects of sharecropping is almost as old as the discipline of economics. Adam Smith and John Stuart Mill were early proponents of the English fixed-rent leasehold tenancy vis-à-vis the French *metayage* (share tenancy). Alfred Marshall argues along the same lines in *Principles of Economics*, claiming that sharecropping leads to an undersupply of effort, as tenants receive only a fraction of their marginal output.

Empirical evidence on the Marshallian inefficiency is usually gathered by evaluating input intensities for farmers who sharecrop and own (or sharecrop and rent) at the same time, see Binswanger and Rosenzweig (1986), Shaban (1987), and Laffont and Matoussi (1995). However, the motive for sharecropping a plot,[1] rather than choosing another contractual form, is hard for the econometrician to observe in observational data. In particular, this is true if preference-driven matching processes are concerned (Ackerberg and Botticini 2002). Farmer characteristics such as ability, risk and time preferences, but also environmental conditions (e.g. soil quality, differences in moisture, patterns of water runoff) are difficult to observe. They bias the estimates if they correlate with the contract choice and input intensities in the crop production process. The literature has addressed these concerns by neutralizing time-invariant farmer characteristics through the comparison of sharecropped and owned plots for the same farmer (Bell 1977, and many others thereafter); or by differencing away unobserved land heterogeneity through spatial fixed-effects (Goldstein and Udry 2008),[2] plot-fixed effects (Newman et al. 2012), or landlord–tenant pair fixed-effects (Deininger et al. 2013).

To circumvent these biases, we employ a field experiment to counterfactually estimate the incentive effects of fixed wage, sharecropping, fixed rent, and ownership contracts. The experimental method allows us to randomly assign subjects to contracts and thereby to avoid endogeneity problems. Our subjects are a representative sample of farmers in rural Ethiopia (from the regions of Amhara and Oromia), a country highly dependent on agriculture and possessing a widespread culture of sharecropping. The experimental task resembles a common agricultural task: collecting beans of a particular color out of a bucket filled with multicolored beans. The performance was measured as the weight of beans the subjects separated from the rest within a given period of time. The

[1] Contract theory identifies a number of mechanisms rendering sharecropping a preferable contract option, e.g., risk-sharing, moral hazard on the part of the tenant, financing constraints, and screening among tenant types (see Singh 1991 for a comprehensive review).

[2] That study is not on Marshallian inefficiency, but on the relationship between land rights and agricultural investment. However, the identification strategy could equally well be applied to estimate the impact of agricultural contracts on the choice of input intensities under unobserved land heterogeneity.

earnings in the experiment correspond to the typical daily expenditures of our subjects.

In the sharecropping treatment, subjects earned one-half of the "value" they produced. This is essentially a piece rate scheme. However, piece rate schemes do not exist in our subjects' experience, while sharecropping contracts play a prominent role. For this reason, the piece rate scheme was framed for our subjects as a sharecropping contract. The design of the other treatments is straightforward: in the fixed wage treatment, subjects earned a fixed fee, independently of their output; in the fixed rent treatment, the subjects earned the full value of their output minus a fixed rent; in the ownership treatment, they earned the full value without deductions.

Our results are surprising and informative about the potential incentive effects of sharecropping. Indeed, we find that the average output under full marginal returns (under the fixed rent and ownership treatments) is significantly larger than under the sharecropping treatment. Compared to the fixed wage treatment, the implied Marshallian inefficiency is about 11 percent. As Table 1 shows, this number is comparable to those typically found in survey data for Ethiopia (Pender and Fafchamps 2006, Deininger et al. 2008, and Deininger et al. 2011).[3]

[Insert Table 1 about here]

Unexpectedly, we find no significant difference in the average output between the fixed wage and the sharecropping treatments, despite a relatively small fixed wage (the subjects earned the same amount in both treatments if they produced a relatively small amount) and a substantial piece rate in the sharecropping treatment (the earnings in the real-effort task covered the typical daily expenditure of our subjects). This is remarkable, given that the empirical literature which analyzes productivity differences between fixed wage and piece rate schemes consistently finds large increases in productivity: on the order of 20 percent.[4]

We find that the subjects' background matters for real-effort output under the sharecropping treatment. Subjects who work under sharecropping incentives in their real life produced significantly less under this treatment than non-sharecroppers. Under any other treatment, there is no significant difference in output between sharecroppers and non-sharecroppers. Hence, the differences in the sharecropping output cannot be attributed to ability. This effect is especially pronounced in the region of Amhara. Real-life sharecroppers in this

[3] Note that, in the table, we show the Marshallian inefficiency comparing the fixed rent and the ownership contracts, not the fixed rent and the wage contracts, to increase the comparability with the findings in the survey data.

[4] A number of papers have compared the performance of piece rate and fixed wage contracts. In his famous Safelite study, Lazear (2000) finds a 44 percent increase in productivity when the payment scheme changes from hourly wages to piece rates. One-half of the effect can be attributed to an increase in the effort on the part of the average worker. Shearer (2004) uses data from a tree-planting firm that randomly assigned fixed wages or piece rates to a subset of workers. He observes a 20 percent increase in productivity. Shi (2010) conducted field experiments in a tree-thinning setting and found similar differences between fixed wage and piece rate compensations.

region even produced 10 percent less output under the sharecropping treatment than under the fixed wage treatment. It is impossible to explain this finding in terms of incentives.

To reconcile these results, we analyze the qualitative interviews we conducted after the experiment and draw on historical accounts. We noted substantial dissatisfaction of our subjects with the sharecropping arrangements. Many real-life sharecroppers complained about the unfair profit sharing where the landowner does not contribute work or other inputs. Such complaints were especially prevalent in Amhara where, in 1997, a highly controversial land reform took place. For political reasons, the reform favored some groups, deprived others, and in its aftermath pushed a substantial number of peasants out of subsistence agriculture and into land markets. Historical accounts (Ege 1997, 2000) report widespread corruption. In the interviews, a number of subjects from Amhara complained that today's landlords are those who bribed officials in order to manipulate the allocation of land.

Based on these insights, we provide the following explanation for our results: the sharecropping treatment reminds real-life sharecroppers of the unfair profit sharing agreement they are exposed to in their real-life contract. In Amhara, it further reminds them of the unfair distribution of land in their community. Both effects lower the motivation for their effort, so that real-life sharecroppers produce less output under the sharecropping treatment than non-sharecroppers, especially in Amhara. We therefore conjecture that fairness concerns matter for the performance of incentive contracts like sharecropping (in particular, because the contract ties the landlord's payoff to the tenant's effort). As far as we know, this behavioral aspect of sharecropping has not been examined yet.

The rest of this paper is organized as follows. Section 2 describes the experimental design and procedures. Section 3 examines the experimental results. In Section 4, we provide an interpretation of the results based on qualitative interviews with our subjects and historical accounts. Section 5 concludes. An online Appendix contains robustness checks and additional material.

2 Experimental Design

Sampling. The study sites were selected in three stages. First, we defined the sample universe to consist of households in Amhara and Oromia, the two economically most active and most populated regions of Ethiopia. Then we sampled two districts ("Woredas") in each region (Gozamen and Bahir Dar Zuria in Amhara, Adaa and Girar Jarso in Oromia). Districts are composed of peasant associations ("Kebeles"), which are the smallest administrative unit in Ethiopia. We randomly selected Kebeles within Woredas, five in Adaa, five in Girar Jarso, eight in Gozamen, and seven in Bahir Dar Zuria.[5]

[5] Sampling by clusters in this three-stage design generates a sample in which households are grouped geographically. Since the clusters vary in population size, the households are sampled with unequal selection probabilities. While the main paper shows the results from unweighted regressions in the empirical analysis, the online Appendix re-does all regressions

Census data on the different Ethiopian Kebeles are not always up to date. Hence, for the sampling of households at the Kebele level, we drew on registers kept by Kebele chiefs. These registers are widely thought to be up to date, as claims to land are documented in them. Randomization at the Kebele level was done by dividing the N households living in the Kebele by 22 (for 20 peasants and 2 replacements in case the sampled peasants do not show up) and then inviting every $N/22$th household head from the list. We invited the head rather than a randomly selected household member, because the experiment was accompanied by a detailed quantitative and qualitative household survey and household heads were considered the most appropriate person to supply this information.

The data collection took place in May 2011 before the start of sowing at the beginning of the crop cycle. Household heads could thus spare time to participate in our data collection. Upon invitation, they were told that they could earn an unspecified amount of money during a research visit from a team from Addis Ababa University. The enumerators had to refer to the replacement household heads only in very few cases.

Design. Each session was organized as follows. We arrived in the village and met the local organizers. Then we met with the sampled peasants in venues such as communal centres or health centres. After the enumerators introduced themselves, a text was read to the peasants (by the same enumerator in all sessions).[6] The peasants received a 20 Birr fee for showing up.[7] They were told that they would be allowed to quit the experiment at any time if they felt uncomfortable with it. However, all of the peasants chose to accomplish the task.

The real-effort task was a sorting task that resembles an agricultural production process typical for Ethiopia.[8] Each participant received two buckets: First, a flat blue bucket with a diameter of 22 centimeters. At the beginning of the experiment this bucket was filled with beans of three different colors (red, yellow, and white). There were 700 grams of beans of each color in each bucket. Second, a raised red bucket with a diameter of 16 centimeters. At the beginning of the experiment this bucket was empty. We asked the peasants to collect as

shown in the main paper using population-weights to take into account unequal the selection probabilities. All the important findings presented in this paper are robust to this change in specification.

[6] The formulations were taken (and slightly paraphrased) from the scripts provided at the *Experiments in 15 Small-Scale Societies* website, see http://www.hss.caltech.edu/~jensming/roots-of-sociality/ (accessed on Feb 15th, 2011).

[7] At the time of data collection, the exchange rate was roughly 1 Birr $\approx$ 0.06 USD.

[8] Two examples: First, coffee is Ethiopia's main export crop. Dry processing of coffee beans is common. The first step in processing coffee is cleaning, which involves winnowing done by hand using a sieve. In the process, the berries are sorted, and cleaned from dirt, soil, twigs and leaves. Unwanted berries are picked out from the top of the sieve and removed. Second, fruit plantations such as Avocado, banana, citrus, grape, pineapple, papaya, mango, peach, and apples are also quite common in Ethiopia. Pests of fruit crops are a major problem in all areas of fruit production, as fruit flies are attracted by ripening or fermenting fruit. One way of dealing with fruit flies is sorting and separating ripe from unripe fruit after harvest.

many beans of a particular color as they could and put them into the red bucket. They had 15 minutes for this task. The performance was measured as the weight of the beans the peasants managed to separate. The enumerators explained to the peasants that the "value" of 10 grams of real-effort output was 4 Birr, and they showed how the output was to be measured after the experiment.[9]

The peasants were randomly assigned to four different contract treatments. Let x be the real-effort output in grams. Under the fixed-wage (fw) treatment, the peasants were compensated with the fixed amount of 20 Birr for doing the real-effort task, regardless of their output,

$$m = 20 \ Birr.$$

Under the sharecropping (sh) treatment, the enumerators told the peasants that their compensation would be one-half the value of the real-effort output. An equal split of the output between the landlord and the tenant is commonly observed in real-life sharecropping arrangements. Hence, the payoff under the sharecropping treatment is

$$m = x \times 0.2 \ Birr.$$

This compensation is essentially a piece rate scheme. However, in contrast to sharecropping arrangements, piece rates are not a common form of contracting for our subjects. Our subjects therefore perceived the piece rate scheme as a sharecropping contract.

In the fixed rent (fr) treatment, the compensation of the peasants is the value of the output minus a "rent" of 20 $Birr$. Hence, the payoff is

$$m = x \times 0.4 \ Birr - 20 \ Birr.$$

Finally, in the ownership (ow) treatment, peasants earn the total value of the real-effort output, so that their payoff is

$$m = x \times 0.4 \ Birr.$$

These payment schemes were not chosen arbitrarily. In non-incentivized preparatory sessions with enumerators and students, the average output was 110 grams. We chose the payoff structure for each contract to avoid that inappropriate scaling of the fixed wage contract influences real-effort output. For 100 grams, the payoff under the first three treatments is 20 Birr, and the payoff under the ownership treatment is 40 Birr. This is a sizable amount of money given that the average monthly per capita expenditure of our subjects is 721 Birr (i.e., 24 Birr per day so that in the 15-minute experiment, the subjects were easily able to earn their daily expenditures).[10]

[9] Figures 1 and 2 in the online Appendix have pictures of the different colored beans and the of the experimental equipment.

[10] Table 1 in the online Appendix has descriptive statistics on the per capita expenditure levels. It also presents some correlations between the choice of real contracts and per capita expenditure levels, implying that there may be a welfare dimension to the choice of agricultural contracts in real life.

Predictions. The experimental design allows us to compare the incentive effects of the four different contract arrangements. Denote by x_i the average output in treatment i. Since the marginal payoffs are equal in the fixed rent and ownership treatments, we expect the average output in both treatments to be equal:

$$x_{fr} = x_{ow}.$$

The extent of Marshallian inefficiency is measured by the difference between the average output under the sharecropping treatment and the output under the fixed rent treatment. We conjecture that this difference is strictly positive.

$$\text{Marshallian inefficiency } = x_{fr} - x_{sh} > 0.$$

Since in the preparatory sessions, the average fixed wage output exceeded 100 grams, the subjects could earn more while sharecropping than under the fixed wage treatment if they just produced the same output. However, under the sharecropping treatment, they have an incentive to produce more output. Consequently, we conjecture that the average output under the sharecropping treatment significantly exceeds the average output under the fixed wage treatment:

$$x_{sh} > x_{fw}.$$

Experimental Procedure. In order to rule out peer effects,[11] we adopted the following protocol. The enumerators arranged for the peasants to sit in a circle with adequate distances between them (the experiment was conducted outside, on meadows, to allow enough space).[12] They asked the peasants to turn so that their backs were facing the inside of the circle. To make sure that the peasants did not hear each other performing the task, the enumerators covered the bottom of the bucket with a paper towel so that the beans did not produce a sound when they were dropped into the bucket. They also turned on music on portable speakers. Given that the paper towel worked well, this was more useful in entertaining the peasants during the task rather than preventing them from hearing each other. The same song was played twice in each session.

During the real-effort task, the enumerators stood inside the circle making sure that the peasants did not turn their heads to watch each other. After the allotted time was up, they collected the red buckets from the peasants and weighed their content on a scale. The payoffs were determined according to the type of contract and the weight achieved. After the real-effort task, we conducted an extensive survey and qualitative interviews. The payments were made at the end of the data collection.

There were 25 sessions in all. The sharecropping, fixed rent, and ownership treatments were each assigned six times. The fixed wage treatment was assigned seven times. Each session had 20 peasants. The enumerators carefully

[11] The peasants might increase their efforts if they watched other peasants exercise a high effort, or the reverse, see Falk and Ichino (2006), Mas and Moretti (2009).

[12] Figure 3 in the online Appendix shows a picture of the experimental setup during the real-effort task.

explained the real-effort experiment. They showed how to carry out the task, and the peasants answered control questions to demonstrate that they understood the task. The enumerators also told the peasants how they would be paid. Explaining and conducting the real-effort experiment usually took more than 60 minutes. The whole experimental session took one-half of the day. In addition to the 20 Birr appearance fee, the peasants earned, on average, 20 Birr under the fixed wage treatment, 26.4 Birr under the sharecropping treatment, 39.8 Birr under the fixed rent treatment, and 58.4 Birr under the ownership treatment.

Effort and Skill. The experimental task resembles the sorting and picking processes commonly found in the agricultural production processes in rural Ethiopia, but it also resembles such processes in the domestic sphere. While agricultural production is infrequent and seasonal, domestic work is done on a daily basis. This implies that subjects who are mainly engaged in house work outside the experiment may bring specific skills in exercising the light manual task in the experiment. As argued in detail in Kemper and Unte (2014), women in areas where natural conditions favored the introduction of the plough clearly devote more time to domestic work than women in areas where natural conditions are unfavorable to the plough. Hence, they have specific skills in exercising the real-effort task. The output of women from plough-positive areas may be determined by both effort and skills. For this reason, we removed 27 women from plough-positive areas from the sample in the empirical analysis below.[13]

3 Results

We first provide an overview of the subjects' behavior in the real-effort task. Table 2 shows the average output for each treatment, and Table 3 shows the mean differences in output for all combinations of treatment pairs.

[Insert Table 2 and Table 3 about here]

The difference in output between the fixed rent and ownership treatments is 2.4 percent and statistically not significant. We find that the outputs under the fixed rent and ownership treatments are significantly larger than under the sharecropping treatment. The output under the fixed rent treatment was 12.0 percent larger than under the sharecropping treatment. The output under the ownership treatment was 9.6 percent larger than under the sharecropping treatment. Thus, there is a Marshallian inefficiency in our real-effort experiment.

The outputs under the fixed wage and sharecropping treatments cannot be statistically distinguished. Under the fixed wage treatment, the output is even 1.6 percent higher than under the sharecropping treatment (this difference is not

[13] The main findings presented below are robust to the inclusion of women from plough-positive areas in the sample as well as excluding women (i.e., women from plough-positive and plough-negative areas) from the sample.

statistically significant). This is surprising, since the empirical contract literature usually reports a large and positive increase in output when the incentive scheme moves from fixed wages to piece rates.

[Insert Table 4 about here]

Table 4 confirms these findings in a broad set of weighted regressions. The regressions were estimated with controls for the subjects' personal characteristics (controls A), controls for the subjects' households characteristics (controls B), controls for the subjects' agricultural production characteristics (controls C), and controls for the subjects' participation in various government programs (controls D).[14] Standard errors are clustered at the session level to account for common background characteristics of subjects in a particular session.

The estimated coefficients for the fixed rent treatment clearly point towards a Marshallian inefficiency. When we include all controls in the regression, the output is estimated to be 10.9 percent higher under the fixed rent treatment than under the sharecropping treatment. For the ownership treatment, the coefficients are somewhat lower, but still significant. The output increases by 8.2 percent. All coefficients are significant, at least at the 10 percent level. Again, the outputs under the fixed wage and sharecropping treatments cannot be statistically distinguished.

Result 1. *The Marshallian inefficiency in our real-effort task amounts to 10.9 percent.*

Result 2. *The sharecropping output cannot be statistically distinguished from the fixed wage output.*

From an incentive perspective, Result 2 only makes sense if we conclude that the piece rate in the sharecropping treatment is too low to induce subjects to work more than in the absence of any incentive. An alternative explanation could be that the framing of the piece rate as a sharecropping contract induced relatively low efforts under this treatment. If this were true, the framing effect should be strongest for real-life sharecroppers. We therefore compare the average output of the real-life sharecroppers with that of non-sharecroppers for each treatment. Table 5 presents the results.

[Insert Table 5 about here]

[14] See the Appendix for the definitions of the variables. In the online Appendix, Tables 2 to 5 present balancing tests for the control variables. They show that 4 out of the 31 variables are significantly correlated with the sharecropping treatment; 3 out of 31 are significantly correlated with the fixed rent treatment; and 6 out of 31 are correlated with the ownership treatment (the reference treatment in all regressions is the fixed wage treatment). Due to the fairly low number of sessions per treatment, the balancing may not have succeeded perfectly. Thus, we decided to demonstrate the robustness of the results against a broad range of controls.

Sharecroppers and non-sharecroppers produce the same output under all treatments except the sharecropping treatment, under which they produce 8.3 percent less than non-sharecroppers. The difference is significant at the 1 percent level. We will show that this effect is especially pronounced for real-life sharecroppers in the region of Amhara.

We further examine the behavior of the real-life sharecroppers by extending our basic regressions. We include a binary indicator for real-life sharecropping and a binary indicator for the region of Amhara. Furthermore, we include interaction terms for sharecropping treatment, real-life sharecropping, and Amhara. Table 6 displays the results.

[Insert Table 6 about here]

The linear regressions confirm that, in general, real-life sharecroppers and non-sharecroppers produce the same output. However, under the sharecropping treatment, real-life sharecroppers produce 6.1 percent less than non-sharecroppers. Again, the effect is especially strong in the region of Amhara. It can be observed that the triple interaction between sharecropping treatment, real-life sharecropping, and Amhara is significant in all regressions.

Given that Amhara is a regional dummy, it could, in principle, pick up any aggregate effect that systematically varies between Amhara and Oromia. These could be caused by differences in diligence or attitudes towards work on the part of people in the two regions, also captured by the regional dummy. However, if this were the case, we should observe a lower output under the other contract treatments as well. As a robustness check, we redid the regression from Table 6 while replacing sharecropping with fixed rent and ownership to test this hypothesis.[15] We find no difference for real-life sharecroppers in Amhara for these contract treatments.

[Insert Table 7 about here]

Running the same regression using only data from Amhara, we observe that real-life sharecroppers from this region produce 9.7 percent less under the sharecropping treatment than under the fixed wage treatment, see Table 7. Real-life sharecroppers in Oromia produce 5.0 percent less output under the sharecropping treatment than under the fixed wage treatment. This difference, however, is not significant (see Table 8 in the online Appendix). We summarize these results.

Result 3. *Under the sharecropping treatment, real-life sharecroppers produce significantly less output than non-sharecroppers. This effect is especially pronounced for real-life sharecroppers in Amhara. They even produce* 9.7 *percent less under the sharecropping treatment than under the fixed wage treatment. Under all other treatments, there is no significant difference between real-life sharecroppers and non-sharecroppers.*

[15] See Tables 6 and 7 in the online Appendix.

4 Interpretation

The regional variation and the behavioral differences between sharecroppers and non-sharecroppers demand an explanation. In this section, we argue that fairness concerns can explain the observed patterns. The sharecropping treatment has a framing effect on subjects, in particular on those who are real-life sharecroppers. This framing reminds the subjects of the unfair profit sharing they are exposed to in real life. In Amhara, it also reminds them of the unfair distribution of land. Both effects reduce their motivation, so that we see a significant behavioral difference between sharecroppers and non-sharecroppers under the sharecropping treatment (but not under the other treatments), and this effect is especially pronounced in Amhara. To substantiate this view, we analyze the qualitative interviews that we conducted after the data collection, and review some historical accounts of the land reform that took place in Amhara in 1997.

Feedback from Interviews. We draw on qualitative interviews which were part of the survey data collected after the experiment. The enumerators asked the subjects the following two questions: What are the advantages and disadvantages of the different land arrangements in general? What are the reasons that you chose your land arrangements? After each interview, the enumerators summarized the main statements in two to three sentences per interview. This is the qualitative data that we use.

The peasants named the following motives for their contract choices in the land market: risk sharing, excess labor for the given land holdings, no real choice between fixed rent and sharecropping contracts (landlords impose sharecropping on tenants), and a land shortage due to an unequal land distribution. A number of peasants complained about the lack of cost sharing between landlord and tenant.[16] A typical farmer statement is as follows:

> *Sharecropping is the better arrangement for the landowner. He gets half of the output without contributing work or other inputs to the crop production process.* (A Farmer from the Kebele Addisena Guilt)

We sorted these motives into a matrix with contract treatment in one dimension and peasant characteristics in the other (gender, age, literate, region, renting-in plots, sharecropping-in plots, plough-positiveness of planted crops, and participation in government programs). We find that the answers are fairly equally distributed across all cells, with two exceptions. First, sharecroppers refer more commonly to land shortage as a reason for going into the land market. Second, the peasants in Amhara more commonly refer to a land shortage due to the land redistribution from 1997 (interestingly, these answers are most

[16] This point is also made in Deininger et al. (2011), who find that sharecropped plots yield 16 to 25 percent less output than owned plots cultivated by the same households. The inefficiency disappears for sharecropping contracts where the input costs are shared. This, however, is very uncommon in Ethiopia (in the sample of Deininger et al., only 12.1 percent of all sharecropping contracts have a cost sharing component).

densely clustered among those sharecroppers who were randomly assigned to the sharecropping treatment). We next review the historical accounts of this land reform, and then return to the interviews.

The 1997 Amhara Land Reform. In 1997 a land redistribution took place in the southern half of Amhara.[17] Its official intent was to achieve a more equal land distribution and to punish those individuals who had held public office during the preceding communist regime (the Därg). This group of people was labeled "birokrasi" and usually consisted of respected and relatively educated individuals. The discrimination was initiated by the new political regime and was not due to demands from below. Initially, people believed that the land reform would lead to a more equal distribution of land based on household size. However, the execution of the reform led to a quite different result.

In the first phase of the land reform, households and land holdings were registered. In the second phase, land was measured, confiscated, and redistributed. The first phase was carried out in secret and without public debate. There was significant political pressure to exaggerate the size and quality of the birokrasis' land holdings. The absence of any appeal system additionally undermined the legitimacy of the land reform. There were plenty of opportunities to game the system, for example, by registering several household members as belonging to different households. Overall, the registration was "quick, but it was neither transparent, nor democratic, and it produced some most arbitrary results" (Ege 2002, p. 80).

In the second phase, officials measured the land and thereby became accessible to peasants. It therefore was possible to change the outcome of the land reform by secret arrangements with officials (a record that entitles one to bigger land holdings in exchange for money, food, and devoted behavior). In particular, the birokrasi had to enter into secret negotiations or else they would lose a large part of their wealth. Since the discrimination against them had no support from below, officials were quite open to these negotiations. Kinship and friendship had a significant influence on the outcomes of these negotiations. Some birokrasi were quite successful and got some extra land for a child even if it was known that this child did not exist. Ege (2002, p. 83) writes that "[there] is even reason to believe that in such a chaotic situation as the 1997 land redistribution, the unscrupulous birokrasi fared better than the honest ones, certainly a design weakness of the reform on its own terms." Peasants reported "secret deals, corruption, misdeeds against individual households, and not the least, land grabbing by the distributors and their friends" (Ege 2002, p. 84).

The land reform produced winners (some of them quite unexpected, given the intended discrimination) and some great losers who ended up with almost no land. The predominant view of the land reform, even among those who supported it, is that it was quite unfair. Ege (2002, p. 71) summarizes the reform as follows: "[...] it was a very dramatic redistribution that ruined many households, uprooted the existing land tenure system without replacing it with

[17] The content of this section is based on Ege (1997, 2002).

a well defined new system, and created a new state–peasant relationship."

The land reform was mentioned in a number of interviews in Amhara, especially by real-life sharecroppers. In particular, those who now have land and offer it on the land market are accused of corrupt behavior. The following quotes express this view:

> *Those who are renting-in and sharecropping-in approach (corrupt) the local officials and try to secure certificates for these parcels which do not belong to them.* (A Farmer from the Kebele Kimbaba)

> *Through sharecropping land arrangement the owner of the land will be the one who [is] benefited. Often [the] land owner benefits from corrupt officials during land redistribution.* (A Farmer from the Kebele Kebi)

> *Today those farmers sharecrop who did not pay officials during land redistribution.* (A Farmer from the Kebele Sebat Amit)

5 Conclusions

In this paper, we provided experimental evidence for Marshallian inefficiency. In a representative sample of real-life tenants, sharecropping output was about 11 percent smaller than the output under fixed rent contracts. However, we also found that the output gap between contracts with full and partial marginal returns cannot be attributed to incentives alone. Real-life sharecroppers produced significantly less than non-sharecroppers: in Amhara, where a controversial land reform took place, real-life sharecroppers even produced 10 percent less under the sharecropping treatment than under the fixed wage treatment. We therefore conjecture that the perceived fairness of profit sharing and the historical context matter for the contractual performance of sharecropping.

Of course, we cannot conclude from our experimental data that fairness concerns play an equally important role in real-life contract arrangements. However, we consider it quite likely that they affect contractual performance. By now, a number of field experiments have shown that social preferences matter for performance. Bandiera et al. (2005) use data from a large farm in the UK to compare the performance of relative incentive and piece rate contracts. They find that productivity under piece rates is 60 percent larger than under relative incentives where a worker's effort exerts a negative externality on that worker's peers' wages. Blanes-i-Vidal and Nossol (2011) show that letting workers know their relative position in the pay and productivity distribution at a large retail company (without introducing any additional incentives) increases productivity by 6.6 percent.[18] Cohn et al. (forthcoming) find that workers who initially feel

[18] Two other papers show that the framing of incentive contracts can influence productivity. Englmaier et al. (2012) experimentally vary the salience of incentives at a large agricultural producer and find a significant 4 percent increase in output. Hossain and List (2012) framed

underpaid reciprocate wage increase with more effort. They attribute this finding to the perceived fairness of wages playing an important role in determining effort. We therefore believe that fairness concerns matter a lot for a contractual arrangement in which the landlord's payoff is tied to the tenant's effort.

We tried to back up our result on the regional variation with evidence from observational data. One could compare input intensities for farmers who sharecrop and own plots, following the standard fixed-effects approach. Our results suggest that the extent of Marshallian inefficiency is higher in Amhara than in Oromia. To confirm this, we would need a data set on agricultural households in Amhara and Oromia that would contain data on agricultural inputs disaggregated by plot and contractual arrangement (to indicate whether a particular plot was sharecropped or rented in the land market). Unfortunately, we were not able to find such a data set, despite the availability of household survey data for Ethiopia.[19] We hope that future data collection efforts will make it possible to test whether there is evidence for the context dependence of incentives in survey data.

Improving access to land through land markets is widely held to be a key to fighting extreme poverty in agrarian economies in Sub-Saharan Africa and elsewhere. Land markets are believed to help transfer land to the most efficient producers and support the occupational transformation (The World Bank 2008). We think that our findings have some policy relevance for two reasons. First, efficiency gains from contracting land and labor in markets may be smaller than expected if land arrangements between the contracting parties are politically, socially, and historically charged in a way that affects productivity through fairness concerns. Second, the relation between farm size and productivity has been controversial for decades. Proponents of land-rich farms argue that there are increasing returns to scale in farm size (due to lumpy inputs and better access to credit), while opponents argue that hiring labor or sharecropping-out land is less productive than family labor (receiving full marginal returns). If fairness concerns matter and big farms are the result of a redistribution process that is perceived as unfair, big farms may lose productivity vis à vis small farms.

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contingent incentives in a Chinese factory either as "gains" or "losses". This framing manipulation led to a 1 percent increase in team productivity.

[19] The Agricultural Sample Surveys and the Ethiopia Rural Socioeconomic Survey of 2011 have data on agricultural inputs by plot for households in Amhara and Oromia, but they do not distinguish between plots that are rented and those that are sharecropped in the land market. The Ethiopian Rural Household Surveys of 1989, 1994/95, 1997, 1999, 2004, and 2009 distinguish between rented plots and sharecropped plots for households in Amhara and Oromia, but they do not collect data on agricultural (labor) inputs by plot.

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A Variable definitions

Outcome variable. The outcome variable of interest is the real-effort output (OUTPUT) in the experiment. This variable is measured in grams and comes from weighing the participant's sorted beans on a scale after the experimental real-effort task.

Treatment variables. The treatment variables of interest are the different contracts commonly found in Ethiopian agriculture. These are binary indicators: the first one is equal to one if that contract was the fixed-wage contract and zero otherwise; the next one is equal to one if that contract was the sharecropping contract and zero otherwise; the next one is equal to one if that contract was the fixed-rent contract and zero otherwise; and the last one is equal to one if that contract was the ownership contract and zero otherwise. The contracts were randomly assigned to the different sessions.

Controls A. Controls A include a number of subject characteristics such as a binary indicator equal to one if the head of the household is female and zero otherwise (GENDER), the age of the head (AGE), the age of the head squared (AGESQ), whether the household is a member of a socio-political organization such as a cooperative (MEMBER), a binary indicator equal to one if the head is literate and zero otherwise (LITERATE), and whether the participant belongs to an ethnic minority (ETHNIC).

Controls B. Controls B include a count of the household members in the age group 0 to 5, (MEMB 0–5), in the age group 6 to 11, (MEMB 6–11), in the age group 12 to 17, (MEMB 12–17), in the age group 18 to 64, (MEMB 18–64), and over 64 years of age (MEMB OVER 64).

Controls C. Controls C include a number of agricultural production characteristics such as whether the household applied fertilizer (FERTOWN), employed wage workers (WAGEOWN), used a hoe (HOEOWN), or used a saddle (SADOWN) on their own plots. Furthermore, it includes a number of agricultural production characteristics such as whether the household applied fertilizer (FERTOTH), employed wage workers (WAGEOTH), used a hoe (HOEOTH), or a saddle (SADOTH) on plots they were renting or sharecropping. Finally, they include controls for the most common crops planted, such as white teff (WTEF), other teff (OTEF), barley (BARL), wheat (WHEAT), maize (MAIZE), millet (MILLET), and beans (BEANS), all of which are binary indicators.

Controls D. Controls D include variables on whether households benefited from government programs, such as the sustainable land management program (SLM), the infrastructure program (INFRASTRUCTURE), and the savings and credit program (SAVINGS), and whether the household belonged to a self-help group (SELFHELP), all of which are binary indicators.

Other variables. We have two other variables: a binary indicator equal to one if the head of the household was a real-life sharecropper and zero otherwise (SHARE), and a binary indicator equal to one if the head of the household was living in the region of Amhara and zero otherwise (AMHARA).

Table 1: Results on Marshallian inefficiency from observational and experimental data in Ethiopia

Paper	Data	Methodology	Specification treatment dummy	Point estimate	Unit
Pender and Fafchamps (2006)	Cross-section N =127	Household fixed-effects	Sharecropping=1, Ownership=0	-0.008	Total labor hours per Ha (in logs)
			Fixed rent=1, Ownership=0,	0.006	Total labor hours per Ha (in logs)
		Instrumental variables	Sharecropping=1, Ownership=0	-0.350*	Total labor hours per Ha (in logs)
			Fixed rent=1, Ownership=0	-0.189	Total labor hours per Ha (in logs)
Deininger et al. (2008)	3-wave panel (1999, 2001, 2004) N =4268	Household fixed-effects	Ownership=1, Sharecropping=0	0.171***	Family labor days per Ha (in logs)
			Ownership=1, Sharecropping=0	0.163***	Total labor days per Ha (in logs)
Deininger et al. (2011)	Cross-section (2007 add-on to to Deininger et al. 2008) N =2487	Pair fixed-effects	Sharecropping=1, Ownership=0	-0.156***	Male adults labor days per Ha (in logs)
			Fixed rent=1, Ownership=0	0.011	Male adults labor days per Ha (in logs)
This study	Cross-section N =472	Randomly assigned treatments	Sharecropping=1, Ownership=0	-0.082**	Real-effort output in grams (in logs)
			Fixed rent=1, Ownership=0	0.027	Real-effort output in grams (in logs)

Notes: Results from "This study" are from an ordinary least squares (OLS) regression including the full set of control variables decribed in the Appendix. The dependent variable is the log real-effort output (OUTPUT) during the experiment (measured in grams). The treatment variables are wage contract (C1), sharecropping contract (C2), and fixed rent contract (C3), all binary indicators equal to one if that contract treatment was randomly assigned to the session and zero otherwise. The ownership treatment (C4) is the reference contract. Standard errors are clustered at the session level. Significance levels at 90(*), 95(**), 99(***) percent confidence.

Table 2: Contract treatments and real-effort output

	(1) Number of sessions	(2) Number of participants	(3) Sample means (s.d.) of output	(4) Average payoff
Fixed wage	7	130	134.52 (28.38)	20.00 (0.00)
Sharecropping	6	115	131.36 (22.99)	26.30 (4.64)
Fixed rent	6	113	147.67 (23.84)	39.07 (9.53)
Ownership	6	114	144.48 (25.33)	57.79 (10.13)
Total	25	472	139.31 (26.13)	35.23 (16.18)

Notes: Descriptive statistics on number of sessions, participants, real-effort output and average payoff of subjects for the different contract treatments in the experiment. Each session had 20 participants at maximum. Real-effort output is measured in grams. Payoffs were made in Ethiopian Birr.

Table 3: Contract treatments and real-effort output (mean differences)

	Fixed-wage	Sharecropping	Fixed-rent	Ownership
Fixed-wage	-	$H_0 : x_{fw} - x_{sh} = 0$ 0.0161 (0.0262) p valuee=0.5379 N =245	$H_0 : x_{fw} - x_{fr} = 0$ -0.1036*** 0.0254 p valuee=0.0001 N =243	$H_0 : x_{fw} - x_{ow} = 0$ -0.0797*** (0.0259) p valuee=0.0023 N =244
Share-cropping	$H_0 : x_{sh} - x_{fw} = 0$ -0.0161 (0.0261) p valuee=0.5379 N =245	-	$H_0 : x_{sh} - x_{fr} = 0$ -0.1197*** (0.0228) p valuee=0.0000 N =228	$H_0 : x_{sh} - x_{ow} = 0$ -0.0959*** (0.0234) p valuee=0.0001 N =229
Fixed-rent	$H_0 : x_{fr} - x_{fw} = 0$ 0.1036*** (0.0254) p valuee=0.0001 N =243	$H_0 : x_{fr} - x_{sh} = 0$ 0.1197*** (0.0228) p valuee=0.0000 N =228	-	$H_0 : x_{fr} - x_{ow} = 0$ 0.0238 (0.0224) p valuee=0.2889 N =227
Owner-ship	$H_0 : x_{ow} - x_{fw} = 0$ 0.0797*** (0.0259) p valuee=0.0023 N =244	$H_0 : x_{ow} - x_{sh} = 0$ 0.0959*** (0.0234) p valuee=0.0001 N =229	$H_0 : x_{ow} - x_{fr} = 0$ -0.0238 (0.0224) p valuee=0.2889 N =227	-

Notes: Two-sample t-tests with equal variances to test for mean differences in log real-effort output between all possible combinations of the treatment variables. From top to bottom cells contain the tested null hypothesis, mean differences in log real-effort output in percent, standard deviations in parentheses, p valuees and number of observations in the combined sample. Significance level at 90(*), 95(**), 99(***) percent confidence.

Table 4: Contract treatments and real-effort output

	(1) OUTPUT	(2) OUTPUT	(3) OUTPUT	(4) OUTPUT
Fixed-wage	0.0139	0.0134	0.0054	0.0061
	(0.0556)	(0.0555)	(0.0518)	(0.0514)
Fixed-rent	0.1184**	0.1187**	0.1095**	0.1088**
	(0.0549)	(0.0545)	(0.0505)	(0.0503)
Ownership	0.0867*	0.0848*	0.0820*	0.0816**
	(0.0444)	(0.0455)	(0.0406)	(0.0393)
Controls A	yes	yes	yes	yes
Controls B	no	yes	yes	yes
Controls C	no	no	yes	yes
Controls D	no	no	no	yes
Mean (s.d.) dependend	4.92	4.92	4.92	4.92
variable	(0.1944)	(0.1944)	(0.1944)	(0.1944)
Observations	469	469	469	469
R-squared	0.1876	0.1904	0.2226	0.2247

Notes: Ordinary least squares (OLS) regressions with cluster-robust standard errors in parentheses. LHS variable is log real-effort output (OUTPUT) during the experiment (measured in grams). Treatment variables are fixed-wage contract, fixed-rent contract and ownership contract, all binary indicators equal to one if the respective contract treatment was randomly assigned to a session and zero otherwise. The sharecropping contract is the reference contract. Significance level at 90(*), 95(**), 99(***) percent confidence. Controls A include participants' characteristics, controls B include participants' households characteristics, controls C include agricultural production characteristics, controls D include indicators on participants' participation in government programs (see appendix for a full description of variables). Differences between maximum sample size n=472 and observations are due to missing values.

Table 5: Comparing real-effort output for real-life sharecroppers and non-sharecroppers

	(1) OUTPUT Fixed-wage	(2) OUTPUT Sharecropping	(3) OUTPUT Fixed-rent	(4) OUTPUT Ownership
SHARE	0.0042	-0.0829***	0.0657	-0.0011
	(0.0684)	(0.0179)	(0.0420)	(0.0305)
Mean (s.d.) dependend	4.88	4.81	5.00	4.96
variable for SHARE=1	(0.1751)	(0.1867)	(0.1647)	(0.1610)
Observations for SHARE=1	62	51	48	61
Mean (s.d.) dependend	4.87	4.90	4.95	4.96
variable for SHARE=0	(0.2609)	(0.1684)	(0.1579)	(0.1907)
Observations for SHARE=0	68	64	65	52
R-squared	0.0001	0.0524	0.0398	0.0000

Notes: Ordinary least squares (OLS) regressions with cluster-robust standard errors in parentheses. LHS variable is log real-effort output (OUTPUT) during the experiment (measured in grams) for each treatment subsample. Treatment variables are fixed-wage contract, sharecropping contract and fixed-rent contract and ownership contract. SHARE is a binary variable equal to one if participant is a real-life sharecropper. Significance level at 90(*), 95(**), 99(***) percent confidence. The lower panel contains descriptive statistics for each treatment sample for respectively real-life sharecroppers (SHARE=1) and non real-life sharecroppers (SHARE=0).

Table 6: Contract treatments and real-effort output of sharecroppers

	(1) OUTPUT	(2) OUTPUT	(3) OUTPUT	(4) OUTPUT
Sharecropping	0.0593	0.0607	0.0635	0.0649
	(0.0628)	(0.0628)	(0.0607)	(0.0610)
Fixed-rent	0.1112	0.1122	0.1120*	0.1126*
	(0.0656)	(0.0664)	(0.0635)	(0.0634)
Ownership	0.0795	0.0774	0.0807	0.0801
	(0.0595)	(0.0608)	(0.0544)	(0.0544)
SHARE	0.0088	0.0107	0.0048	0.0032
	(0.0243)	(0.0251)	(0.0231)	(0.0235)
AMHARA	-0.0757	-0.0760	-0.0848	-0.0854
	(0.0672)	(0.0681)	(0.0810)	(0.0820)
Sharecropping* SHARE	-0.0692**	-0.0720**	-0.0596*	-0.0605*
	(0.0291)	(0.0311)	(0.0328)	(0.0338)
Sharecropping* AMHARA	-0.0537	-0.0515	-0.0410	-0.0429
	(0.0645)	(0.0634)	(0.0652)	(0.0666)
Sharecropping* SHARE* AMHARA	-0.0378*	-0.0423*	-0.0642**	-0.0622*
	(0.0184)	(0.0225)	(0.0279)	(0.0311)
Controls A	yes	yes	yes	yes
Controls B	no	yes	yes	yes
Controls C	no	no	yes	yes
Controls D	no	no	no	yes
Mean (s.d.) dependend variable	4.92 (0.1944)	4.92 (0.1944)	4.92 (0.1944)	4.92 (0.1944)
Observations	469	469	469	469
R-squared	0.2168	0.2205	0.2532	0.2549

Notes: Ordinary least squares (OLS) regressions with cluster-robust standard errors in parentheses. LHS variable is log real-effort output (OUTPUT) during the experiment (measured in grams). The fixed-wage contract is the reference contract. Significance level at 90(*), 95(**), 99(***) percent confidence. See the appendix for a full description of variables. Differences between maximum sample size n=472 and observations are due to missing values.

Table 7: Contract treatments and real-effort output of sharecroppers (only Amhara)

	(1) OUTPUT	(2) OUTPUT	(3) OUTPUT	(4) OUTPUT
Sharecropping	-0.0118	-0.0115	0.0011	-0.0063
	(0.0857)	(0.0823)	(0.0717)	(0.0758)
Fixed-rent	0.0862	0.0872	0.0863	0.0830
	(0.0981)	(0.0977)	(0.0939)	(0.0982)
Ownership	0.0754	0.0758	0.0688	0.0636
	(0.0791)	(0.0780)	(0.0627)	(0.0664)
SHARE	0.0034	0.0040	0.0031	0.0025
	(0.0284)	(0.0295)	(0.0244)	(0.0258)
Sharecropping*	-0.0815**	-0.0822**	-0.1060**	-0.0969**
SHARE	(0.0302)	(0.0313)	(0.0387)	(0.0413)
Controls A	yes	yes	yes	yes
Controls B	no	yes	yes	yes
Controls C	no	no	yes	yes
Controls D	no	no	no	yes
Mean (s.d.) dependend	4.92	4.92	4.92	4.92
variable	(0.1944)	(0.1944)	(0.1944)	(0.1944)
Observations	280	280	280	280
R-squared	0.2833	0.2873	0.3330	0.3363

Notes: Ordinary least squares (OLS) regressions with cluster-robust standard errors in parentheses. LHS variable is log real-effort output (OUTPUT) during the experiment (measured in grams). The fixed-wage contract is the reference contract. Significance level at 90(*), 95(**), 99(***) percent confidence. See the appendix for a full description of variables. Differences between maximum sample size n=472 and observations are due to missing values.