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The Effects of Land Markets on Resource Allocation and Agricultural Productivity

By Chaoran Chen, Diego Restuccia and Raul Santaeulalia-Llopis

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Chaoran Chen
York University

Diego Restuccia
University of Toronto and NBER

Raül Santaeulàlia-Llopis
MOVE-UAB and Barcelona GSE

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Abstract

We assess the effects of land markets on misallocation and productivity by exploiting policy-driven variation in land rentals across time and space arising from a large-scale land certification reform in Ethiopia, where land remains owned by the state. Our main finding from detailed micro panel data is that land rentals substantially reduce misallocation and increase agricultural productivity. Our evidence builds from an empirical difference-in-difference strategy, an instrumental variable approach, and a calibrated quantitative macroeconomic framework with heterogeneous household-farms that replicates—without targeting—the empirical effects. These effects are nonlinear—impacting more farms farther away from efficient operational scale, consistent with our theory. Using our model, we find that more active land markets reduce inequality, an important concern for the design of land policy. We also find that the positive effects of land markets are mainly driven by formal market rentals as opposed to informal rentals. Finally, our analysis also provides evidence that land markets increase the adoption of more advanced technologies such as the use of fertilizers.

Keywords: Land markets, rentals, effects, misallocation, productivity, inequality, micro data, quantitative macro, informal markets, technology, fertilizers.
JEL classification: E02, O10, O11, O13, O43, O55, Q10, Q15, Q18, Q24, D5.

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1 Introduction

What are the effects of land markets on resource allocation and agricultural productivity? This question is important for many poor countries in which land transactions are either prohibited by law or face high transaction costs (Binswanger and Rosenzweig, 1986; Rosenzweig and Binswanger, 1993). The advocates of these prohibitions base their arguments on the lack of evidence in favor of the efficient use of resources generated from land markets and on the notion that common or customary tenure—as opposed to the private titling of land—keeps inequality and landlessness in check. Indeed, many governments and institutions justify "against-market" land policy on these grounds in poor countries.[1] Unfortunately, despite its importance and the large efforts devoted into understanding it, the answer to whether land markets improve resource allocation and productivity as well as its implications for equity remains poorly understood. We address these questions assessing both empirically and quantitatively the effects of land markets in the context of a large-scale land certification reform in Ethiopia that partially lifts restrictions on land rental transactions but it does not allow for land purchases or sales as the ownership continues to reside with the state.

Ethiopia provides a unique and relevant context to investigate the effects of land markets on productivity. From 1974 until the early-1990s, the Communist government in power expropriated and uniformly redistributed all of the rural land in the country, and prohibited land transactions by law. Although land ownership still resides with the state and many of the restrictions to land transactions remain in place, a land certification reform was launched

[1]There, the ownership of land typically resides with the collective or the state and use rights of land are distributed by local leaders on a fairly egalitarian basis. However, although long-lived land-use rights can help improve land tenure security and generate investment (Besley, 1995), they do not necessarily entail the right to sell or rent, which prevents land transactions and reallocations (Galiani and Schargrodsky, 2011).

in the 2000s to grant land certificates to farmers that allow land to be reallocated across farmers via rentals (up to a limit) of the use rights (Holden and Ghebru, 2016).

A key aspect of this land reform is that the granting of land certificates—and, hence, the lifting of barriers to land rental market activity—was decentralized and implemented by local governments with different intensity and timing across zones (i.e., sub-regions) as opposed to being contemporaneously implemented by the central government (Deininger et al., 2008). Indeed, using representative panel data that catches the reform in the 2010s—in three waves 2011/12, 2013/14, and 2015/2016—we find large variation in land rental market activity across space and time in both the intensive and extensive margins. Precisely, while some zones are in advanced stages of the land reform with a sizeable share of rented land in cultivation throughout our sample (i.e., early reformers), there are other zones for which we do not find much land market activity except for the last periods (i.e, late reformers) or not at all (i.e., unreformed). This layout provides us with a unique policy-driven variation in land rentals across time and space that we exploit in our analysis.[2]

Our assessment of the effects of land markets builds on both empirical evidence and quantitative macroeconomic theory. First, we use an empirical difference-in-difference approach to assess the effects of increasing reform intensity on resource allocation and agricultural productivity—where the treatment zones are those that show an increase in land market

[2]Since our goal is to assess the effects of land rental markets, we focus directly on the variation in land rental activity as opposed to the variation in the distribution of land certificates. In this context, notice that although the land reform was initiated in the 2000s, our representative panel data is collected in the 2010s. This is relevant as it allows us to capture potential lagging behavior between the granting of land certificates and the farms' engagement in land rental activity, for which we find evidence (see Section 2.3). A plausible explanation for this lagging behavior is the potential lack of trust in the institutional reform (Ostrom, 2010). For example, this could be driven by the fact that Ethiopian farmers have witnessed a recent past with recurrent governmental land expropriations (Gottlieb and Grobovšek, 2018). We discuss this further in Section 2.

activity and the control zones those that do not. Our main finding is that land rentals reduce misallocation and increase agricultural productivity. An increase in one percentage point of land rentals increases agricultural productivity by a significant 3.2 percent. We assess the validation of these effects by showing nonsignificant pre-trends between treatment and control zones and nonsignificant placebo tests. We also explore an analogous quantile difference-in-difference specification where quantiles group farmers with similar distance between actual factor inputs and optimal operational scale and find that the effects of land markets are nonlinear—impacting more those farms farther away from their efficient operational scale. Further, an advantage of studying a large-scale land reform with variation in not only reform intensity but also in the reform timing across zones is that it allows us to conduct a set of alternative difference-in-difference strategies to additionally assess the effects of land markets from different directions. In particular, we study the effects of reform adoption—the extensive margin of land rentals—with two additional specifications in which we use late reformers as treatment zones. These two specifications differ in that the control zones are defined either as the unreformed or as the early reformers. In both cases we reach similar insights to those derived from the study of reform intensity: land rentals reduce factor misallocation and increase agricultural productivity.

Second, it is important to acknowledge that the empirical use of policy as an exogenous source of variation is not free of caveats. In particular, a valid concern regarding our difference-in-difference analysis is the potential endogeneity of the policy implementation itself. For example, some local governments might be willing (or bounded) to implement and enforce the land reform more strongly than others, which can potentially yield endogenous differences in land rentals. In this context, our results from the pre-trend analysis suggest that there is no pre-existent factor—including the type of local government or institutions—behind the rise in land rentals. Further, we directly study the relationship between each zone and political leadership. The idea is that the local authorities can decide to implement the reform earlier or more intensively in zones that are closer to the ruling power through, perhaps, higher state capacity. However, proxying the closeness to the ruling power with the distance to the country's capital, the hometown of the country's president, and the hometown of the prime minister, we find nonsignificant differences between treatment and control zones. We further address the endogeneity concern by using religion and lagged rentals as instruments. We find that religion and land rentals are highly correlated with a significantly larger proportion of farmers of christian orthodox faith (47.9 percent) that engage in land rentals than muslim farmers (21.6 percent) which provides evidence of a strong first stage.[3] Further, notice that the institutional context is such that resource reallocations (i.e., land transactions) occur only through land rentals. That is, the only channel by which religion can affect the reallocation of resources is through rentals which satisfies the exclusionary restriction. This is consistent with a theoretical framework in which resource allocations are affected by land rentals solely through an institutional cost that determines the access to land markets; and notice that religion can be interpreted as such institutional (or cultural) cost. In line with our difference-in-difference results, we find that land rentals—either instrumented as religion or lagged rentals—reduce misallocation and increase agricultural productivity.

Third, we consider a quantitative macroeconomic theory with households-farms that are

[3] Religion comprises a number of faiths in Ethiopia. Precisely, we find that 42.3 percent of households are Chrisitian orthodox, 31.2 percent muslim, 23.4 percent protestants, and the rest declare none or other religions.

heterogeneous in their permanent productivity and face zone-specific institutional barriers to accessing land rental markets. These institutional barriers are summarized by a cost parameter, $\chi_z \in [0, \infty)$. We calibrate these institutional costs by zone in order to match the *status quo* land rental market activity separately for each zone. We are particularly interested in assessing whether our calibrated model is quantitatively able to generate the estimated empirical effects of land markets. To do so, we conduct a policy experiment on the *status quo* allocations through a land reform that we formalize as an exogenous and unexpected reduction in the institutional costs. Specifically, we change χ_z in order to match the actual changes in land rental activity that we observe in the data over time and by zone. The policy experiment generates a new set of *counterfactual* allocations in which the land rental activity in the model matches the actual land rentals after the reform. Then, we use the model-generated *status quo* and *counterfactual* allocations to estimate the effects of land rental markets on resource misallocation and agricultural productivity using the same difference-in-difference specification as the empirical counterpart. The main result from our policy experiment is that the model-generated effects are very similar to the empirical effects. Precisely, the model implies that a one percentage point increase in land rentals increases agricultural productivity by 2.4 percent, which explains approximately three fourths of the empirical effects. It is important to highlight that our calibration strategy targets the change in land rentals but not the empirical effects. Also, notice that the model-generated effects of land rental markets are entirely driven by the policy experiment—a change in χ_z. That is, unlike the empirical assessment, the model-based assessment results from a controlled experiment that exogenously changes the institutional barriers, χ_z. Therefore, the model-based analysis is not tainted by the potential endogeneity of the policy or any other concerns behind the changes in allocative efficiency that follow the land rental reform.

An important implication of land markets—beyond their effects on resource allocation and productivity—is whether they enhance or reduce farm income inequality. This is a critical aspect of the political discourse on land market policy in poor countries (Deininger and Binswanger, 1999; Deininger and Feder, 2001). But a complete assessment of the effects of land rental markets on inequality is challenging as it requires data that is typically not available, in particular it requires the record of both land rental payments and receipts as well as consistency in that the sum of land rents paid be identical to the sum of land rents received. This is unlikely to be the case even for administrative data sets. Fortunately, although our data only contains information on land rental paid, our model structure contains both land rental payments and receipts which clear in general equilibrium. Thus, we use our model-generated *status quo* and *counterfactual* allocations to construct measures of within-zone inequality in farming income. Then, we conduct a difference-in-difference estimation—analogous to our benchmark analysis on resource allocation and productivity—in order to assess the effects of land rental markets on inequality. We find that an increase in land rental market activity substantially reduces zone-level inequality, for a wide range of inequality measures. That is, the efficiency gains generated from land rental markets do not arise with the typical cost of a loss in equity.

Although the reduction in misallocation due to land rentals is economically substantial, not all rentals necessarily imply larger efficiency. In particular, we find a strong relationship between personal connections and rental transactions with more than eighty per cent of land rentals occurring through relatives and friends. Plausibly, these type of rentals aim at goals other than efficiency such as redistribution or the provision of social insurance which can be related to household proximity in kin (Kinnan and Townsend, 2012) or social stratification (Munshi and Rosenzweig, 2016).[4] In this context, we define informal land rentals as those that are stipulated to be for free (zero rental payments) in the rental contract and formal rentals as those for which the rental contract stipulates a non-zero rental payment.[5] Notice that our definition of formal rentals includes sharecropping contracts (Shaban, 1987; Sadoulet et al., 1997; Burchardi et al., 2018). We find that the effects of formal rentals on resource misallocation and agricultural productivity are approximately twenty-five percent larger than our benchmark average results. In contrast, the effects of informal rentals are smaller and not significant. That is, the effects on resource misallocation and agricultural productivity are generated by market forces and not by informal rentals.

We also explore whether land rental markets affect technology adoption and find mixed results. On the extensive margin—with a probit analysis—we find that land rental markets are cross-sectionally associated with higher use of fertilizers, livestock, and agricultural equipment (e.g., tractors). On the intensive margin—with a difference-in-difference analysis analogous to our benchmark empirical strategy—we find that the causal effects differ by item. We find that an increase in land rental markets generates a significant increase in fertilizer use, but it does not affect livestock or agricultural equipment in a significant manner. Plausible explanations for this nonsignificant result on larger investments are the fact that most rentals are restricted to short-term (a year) contractual arrangements (Goldstein and Udry, 2008) and that the size of farms remains small in Ethiopia (less than an hectare, on

[4] The seminal papers by Rosenzweig and Stark (1989), Townsend (1994) and Udry (1994) highlight the role of insurance in poor countries. See also the more recent studies of Morten (2019) and Kinnan (2019).

[5] A rental contract between renter and rentier is basically defined by the arranged rental price for a given plot and period of time (typically, for a single rainy season). This contractual arrangement is stipulated before cultivation and, therefore, before harvest.

average) which limits the payoffs of large investments (e.g., tractors) that are perhaps more profitable with large operational scales (e.g., Chen, 2019).

Our paper is related to a growing macroeconomic development literature on agricultural productivity.[6] The importance of agricultural productivity in accounting for income per capita differences across countries has been emphasized, among many others, in Gollin et al. (2002), Restuccia et al. (2008) and Gollin et al. (2014).[7] The measurement of the extent of misallocation in poor countries has been recently emphasized using micro panel data in Restuccia and Santaeulàlia-Llopis (2017) and Gollin and Udry (2017). In sharp contrast with these authors, our interest is on the changes in the extent of misallocation due to the effects of land markets—and not on the extent of misallocation *per se*. If the introduction of land markets generates positive effects on resource allocation and productivity, then we have identified the lack of land markets as one source—among potentially many others—of factor misallocation (Restuccia and Rogerson, 2017). In this context, our paper also relates to the microeconomic development literature studying the role of institutions that impede economic development.[8] We specifically focus on barriers to accessing land markets and how this microeconomic distortion matters for macroeconomic agricultural productivity. To investigate this question, we use a specific large-scale land policy reform that generates variation across space and time in land market activity. This approach of exploiting policy-driven variation across time and space follows an important existing literature in growth and development economics (Besley and Burgess, 2000; Banerjee and Iyer, 2005; Giné, 2005;

[6]For example, see Adamopoulos (2011), Lagakos and Waugh (2013), Donovan (2016), Chen (2017), and Adamopoulos et al. (2017).

[7]See also Gollin et al. (2004, 2007). More recently, Adamopoulos and Restuccia (2014) discusses the role of factor misallocation in explaining cross-country differences in agricultural productivity.

[8]See Acemoglu et al. (2001), Banerjee et al. (2002), and Banerjee and Iyer (2005), among others.

de Janvry et al., 2015). While land reforms have been studied in a recent empirical literature (Deininger et al., 2008, 2011; Chari et al., 2017), our empirical contribution has the advantage of hindsight using a rich policy layout that combines the intensive and extensive margins of a land reform—e.g., early reformers, late reformers and unreformed areas. Further, our paper integrates empirical strategies and quantitative macroeconomic theory which provides a comprehensive aggregate assessment of the policy-driven land reallocation effects triggered at the micro level. Furthermore, we use the estimated empirical effects to externally validate the theoretical effects—a methodology that resembles the work in Todd and Wolpin (2006). This validation exercise warrants our study of the implications of land markes for inequality through model counterfactuals. In this context, our work closely relates to a recent strand of the development literature that combines empirical approaches and theory such as Mobarak and Rosenzweig (2014), Bryan et al. (2014), Lagakos et al. (2018), and Meghir et al. (2019).

The paper proceeds as follows. Section 2 describes the data, the institutional background and the land reform and market activity in Ethiopia. In Section 3, we present our theoretical framework, define the equilibrium and qualitatively discuss the theoretical effects of land markets. Section 4 shows our empirical results about the effects of land rentals on factor misallocation and agricultural productivity using a difference-in-different approach (on the intensive and extensive margins of the land reform) and an IV strategy. In Section 5, we calibrate our theoretical framework and quantitatively assess the effects of land markets through a policy experiment that resembles the land reform in terms of changes in land rental market activity. We assess the effects of resource allocation, productivity and inequality. We provide further insights related to formal versus informal land rental markets and effects on technology adoption in Section 6. In Section 7, we provide robustness exercises and some extensions of our analysis. Section 8 concludes.

2 Data and Institutional Background

2.1 Data

We use household-level panel data from the World Bank, the Ethiopia Integrated Survey of Agriculture (ISA), for all available waves 2011/12, 2013/14, and 2015/16. The ISA's provide information over the entire process of crop production, including physical measures of farm inputs and outputs. These are representative surveys of the population, with approximately 5,250 households interviewed per wave, among whom around two thirds live in rural areas and participate in agricultural production. Each household is surveyed twice in a year: the first round is during the planting season, and the second round is during the harvest season.

Almost all farms in Ethiopia are family farms. Therefore, we treat a family farm operated by a household as our basic unit of production. We construct our measures of factor inputs, outputs, and total factor productivity (TFP) at the household-farm level. A farm operated by a household typically consists of several different plots of land; we therefore aggregate the inputs and outputs of these plots to the household level. A detailed description of our variables for output, capital, land quality, land, and labor input, as well as transitory shocks such as rain, is in Appendix A.

The panel dimension of the Ethiopia ISA data is key in two aspects of our analysis. First, we use the panel dimension of our survey data to compute a permanent component of individual farm TFP. This permanent component—or fixed effect—captures unobserved heterogeneity in productivity. We use this benchmark productivity to conduct our reallocation exercises. Second, we use the policy-driven variation across time and space of land rentals in Ethiopia to provide direct evidence of the effects of rentals on aggregate productivity with a difference-in-difference approach that requires the panel structure.

2.2 Institutional Background

Ethiopia is one of the poorest countries in the world. According to the World Bank's World Development Indicators, Ethiopia's real GDP per capita adjusted by purchasing power parity (PPP) is only about three percent of that of the United States. Agriculture is an important sector in Ethiopia, employing around three quarters of the work force. Despite being a poor country, Ethiopia's recent economic performance is positive, with a strong annual real GDP per capita growth rate of 7.1 percent between 2005 and 2018. As a result, Ethiopia is sometimes referred to as a miracle of economic growth in Africa.

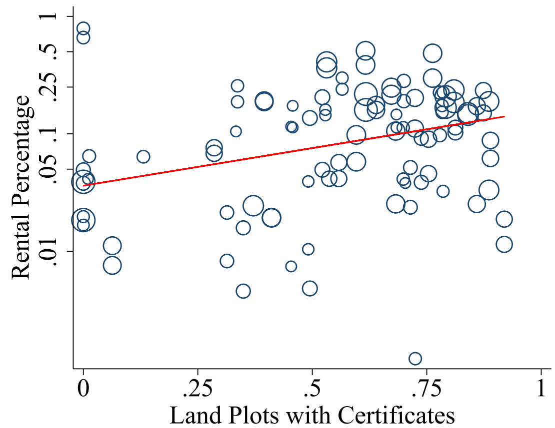
We focus the study in Ethiopia because of its historical institutional background related to land policies and its more recent land certification reform that we assess. Current land institutions in Ethiopia are shaped by historical events, but their prevailing characteristic has been state control over the allocation and use of land. The evolution of land institutions can be divided into three periods. The first period is the imperial period, spanning from the mid nineteenth century to 1974. During this period, land ownership was usually granted to political supporters regardless of occupation or use in farming, which created a feudal regime. Further emergence of private property during this period resulted in powerful landlords. The second period, from 1975 to 1991, resulted from the severe social injustices created by the feudal regime that lead to a Communist regime. A comprehensive land reform, "Land to the Tiller", was then implemented. The Communist government expropriated all of the land in the country and redistributed it to all rural households—adjusting for soil quality and family size—in the form of use rights. Land redistributions were frequent, every one to two years, to achieve an equitable allocation of use rights among the local rural populations, and land transactions were strictly prohibited.

The third period started with the collapse of the Communist regime in 1991, under a market-oriented government that has largely maintained land-related policies from the previous regime. Essentially, land ownership still resides with the state and households are assigned use rights by local authorities at the village (*kebele*) or district (*woreda*) level. Many of the restrictions to land transactions remain in place. However, land certification reforms have been implemented since the early 2000s to mainly promote tenure security by issuing land certificates of use rights. Critically, farmers with these land certificates are allowed to rent out land, but not to sell land because land is entirely owned by the state.

2.3 Land Certification Reform and Land Rental Markets

Our interest is in assessing the effects of land markets on economic development. For this reason, and since the land certification reform under study is specifically a reform that lifts restrictions to land rental markets (Holden and Ghebru, 2016), we directly focus on land rental market activity as our source of variation. In particular, we measure land rental market activity, $R_{z,t}$, as the ratio between the size of total rented land and the size of total

Figure 1: Land Certificates and Land Rentals



Note: This figure shows the percentage of land rentals within a zone increases with the percentage of land plots with certificates in the previous period. We report 100 zone-year observations that have a positive percent of rental land and more than 10 observations. The size of the circles indicate the number of observations in each zone.

cultivated land in a given zone z and period t.[9]

Notice that we focus on $R_{z,t}$ as opposed to direct measures of the granting of land certificates—e.g. the percentage of the land plots with certificates. This distinction is important because the granting of land certificates does not necessarily generate immediate land market activity, which is our object of interest. Indeed, we find that although land certificates have been already granted in all zones (at least partially), there are zones with granted land certificates where we do not observe land rental market activity at all through our entire sample period (from 2011/12 to 2015/16).[10] This suggests that certain lagging behavior between the granting of land certificates and land rental activity exists. That is, it

[9]Despite the land reform, the land rental market is relatively under-developed in Ethiopia. Indeed, severe restrictions on land rentals remain in place. For example, only a fraction of use rights can be rented and the renting household must dwell in the rural area as well as be engaged only in farming. See Holden and Ghebru (2016) for a discussion of a set of legal restrictions on land rentals present in Ethiopia since 2006.

[10]We find that the certificates in these zones where (on average) granted in 2008. That is, we find zones in which farms do not engage in land rentals in the 2010s even though land certificates were granted in those zones in the 2000s.

is plausible that it takes time for farmers—who throughout their lifetime have been subject to recurrent land expropriations by the government in Ethiopia—to trust and use the new land rental entitlements. Indeed, during the launching of the land reform, local governments still illegally evicted landholders with de-facto imprisonment threats (ELTAP, 2007), which further depletes trust.[11] Nevertheless, we find that land rentals are clearly associated with the land certificate reform. Figure 1 shows this positive association between the percentage of the land plots with certificates in the previous period and R_z—the percentage of land rentals in the current period—across zones, pooling both 2013/14 and 2015/16 waves of data. The contemporaneous correlation within each year is also positive and significant: The Spearman's rank correlations between the two are 0.44 and 0.37 for the two waves, respectively, both of which are significant at the one percent level.

The key aspect of the land market reform under study is that its implementation was decentralized to local authorities (Deininger et al., 2008), so the timing and extent of land rental market activity potentially differ across zones and time. The fact that the implementation of the policy was decentralized to local governments has implied a large degree of variation in land rentals (intensive and extensive margins) that we exploit in our empirical strategy. We show several features of the land rental market activity across space and time in Table 1. In terms of the intensive margin of land rentals, the nationwide percentage of rented land is 10.9 in 2013/14. This percentage differs greatly across space with many zones in which there is no evidence of the presence of land rental market activity whereas other zones show more than 60 percent of cultivated land is acquired through the rental market.[12]

[11] See Ostrom (2010) for a discussion on the importance of trust in the context of policy implementation.

[12] There are four levels of administrative divisions in Ethiopia: regions (states), zones (counties), woreda (districts), and kebele (wards). For the 2013/14 sample, we have farm location information down to the kebele level. We have a total of 2,877 observations, located across 10 regions, 73 zones, and 272 woredas.

Table 1: Land Rentals across Time and Space

	Percent						Obs.
	Aggregate	Min.	10 pct	Median	90 pct	Max.	
$R_{z,2013/14}$	10.9	0.0	0.0	5.2	24.7	66.0	67
$R_{z,2015/16}$	11.5	0.0	0.0	6.3	27.7	73.2	67

Notes: Data from Ethiopia ISA 2013/14 and 2015/16. The share of land rentals R_z is defined as the ratio between the size of total rented land in cultivation and the size of total cultivated land in zone z. We show distributional statistics of the level of R_z separately for our two waves of data 2013/14 and 2015/16. We drop zones with less than 10 household observations in either year.

The distribution of the proportion of rented land is substantially skewed with a median zone with percentage of rented land of 5.2 percent, a 90th percentile zone with rented land of 24.7 percent and a maximum zone with 66.0 percent of cultivated land from rentals in 2013/14. The heterogeneity in the extensive margin is also important. There are a total of 8 zones that have not experienced land rental activity in 2013/14. Finally, across time, there is a substantial and heterogeneous increase in land rental market activity between 2013/14 and 2015/16. The nationwide share of rented land in total cultivated land increases from 10.9 percent to 11.5 percent. More importantly for our analysis, rental markets have developed at differential pace across regions. In terms of the intensive margin, while 34 zones out of 67 zones did not experience an increase in land rentals, 7 zones experienced land rental increases by at least 10 percentage points, and 3 zones experienced rental increases of more than 15 percentage points. In terms of the extensive margin we find that 6 zones out of the 8 zones that had not implemented rentals in 2013/14 implement rentals in 2015/16. That is, there are zones that do not show land rental market activity throughout our sample period.

Due to sample size, we mainly focus our analysis at the zone level since we have a reasonable number of zones and a relatively large number of observations within each zone.

3 Theoretical Framework

In this Section, we propose a macroeconomic model to qualitatively and quantitatively assess the effects of land rental markets on resource misallocation and productivity within zones in Ethiopia.

3.1 Environment, Technology and Farm Problem

Our economy is populated by heterogeneous household farms that differ in their permanent farming productivity, $s_i \in S$. Each household farm produces a homogeneous agricultural good using the following decreasing returns to scale technology:

$$\widetilde{y}_{it} = (s_i \zeta_{it})^{1-\gamma} \left(k_{it}^{\alpha} \ell_{it}^{1-\alpha}\right)^{\gamma},$$

where $\widetilde{y}_{it}$ is the output of farm i in period t (measured as value added net of intermediates such as fertilizer and seeds), k_{it} is the associated capital input and ℓ_{it} is a measure of land size adjusted for land quality. Precisely, $\ell_{it} = q_{it} l_{it}$, where q_{it} is land quality and l_{it} is land size. Notice that household-farm productivity consists of a permanent component, s_i, that does not change over time and a transitory component ζ_{it} (e.g., rain shocks and illnesses). All variables are in per capita (hourly) terms.[13]

Two remarks are in order. First, we are interested in reallocations guided only by the permanent component of productivity s_i. For this reason, in practice, we use our panel data

[13]Notice that we set all variables in per capita terms. This choice follows the idea that the reallocations of capital and land that we conduct across household farms are not accompanied by the reallocation of household members across farms. The reason is that the labor input in agricultural production is largely provided within family. We conduct robustness to this assumption in Section 7.2 where we allow for the labor input to be reallocated.

to recover this permanent component (a fixed effect) and measure the transitory shocks ζ_{it} as the residual deviations from that permanent component. Second, we also use our rich data on land quality at the plot level to net its effects on output (see Section 2.1). This way, we can define our benchmark output as $y_{it} = \frac{\widetilde{y}_{it}}{\zeta_{it}^{1-\gamma} q_{it}^{(1-\alpha)\gamma}}$, that is,

$$y_{it} = s_i^{1-\gamma} \left(k_{it}^{\alpha} l_{it}^{1-\alpha} \right)^{\gamma}, \tag{1}$$

where the parameter $\gamma \in (0, 1)$ governs returns to scale at the farm level, and α is a factor share parameter.

In our calibration strategy, we use our micro data to estimate the factor shares of income with capital, labor, and land shares that pin down the values for α and γ. We find that the capital, labor, and land shares are 0.147, 0.464, and 0.389, respectively. This implies that $\alpha = 0.274$ and $\gamma = 0.536$.[14] Given values for α and γ, together with farms' actual inputs (including land quality) and outputs in the data, we recover farm-level productivity separately for each year, $s_i \zeta_{it}$, which is the product of a permanent s_i and transitory component ζ_{it}. Then we use our panel data to recover our benchmark measure of permanent farm-level productivity for the reallocation exercises, s_i, which is constructed as the geometric mean of farm-level productivity across years. That is, our benchmark productivity measure s_i is equivalent to the outcome of an estimation of household-farm fixed effects of productivity (in logs) and, hence, it captures permanent unobserved heterogeneity across farms.[15] Notice

[14] Appendix B describes in detail how we obtain these factor shares.

[15] After observing the implied distribution of productivity we trim approximately 0.8 percent of the farm TFP distribution to remove what we find are candidate outliers which may reflect measurement error in inputs and outputs in the data. A more systematic (and aggressive) trimming, say one percent of the TFP distribution on both tails, barely changes the dispersion of TFP with a standard deviation of 0.75, as opposed to our benchmark 0.79.

that although farm productivity s_i is invariant to time, farm output and factor inputs can change over time in production, see equation (1). However, for the ease of notation, we drop time subscripts for all variables in our analysis from now on.

Farm's problem. Each household farm solves the following profit maximization problem:

$$\max_{k_i, l_i} \pi(s_i, \bar{l}_i) = s_i^{1-\gamma} \left(k_i^{\alpha} l_i^{1-\alpha}\right)^{\gamma} - rk_i - c(l_i, \bar{l}_i), \tag{2}$$

where l_i and $\bar{l}_i$ denote the actual operational scale and land endowment, respectively. For example, if a farmer is endowed with one hectare of land and rents in half hectare, then we denote this farmer's initial endowment as $\bar{l}_i = 1$ and operational scale as $l = 1.5$. The function $c(l_i, \bar{l}_i)$ represents the cost of changing the amount of land endowments to the desired operational scale. This cost is a combination of the rental cost of land plus a land transaction cost that captures the institutional barriers to accessing land rental markets determined by, for example, the amount of land certificates distributed in given zone that allow for rentals. We write this cost function as

$$c(l_i, \bar{l}_i) = q(l_i - \bar{l}_i) + b(l_i, \bar{l}_i), \tag{3}$$

where q is the rental rate of land, $q(l_i - \bar{l}_i)$ the rental payment (receipt) of land if $l_i > \bar{l}_i$ ($l_i < \bar{l}_i$), and $b(l_i, \bar{l}_i)$ represents the institutional barrier to accessing land rental markets. By definition, the transaction cost is such that $b(\bar{l}_i, \bar{l}_i) = 0$ and $b(l_i, \bar{l}_i) > 0$ for all $l_i \neq \bar{l}_i$. Further, we assume that the transaction cost $b(l_i, \bar{l}_i)$ increases in the distance between l_i and

$\bar{l}_i$ at an increasing rate. Formally,

$$\frac{\partial b_l(l_i, \bar{l}_i)}{\partial l_i} > 0. \tag{4}$$

The solution to the profit maximization problem (2) is straightforward. The optimal operational land per farmer is

$$l_i^* = s_i \gamma^{\frac{1}{1-\gamma}} \left(\frac{\alpha}{r}\right)^{\frac{\alpha\gamma}{1-\gamma}} \left(\frac{1-\alpha}{q(1+\tau(l_i^*, \bar{l}_i))}\right)^{\frac{1-\alpha\gamma}{1-\gamma}} \tag{5}$$

where $\tau(l, \bar{l}) = b_l(l, \bar{l})/q$ represents an *endogenous* land wedge (i.e., a "distortionary tax" on the rental price of land). The RHS of the equation is decreasing in l while the LHS is increasing in l, and hence the solution is unique. Further, the optimal land operational scale depends on the farm's productivity and its initial endowment. This way, we denote $l_i^*(s, \bar{l})$ as the solution to this first order condition. One can easily show that l_i^* decreases with τ and q (and also through r due to its complementarity with capital). The solution for capital k_i^* is analogous, and although there is no direct wedge on capital, it is straightforward to show that its optimal allocation is also affected by the implied distorted factor rental prices.

3.2 Equilibrium

Given an initial joint distribution of land endowments, capital and permanent productivity $\Phi(s, \bar{l}, \bar{k})$, an equilibrium is a set of land and capital allocations (l_i^*, k_i^*) such that given the rental prices of land q and capital r farms solve their profit maximization problem (2) and land and capital rental markets clear, that is, $\sum_i l^*(s_i, \bar{l}_i) = L = \sum_i \bar{l}_i$. and $\sum_i k^*(s_i, \bar{l}_i) = K = \sum_i \bar{k}_i$, respectively.[16]

[16] We describe a solution algorithm to find this equilibrium in Appendix C.1.

3.3 Theoretical Effects of Land Rental Markets

We now use our theoretical framework to qualitatively examine the effects of access to land rental markets. To do this, we need to specify a functional form for the barriers to accessing land rentals that satisfy the assumptions described in (4). We assume these barriers take a standard quadratic cost adjustment function $b(l, \bar{l}) = \frac{\chi_z}{2}(l - \bar{l})^2$, where $\chi_z \in [0, \infty)$ is a zone-specific component capturing the difficulty of accessing to land rental markets within a given zone (a geographical area) z. Because we are interested in resource allocations within zones z which are subject to potentially different implementations of the land reform policy, we separately solve for the optimal allocations within zones, that is, subject to the total amount of zone-specific aggregate capital K_z and land L_z.

We define the total amount of rentals in a given zone as

$$R_z = \frac{\sum_i (l^*(s_i, \bar{l}_i) - \bar{l}_i)\mathbb{1}(l^*(s_i, \bar{l}_i) > \bar{l}_i)}{\sum_i \bar{l}_i}, \tag{6}$$

where $\mathbb{1}(l^*(s_i, \bar{l}_i))$ is a binary variable which is equal to one if farmer i rents in land and zero otherwise. We use this binary variable to prevent double counting, i.e., renting in and renting out should be counted only once.

3.3.1 Upper and lower bounds

We use the model to characterize the effects of the land reform that changes the barriers associated with land rentals. This implies the analysis of the equilibrium allocations under different values for $\chi_z \in [0, \infty)$. We first focus on the lower and upper bounds of χ_z, and then on the perhaps more plausible interior values of χ_z.

(a) *Efficient* allocations ($\chi_z = 0$). We use three measures of resource misallocation that compare the equilibrium allocations with the *efficient* allocations. It is straightforward to show that in our framework, the *efficient* allocations are equivalent to the equilibrium allocations with perfect land rental markets, i.e., $\tau(l, \bar{l}) = \chi_z = 0$. If land rental markets function perfectly, then equilibrium allocations are

$$l^*(s_i, \bar{l}; \chi_z = 0) = l^e(s_i) = \frac{s_i}{S_z} L_z, \tag{7}$$

where $S_z = \sum_{i \in z} s_i$. That is, optimal land is proportional to farmer's productivity s_i. The solution for capital is analogous. Notice that in this case the initial endowment $\bar{l}_i$ does not affect the optimal operational scale of land which is solely a function of individual productivity s_i. Clearly, in this case, land rentals are positive ($R_z > 0$) as long as the initial land endowments $\bar{l}_i$ differ from the optimal allocations $l_i^e(s_i)$ for each and all i.

(1) Efficiency gains. Since the only friction in this economy is the barrier the access to renting market, if we set $\chi_z = 0$, then the solution to the profit maximization problem (2) is actually the *efficient* allocation of land and capital, i.e., we maximize aggregate output given aggregate resource feasibility.[17] In particular, the equilibrium farm output $y_i^*(s_i, \bar{l}; \chi_z = 0)$ is equal to the *efficient* farm output $y_i^e = \frac{s_i}{S_z} Y_z^e$, where Y_z^e is the zone-level *efficient* aggregate output equal to

$$Y_z^*(\chi = 0) = Y_z^e = \sum_{i \in z} y_i^e = S_z^{1-\gamma} (K_z^\alpha L_z^{1-\alpha})^\gamma.$$

[17] The *efficient* allocations, $l^e(s_i)$ and $k^e(s_i)$, maximize aggregate output subject to aggregate resource constraints. Specifically, $k^e(s_i)$ and $l^e(s_i)$ are the solution to $\max_{k^e, l^e} \sum_{i \in z} s_i^{1-\gamma} \left(k_i^\alpha l_i^{1-\alpha}\right)^\gamma$ subject to $\sum_{i \in z} k_i = K$ and $\sum_{i \in z} l_i = L$. This implies the following *efficient* allocations: $k_i^e(s_i) = \frac{s_i}{S_z} K_z$ and $l_i^e(s_i) = \frac{s_i}{S_z} L_z$.

This also implies that any other allocation of capital and land, $l_i(s_i, \bar{l}; \chi_z > 0)$ and $k_i(s_i, \bar{l}; \chi_z > 0)$, is not identical to the efficient allocations, $k_i^e(s_i)$ and $l_i^e(s_i)$. The difference indicates the extent of resource misallocation. In particular, we can measure how much the actual aggregate output in the data, $Y_z^a = \sum_{i \in z} y_i^a = \sum_{i \in z} s_i^{1-\gamma} \left((k_i^a)^\alpha (l_i^a)^{1-\alpha}\right)^\gamma$—where k_i^a and l_i^a are the allocations of capital and land from the data–differs from the efficient aggregate output Y_z^e. The ratio between actual and efficient aggregate output provides a summary statistic of the impact of misallocation on aggregate output and TFP within each zone:

$$e_z = \frac{Y_z^e}{Y_z^a} \geqslant 1. \tag{8}$$

where the ratio e_z measures the efficiency gain that zone z achieves if factors are reallocated efficiently within zones.

(2) Marginal product of land. An alternative direct measure of the extent of misallocation is the dispersion in the marginal product of land. To see this, notice that if $\chi_z = 0$, then our economy reaches the *efficient* allocations by equalizing the marginal product of land across all farms in zone z. The same occurs for capital. This way, the *efficient* marginal product of land for each firm i given by

$$\text{MPLa}_i^*(\chi = 0) = \text{MPLa}_i^e = (1-\alpha)\gamma \frac{y^e(s_i)}{l^e(s_i)} = (1-\alpha)\gamma \frac{Y_z^e}{L_z}$$

which is identical across farmers. This implies that the dispersion (standard deviation) of the MPLa_i across farms is zero in *efficiency* within a given zone, and strictly positive otherwise, i.e., with $\chi_z > 0$. Deviations from this *efficient* zero dispersion can be used to measure the extent of misallocation.

(3) Total Factor Revenue Productivity (TFPR). We also construct a widely used summary measure of misallocation as the dispersion of farm-level revenue productivity ("TFPR"). Under the *efficient* allocations in our framework, TFPR is give by

$$\text{TFPR}_i^*(\chi = 0) = \text{TFPR}_i^e = \frac{y_i^e(s_i)}{(k_i^e(s_i)^\alpha (l_i^e(s_i))^{1-\alpha}} = \frac{Y_z^e}{(K_z^e)^\alpha \, (L_z^e)^{1-\alpha}}$$

which is a constant and hence also equalized across farms. Therefore, we also use the dispersion (standard deviation) of TFPR across farms within a given zone to measure the extent of misallocation.[18]

(b) Absence of land rental markets $(\chi_z \to \infty)$**.** The opposite extreme to *efficiency* is the presence of prohibitive transaction costs, that is, a ban on land rentals. This is the case where absolutely no land certificates are redistributed (i.e., $\chi_z \to \infty$). If rentals are forbidden, then every farmer uses their endowment of land only. That is, the equilibrium allocation is $l^*(s_i, \bar{l}_i; \chi_z \to \infty) = \bar{l}_i$ and aggregate land rentals are zero, $R_z = 0$. In this case, the output of individual farms is given by $y^*(s_i, \bar{l}_i; \chi \to \infty) = s_i^{\frac{1-\gamma}{1-\alpha\gamma}} \left(\frac{\alpha\gamma}{r}\right)^{\frac{\alpha\gamma}{1-\alpha\gamma}} \bar{l}_i^{\frac{\gamma(1-\alpha)}{1-\alpha\gamma}}$. Then, the total equilibrium output of this zone is given by

$$Y_z^*(\chi \to \infty) = \sum_{i \in z} s_i^{\frac{1-\gamma}{1-\alpha\gamma}} \left(\frac{\alpha\gamma}{r}\right)^{\frac{\alpha\gamma}{1-\alpha\gamma}} \bar{l}_i^{\frac{\gamma(1-\alpha)}{1-\alpha\gamma}}.$$

[18] See Hsieh and Klenow (2009) and Adamopoulos et al. (2017) for related definitions of revenue productivity. In fact, it can be shown that dispersion of TFPR only depends on farm-level distortions and is independent of the underlying farmer productivity distribution S.

Therefore, the efficiency gain is:

$$e_z = \frac{Y_z^e}{Y_z^*(\chi \to \infty)} \geqslant 1,$$

which will be equal to one only if $s_i/s_j = \bar{l}_i/\bar{l}_j$ for *all* i and j.

The marginal product of land is

$$\text{MPLa}_i^*(\chi \to \infty) = (1-\alpha)\gamma \frac{y^*(s_i, \bar{l}_i; \chi \to \infty)}{l^*(s_i, \bar{l}_i; \chi \to \infty)} = \left(\frac{s_i}{\bar{l}_i}\right)^{\frac{1-\gamma}{1-\alpha\gamma}} \left(\frac{\alpha\gamma}{r}\right)^{\frac{\alpha\gamma}{1-\alpha\gamma}},$$

which is not equalized among farmers unless $s_i/s_j = \bar{l}_i/\bar{l}_j$ for *all* i and j. The revenue productivity (TFPR) is given by

$$\text{TFPR}_i^*(\chi \to \infty) = \frac{y^{nr}(s_i, \bar{l}_i)}{(k^{nr}(s_i, \bar{l}_i))^{\alpha}(l^{nr}(s_i, \bar{l}_i))^{1-\alpha}} = \left(\frac{s_i}{\bar{l}_i}\right)^{\frac{1-\alpha-\gamma+\alpha\gamma}{1-\alpha\gamma}} \left(\frac{\alpha\gamma}{r}\right)^{\frac{\alpha(\gamma-1)}{1-\alpha\gamma}}.$$

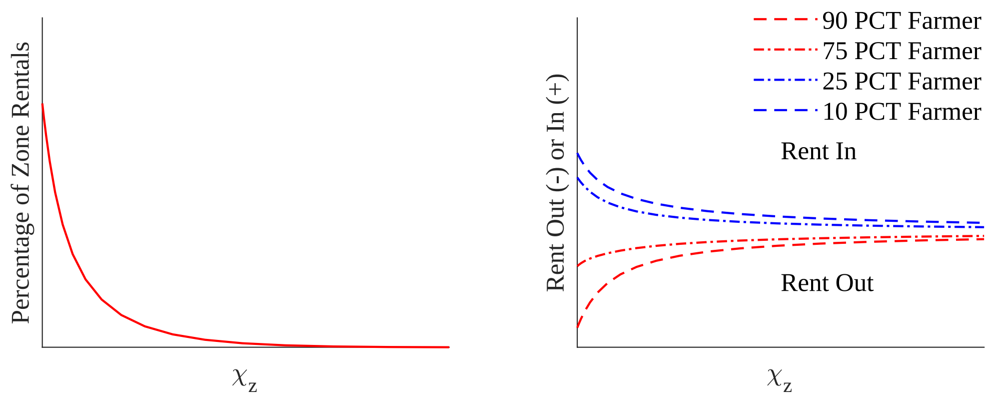
Again it will be equalized only if $s_i/s_j = \bar{l}_i/\bar{l}_j$ for *all* i and j.

(c) Imperfect land rental markets, $\chi_z \in (0, \infty)$. In reality, it is likely that χ_z is between zero and infinity. That is, not all households have a land certificate that allows them to rent-out land, but some do. This interior solution brings us back to the equilibrium allocations in (5).

3.3.2 Qualitative results

We now show the general qualitative relationship between the equilibrium land rentals R_z and the institutional barriers to land rental χ_z, but since there is no close-form solution for

Figure 2: Institutional Land Rental Costs (χ_z) and Equilibrium Land Rentals



Note: The left panel plots how the percentage of zone rentals decreases as land rental costs (χ_z) increase. The right panel plots the rent-in or rent-out activities for farmers with productivity at the 10th, 25th, 75th, and 90th percentile of the distribution.

intermediate values of the cost, we use a numerical solution of the equilibrium. Without loss of generality, and for illustration purposes, we only focus on the reallocation of land (not capital) and assume a scenario where all farmers are endowed the same amount of land, $\bar{l}_i = \bar{l}$ for all i. Farmers, however, differ in their individual productivity, s_i. Hence, farmers with highest or lowest s_i are the farthest away from efficient allocation.

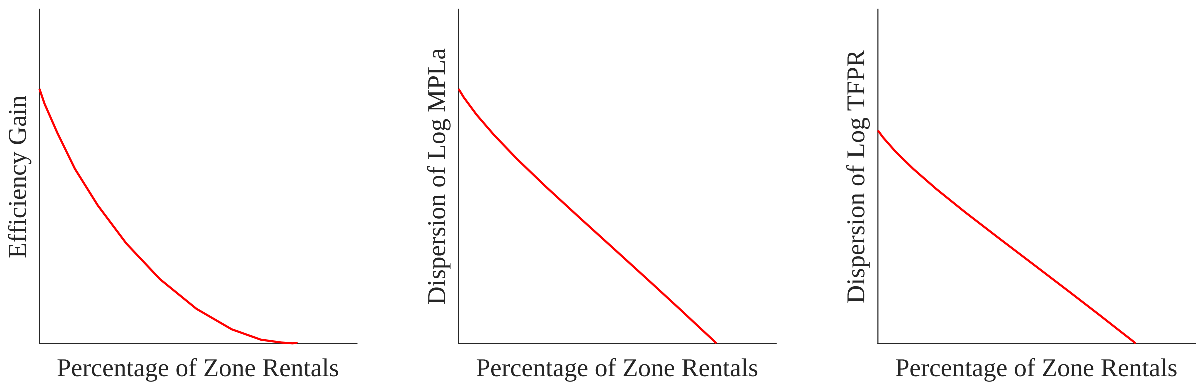
The left panel in Figure 2 shows the effect of the institutional land rentals costs, χ_z, on the percentage of land rentals in a given zone, R_z. The institutional costs are a catch-all friction for potentially different sources of misallocation related to land rentals in a given zone. For example, χ_z can represent the size of the distribution of land certificates among households in a given zone as well as additional frictions such as the fact that, despite the distribution of land certificates in a given zone, sub-zone local leaders might differ in their willingness to enforce what the land certificates entail. The institutional costs can also capture the fact that despite holding land certificates, household might be reluctant to use them for land transactions given the historical context, prone to expropriations, that they have experienced. It is reasonable to argue that it takes time—perhaps, a generation—for households to trust the new institutional environment. Independently of the interpretation of the institutional costs, we find that a reduction in these costs endogenously increases the amount of land rentals.

We scrutinize the effects across the distribution of productivity in a world where all farmers are initially endowed with the same amount of land. The right panel in Figure 2 shows that for any level of χ_z farmers with large productivity (e.g., top 10 and 25 percentile of the productivity distribution) rent-in land, and farmers with low productivity (e.g., 75 and 90 percentile of the productivity distribution) rent-out land. In addition, the farther farmers are from their operational scale the more rental activity they generate. For example, the top 10% of the productivity distribution rent in more land than the top 25% and the bottom 90% of the productivity distribution rent out more land than the bottom 75% of the distribution. Notice also that the lower are the institutional costs of land rentals the larger are these differential effects.

The reform that distributes land certificates aims at reducing the institutional costs, χ_z, and hence increase land rentals. We now show the effects of the land reform on our measures of resource misallocation. Clearly, the land rental reform reduces misallocation. The left panel of Figure 3 shows that the increase in rentals generated by a reduction of the institutional land rental costs in a given zone reduces the zone-level efficient gains. The same occurs for the within-zone dispersion of the MPLa and TFPR, see middle and right panels of Figure 3, respectively.

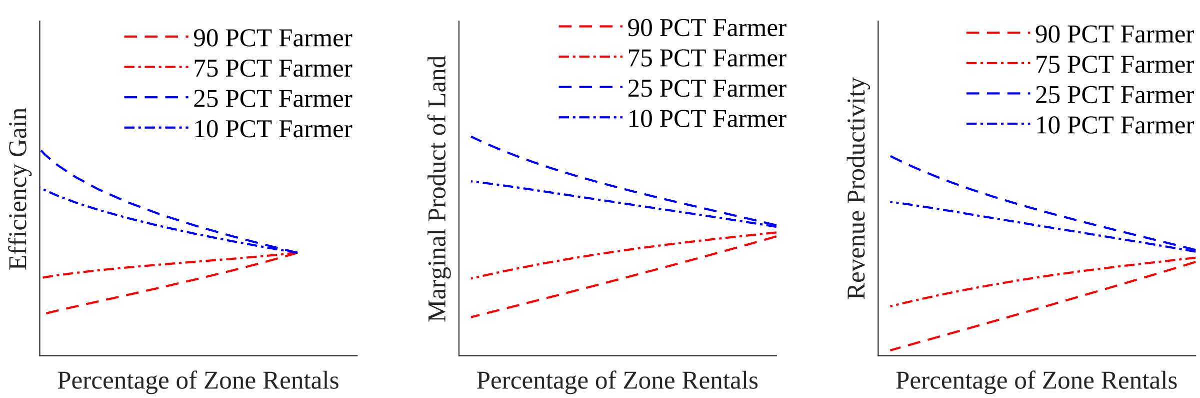
Further, we can also examine the impact of the land rental reform at the farm level. To

Figure 3: Effects of a Land Rental Markets on Zone-Level Misallocation



Note: The figures show that zone-level misallocation, measured as the zone-level efficiency gain, the dispersion of marginal product of land, or the dispersion of revenue productivity, declines with the percentage of zone rentals.

Figure 4: Effects of Land Rental Markets on Farm-Level Misallocation



Note: The left panel plots the relationship between farm-level efficiency gain, defined as $y_i^e(s_i)/y^*(s_i, \bar{l}_i)$, and the zone-level rental activities, for farmers with productivity at the 10th, 25th, 75th, and 90th percentile of the distribution. As rental activity increases in a zone, the farm-level efficiency gain converges to the zone-level average, which is one. The middle and right panels show the same relationship for the marginal product of land and for the revenue productivity.

do so, we define the farm-level efficiency gain as

$$e_i = \frac{y_i^e(s_i)}{y_i^*(s_i, \bar{l}_i)}$$

which should equal to one when there is no resource misallocation. Any deviation from one indicates misallocation: If farm-level efficiency gain is greater (smaller) than one, then the efficient output should be higher (lower) than the actual output. Similarly, we can define the farm-level MPLa and TFPR as

$$\text{MPLa}_i = \frac{y_i^*(s_i, \bar{l}_i)}{l_i^*(s_i, \bar{l}_i)}, \quad \text{TFPR}_i = \frac{y_i^*(s_i, \bar{l}_i)}{l_i^*(s_i, \bar{l}_i)^{1-\alpha} k_i^*(s_i, \bar{l}_i)^{\alpha}}.$$

Absent from misallocation, farm-level MPLa and TFPR should equal to their zone-level average and any deviation from zone average indicates misallocation. Figure 4 numerically illustrates our theoretical results at the farm level. A land reform that reduces χ_z increases land rentals within a zone which reduces farm-level efficiency gains. The same occurs for MPLa and TFPR that move toward the zone average. Notice that that the effects are non-linear in that the farmers that benefit most from the institutional change brought by the land reform are the ones that are the farthest away from their optimal operational scale.

4 The Empirical Effects of Land Markets

Our main empirical strategy consists of conducting a set of difference-in-difference exercises exploiting the variation in land rentals across time and space to capture both the intensive and extensive margins of the reform, as well as its maturity. Our difference-in-difference specifications simply differ in what we consider as treatment and control zones. As we will see, all these specifications (either intensive or extensive) deliver a unique message: land rentals unambiguously increase productivity by improving the efficiency of the allocation of factor inputs. Finally, we provide additional empirical evidence through a set of IV strategies.

4.1 The Effects of Increasing Land Reform Intensity

We define the control group as zones for which the share of rented land, R_z, difference-in-difference does not increase between 2013/14 and 2015/16, and the treatment group as those zones for which land rentals increases in that period. This specification captures the effects of increasing the intensity of the land reform independently of the reform maturity (i.e. level of R_z) in each zone. Whether or not our strategy is convincing clearly depends on how similar the control and treatment groups are. Table 2 shows that these two groups do not show significant differences in socioeconomic and demographic variables such as age, gender, marital status, schooling years, health status, and household size. There is no significant difference in geographical variables (e.g., distance to population centers and markets) and amount of rain either. We find some slight but significant differences in average slope of 1.2 degrees and in elevation of 61.3 meters. Perhaps, most importantly, we do not find significant differences between factor inputs (labor, land, and capital) and farm output which are crucial to determine productivity.

A potential concern is that the land institutional reform across zones may be correlated with the closeness to the central political leadership that decentralized the reform. That is, areas closer to the ruling power can decide to implement the reform quicker than those

Table 2: Comparison of Households in Treatment and Control Zones: Reform Intensity

	Treatment Zones Mean	Treatment Zones Std.	Control Zones Mean	Control Zones Std.	t-test $\|t\|$
Household Characteristics:					
Demographic Variables					
Household Head					
Age	46.7	14.7	45.9	14.6	1.30
Gender (1=female)	0.18	0.39	0.19	0.39	0.53
Marriage Status (1=married)	0.82	0.38	0.82	0.39	0.17
Schooling (years)	2.3	2.6	2.2	2.4	1.58
Health Status (1=healthy)	0.28	0.45	0.27	0.45	0.44
Number of Household Members	6.1	2.3	6.0	2.3	0.76
Population and Geographical Variables					
Distance to Market (km)	65.0	44.5	68.7	50.7	1.89
Distance to Population Center (km)	35.9	28.1	37.0	24.4	0.97
Annual Precipitation (mm)	1173.3	382.8	1149.3	370.5	1.53
Average Slope	11.8	10.1	13.0	10.5	2.95
Average Elevation (m)	1964.6	488.8	1903.3	543.9	2.87
Factor Input and Output					
Labor (hour)	1467.3	1800.6	1447.3	3398.0	0.18
Land (hectare)	1.3	4.5	1.3	3.8	0.06
Fraction of Rented Land	0.12	0.24	0.12	0.26	0.12
Capital (1000 local currency)	5.5	6.2	5.2	7.3	1.10
1 if use cattle	0.55	0.50	0.50	0.50	2.52
Output (1000 local currency)	23.0	37.7	24.1	44.8	0.72
Number of Observations	1,011		1,345		
Zone Characteristics:					
Distance to Political Leadership					
Distance to Capital (km)	424.8	217.7	440.3	220.8	0.29
Distance to Arjo (km)	543.6	294.5	556.3	273.2	0.18
Distance to Adwa (km)	990.2	403.1	1122.5	401.2	1.33
Distance to Boloso Sore (km)	560.2	350.8	470.3	347.4	1.04
Zone-Level Rented Land (R_z)					
2013/14	0.12	0.15	0.16	0.18	0.91
2011/12	0.19	0.18	0.13	0.13	1.58
Zone-Level Market Land (R_z)					
2013/14	0.11	0.15	0.14	0.18	0.68
2011/12	0.15	0.16	0.10	0.13	1.40
Number of Observations	27		42		

Note: This table compares the sample mean and standard deviation for key demographic, geographical, and economic variables between the treatment and control group. We define the control groups as those zones for which the share of rented land, R_z, did not increase between 2013/14 and 2015/16, and the treatment group as those zones for which land rentals increased in that period.

farther away from it because of, for example, differences in the provision of state capacity. If this were the case, it would suggest that the policy variation that we observe in terms of rental activity cannot be used to identify the effects of land rental markets. To assess this issue we test whether the treatment and control zones differ in distance to the capital city. To do so we use the available GPS location of the branches where survey agents work in each zone. We can link these branches to zones and use the distance between the branches and the capital city to approximate the distance between the zones and the capital city. The road distance is calculated by Google Maps through the shortest route by car. Our findings are that the distances do not differ significantly between the treated zones and the control zones, see Table 2.

Furthermore, since it is not unusual to find economic reforms taking place in locations with ties to the political leaders, we also calculate the distance of zones to the birth locations of the president and prime ministers of Ethiopia. The relevant presidents to our sample period are Girma Wolde-Giorgis (2001-2013, born in Addis Ababa, the capital city) and Mulatu Teshome (2013–2018, born in Arjo), and the relevant prime ministers are Meles Zenawi (1995–2012, born in Adwa) and Hailemariam Desalegn (2012–2018, born in Boroso Sore). None of the comparisons show significant differences between the treated zones and the control zones.

Finally, we study the level of rentals in the reference wave where we initiate the difference-in-difference (i.e., 2013/14) and find no significant difference in the amount of rented land between treatment and control zones. The same insights are obtained with data from the previous wave. We provide our benchmark results and an economic interpretation in Sections 4.1.1-4.1.3 and discuss further the validation of the difference-in-difference strategy with a pre-trend analysis and placebo tests in Section 4.1.4.

4.1.1 DID specification and results

We consider the following benchmark difference-in-difference specification to assess the impact of land rentals on resource misallocation:

$$|\log e_{izt}| = \alpha_z + \lambda_t + \psi d_{zt} + \beta \log \mathrm{TFP}_{iz} + \varepsilon_{izt}, \tag{9}$$

where e_{izt} is an individual measure of the degree of misallocation for farm i in zone z and time t, α_z is a zone fixed effect, λ_t is a year fixed effect, and dummy d_{zt} captures the implementation of the land rental reform. In the treatment zones the indicator variable d_{zt} equals one, and in the control zones d_{zt} equals zero. The parameter of interest is ψ, which captures the effect of land rentals on individual farm-level misallocation. We also control for the permanent component of individual farm-level TFP.

We use three measures of farm-level misallocation $|\log e_{izt}|$: (a) individual efficiency gain, $|\log(y^e_{izt}/y^a_{izt})|$, where y^e_{izt} is efficient output of farm i in a zone-level efficient reallocation and y^a_{izt} is actual output in the data; (b) individual marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\mathrm{MPLa}_{izt}/\overline{\mathrm{MPLa}}_{zt})|$; and (c) individual revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\mathrm{TFPR}_{izt}/\overline{\mathrm{TFPR}}_{zt})|$. Notice that deviations from efficient allocations may imply efficiency gains or loses and therefore the ratio in logs between efficiency values and actual data can take positive or negative values. For this reason, we consider the absolute value of the log efficiency gains. This implies that we can unambiguously interpret a negative (positive) estimate for ψ as a movement towards (away from) efficiency.

The difference-in-difference results are reported in Table 3, panel (a). Using farm-level productivity gains from zone-level efficient reallocations, we find that land rentals generate a significant decline in resource misallocation. The increase in land rentals generate a decline in efficiency gains with a significant coefficient of -0.132. We also apply this specification to the additional measures of farm-level misallocation: relative MPLa and relative TFPR. The estimated effect for each MPLa and TFPR is also negative and significant with coefficients of -0.192 and -0.151. Further trimming the increase in rentals we find larger effects. In particular, if we restrict the analysis to treatment zones that increase rentals by more than one percent, we find relevant coefficients of -0.156 for farm-level efficiency gains, -0.236 for MPLa, and -0.212 for TFPR (see Appendix D).

In sum, our analysis implies that a more active land market reduces resource misallocation and increases agricultural productivity. These empirical results are robust to the measure of misallocation used in the analysis. Also, the fact that we exploit variation across zones and over time underscores alternative explanations for the relationship between land rentals and misallocation such as misspecification or measurement error in inputs or output.

4.1.2 Non-linear effects

The previous results capture the average effect between land rentals and individual measures of misallocation. However, our theoretical framework implies that efficiency gains are larger when resources are reallocated among farmers with the larger deviations from efficient production. As a result, it is relevant to assess whether rental markets empirically ease misallocation disproportionally more for farmers farthest away from efficient production. To

Table 3: Effects of Land Rental Markets: Reform Intensity

(a) Benchmark Specification

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
Land Rentals (d_z)	-0.132	-0.192	-0.151
	(0.033)	(0.031)	(0.030)
Observations	4,712	4,712	4,712
R^2	0.23	0.12	0.16

(b) Quantile Specification

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
ψ_{Q1}	-0.034	-0.087	-0.007
	(0.030)	(0.032)	(0.028)
ψ_{Q2}	-0.054	-0.062	-0.077
	(0.034)	(0.037)	(0.033)
ψ_{Q3}	-0.128	-0.098	-0.099
	(0.040)	(0.036)	(0.039)
ψ_{Q4}	-0.278	-0.379	-0.362
	(0.070)	(0.068)	(0.062)

Notes: Results of Regression (9) in panel (a) and of Regression (10) in panel (b) for the following measures of farm-level misallocation: (a) efficiency gain $|\log(y^e_{izt}/y^a_{izt})|$, where y^e_{izt} is efficient output of farm i in a zone-level efficient reallocation and y^a_{izt} is actual output in the data, (b) marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\text{MPLa}_{izt}/\overline{\text{MPLa}}_{zt})|$, (c) revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\text{TFPR}_{izt}/\overline{\text{TFPR}}_{zt})|$. Standard deviations are in the parentheses. Panel data for the Ethiopia ISA 2013/14 and 2015/16 waves are as described in Section 2.

explore the potential non-linear relationship between land markets and misallocation across farmers, we divide the distribution of $|\log e_{iz}|$ into four quantile groups (quartiles) and run the following regression separately for each group:

$$|\log e_{izt}| = \alpha_{Qz} + \lambda_{Qt} + \psi_Q d_{zt} + \beta_Q \log \mathrm{TFP}_{iz} + \varepsilon_{izt}, \tag{10}$$

where the first quantile ($Q1$) represents farms that are closest to their optimal operational scale, and the last quantile ($Q4$) consists of farms that are farthest from their optimal operational scale.

Our findings are in panel (b) of Table 3. The effects of land rentals on resource misallocation is clearly nonlinear, consistent with the specification of our theoretical framework. As farmers approach their efficient operational scale, the beneficial relationship between land rentals and efficiency gains tends to diminish. Specifically, land rentals are not associated with much changes in the efficiency gain for farmers that are already close to their efficient allocation. The negative relationship between land rentals and efficiency gains starts to be significant in the third quantile, with $\psi_{Q3} = -0.128$, and substantially increases as we move away from efficiency with significant elasticities of $\psi_{Q4} = -0.278$ in the fourth quantile. Both estimates are significant at the one percent level. The results are even larger when using the other farm-level misallocation measures in terms of MPLa and TFPR (see the last two columns in panel (b) of Table 3).[19]

[19] Our results stand if we additionally control for other household characteristics, see Appendix E.

4.1.3 Economic interpretation of results

To provide a quantitative interpretation of our results, we use our quantile specification for farm-level outcomes to compute the changes in individual farm-level MPLa generated by an increase in land rentals across waves as follows:

$$\Delta\left|\log\left(\frac{\text{MPLa}_{izt}}{\overline{\overline{\text{MPLa}_{zt}}}}\right)\right| = \psi_Q. \tag{11}$$

Then, we plug our estimates for ψ_Q into equation (11) to compute the projected individual MPLa generated by an increase in land rentals, denoted as MPLa_{iz}^p. Note that we have $\text{MPLa}_{iz}^p \propto s_{iz}^{1-\gamma} k_{iz}^{\alpha\gamma} (l_{iz}^p)^{(1-\alpha)\gamma-1}$. We can then solve out the projected land input associated with rental l_i^p as

$$l_{iz}^p \propto \left(\frac{\text{MPL}_{iz}^p}{s_{iz}^{1-\gamma} k_{iz}^{\alpha\gamma}}\right)^{\frac{1}{(1-\alpha)\gamma-1}}. \tag{12}$$

Notice that equation (12) solves l_i^p up to a scalar, which is determined by the land market clearing condition of each zone $\sum_{i\in z} l_{iz}^p = L_z$. We can then substitute l_{iz}^p into the production function to solve out the projected output y_{iz}^p as $y_{iz}^p = s_{iz}^{1-\gamma} k_{iz}^{\alpha\gamma} (l_{iz}^p)^{(1-\alpha)\gamma}$ for the 2013/14 wave.

The projected zone-level efficiency gain associated with rental is calculated as $e_z^p = \sum_{i\in z} y_{iz}^e / \sum_{i\in z} y_{iz}^p$. Comparing the average of these implied efficiency gains (1.423) with our benchmark average efficiency gains per zone (1.527), we find that an increase in land rentals reduces efficiency gains by 16.6% on average per zone (calculated as $1-\log(1.423)/\log(1.527)$). Because the implementation of the land rental policy (i.e., $d_{z,t} = 1$) comes with different levels of growth of rentals per zone R_z, we divide the estimated effects by the aggregate growth in the share of rented land across our two waves within treatment groups, which is 5.1 percent. This implies that an increase in one percentage point of land rentals increases aggregate productivity by 16.6/5.1=3.2 percent.

4.1.4 Validation of the DID strategy

A common problem with difference-in-difference estimates is that the policy reform may be targeted, while a pre-condition of the validity of our difference-in-difference assumption is that the policy is not implemented based on pre-existing differences in productivity across zones. In other words, one possible reason why we find that zones that increase land rentals reduce within-zone misallocation is that those zones for which rentals increase already have a pre-existing decreasing trend in misallocation, compared with zones for which rentals do not increase. This would imply that the reason behind the reduction in misallocation is not land rentals, but something else that explains both increasing land rentals and reductions in misallocation suggesting a clear endogeneity problem between these two variables. We empirically address this issue in two ways.

First, we design a placebo test to assess this common trend using the previous Ethiopia ISA 2011/12.[20] Consider again our benchmark difference-in-difference specification (9), where the dummy d_{zt} indicated treatment in the 2015/16 wave. Suppose that we instead estimate that same specification using the previous waves, i.e., 2011/12 and 2013/14 data.

[20]Notice that Ethiopia ISA 2011/12 wave cannot be directly used to quantify misallocation because agricultural output is missing from that dataset. Fortunately, the data needed to construct all our factor input variables (including land quality) is collected in the 2011/12 wave. This implies that we can use our benchmark permanent component of farm productivity estimated using the 2013/14 and 2015/16 waves to reconstruct the implied farm output in the 2011/12 wave net of transitory components, exactly as we did for the 2013/14 and 2015/16 waves. In this manner, the results for 2011/12 wave are comparable across all three waves. A caveat of this approach is that the permanent component is computed using the last two waves, as opposed to using all three waves.

Table 4: Placebo Test

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	TFPR	MPLa
Land Rentals (%), ψ	-0.012	0.006	-0.029
	(0.037)	(0.037)	(0.034)
Observations	3,706	3,706	3,706
R^2	0.22	0.16	0.23

Note: This table shows the results of the placebo test, where we re-estimate Equation (9) using 2011/12 and 2013/14 wave data, and assign the treatment to the same zones for 2013/14 wave.

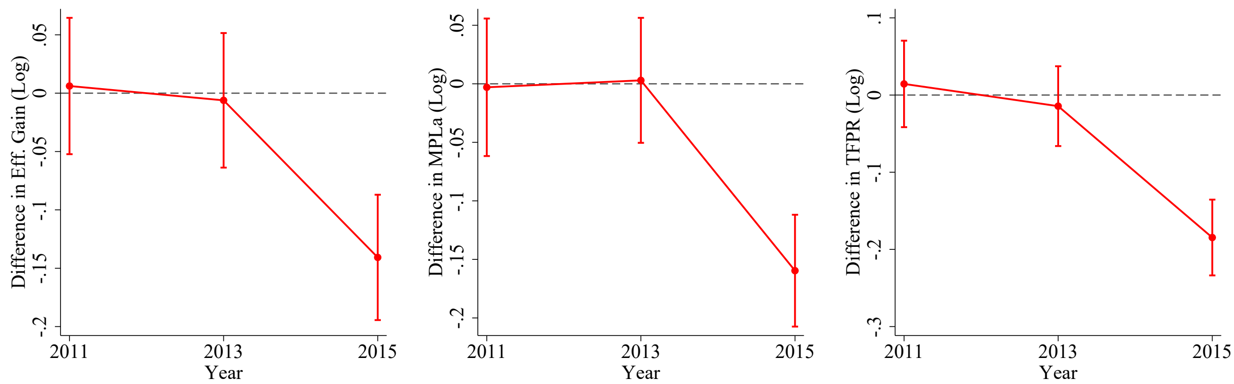
That is, assume that the same zones that are treated in 2015/16 wave are also treated in 2013/14 wave. If both groups of zones have common trend before 2015/16, then we should find the estimate of ψ to be insignificant. We report the results of this placebo test in Table 4. There is no pre-existing reason (or trend) that reduces misallocation within the treated zones compared with the control zones.

Second, Figure 5 shows the average of misallocation (weighted by farm output) within zones where rentals increase between 2013/14 and 2015/16 waves (treated group) and within zones where rentals do not increase (control group) with associated confidence intervals. We show these results separately for our three measures of misallocation. Clearly, the two groups have similar trends between 2011/12 wave and 2013/14 wave, but the trends differ after that, as those zones where rentals increase after 2013 have significantly lower misallocation in the 2015/16 wave.

4.2 The Effects of Land Reform Adoption

Despite the fact that the land policy reform was launched in early 2000s, our sample still captures zones for which the land reform has not yet been implemented at all. This allows

Figure 5: Common Trend Analysis: Misallocation Outcome from Reform Intensity



Note: The figure shows the differences in the average within-zone misallocation between the treated group, where rentals increase between 2013/14 and 2015/16 waves, and the control group, where rentals do not increase. We show these results separately for our three measures of misallocation. The bar shows the 95% confidence interval.

us to assess the effects of reform adoption on misallocation. To do this, we use two common difference-in-difference strategies where the treatment group are the late reformers defined as the zones that adopted the land rental market reform between 2013/14 and 2015/16. That is, the treated zones exhibit land rentals in 2015/16, but not in 2013/14. The two difference-in-difference strategies differ in the control group that we describe next.

4.2.1 Unreformed vs. late reformers

We estimate the effects of land markets by comparing the behavior of late reformers (treated zones) against unreformed (control zones). There are eight zones for which the proportion of rented land in total land is not significantly different from zero in 2013/14. Out of these eight zones, six zones implemented the reform and initiated land rentals in 2014/15 (treated zones), and two zones did not implement the reform and remained without land rentals in 2014/15 (control zones). We use this 8-zone subsample to re-estimate our benchmark

Table 5: Effects of Reform Adoption

	(a) Unreformed vs. Late Reformers					
	(a1) Effects of Land Markets			(a2) Placebo Tests		
	Eff. Gain	MPLa	TFPR	Eff. Gain	MPLa	TFPR
Land Rentals, ψ	-0.184	-0.240	-0.241	0.052	-0.068	-0.175
	(0.123)	(0.114)	(0.117)	(0.184)	(0.195)	(0.178)
Observations	332	332	332	150	150	150
R^2	0.10	0.15	0.16	0.20	0.17	0.27

	(b) Early vs. Late Reformers					
	(b1) Effects of Land Markets			(b2) Placebo Tests		
	Eff. Gain	MPLa	TFPR	Eff. Gain	MPLa	TFPR
Land Rentals, ψ	-0.135	-0.259	-0.252	0.060	-0.033	0.002
	(0.083)	(0.079)	(0.079)	(0.069)	(0.070)	(0.065)
Observations	1,378	1,378	1,378	1,180	1,180	1,180
R^2	0.23	0.18	0.17	0.29	0.19	0.26

Note: Results of Regression (9) for the following measures of farm-level misallocation: (a) efficiency gain $|\log(y^e_{izt}/y^a_{izt})|$, where y^e_{izt} is efficient output of farm i in a zone-level efficient reallocation and y^a_{izt} is actual output in the data, (b) marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\text{MPLa}_{izt}/\overline{\text{MPLa}}_{zt})|$, (c) revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\text{TFPR}_{izt}/\overline{\text{TFPR}}_{zt})|$. Standard deviations are in the parentheses. Panel data for the Ethiopia ISA 2013/14 and 2015/16 waves are as described in Section 2. In Panel (a) we compare the unreformed zones with zones who start reform in 2015/16 but not in 2013/14 as described in Section 4.2.1. In Panel (b), we compare those late reformers to early reformers as described in Section 4.2.2. The results of the placebo test re-estimate Equation (9) using 2011/12 and 2013/14 wave data, and assign the treatment to the same zones for 2013/14 wave.

difference-in-difference specification (9) with dummy $d_{zt} = 1$ for the treatment group defined by $R_{z,2013/14} = 0$ and $R_{z,2015/16} > 0$, and $d_{zt} = 0$ for the control group defined by $R_{z,2013/14} = R_{z,2015/16} = 0$. Clearly, this extensive margin analysis is much more demanding on our data than the one focused on reform intensity (Section 4.1) as our sample reduces to a total of 332 households spread into two control zones and six treated zones, with 72 and 260 households respectively.

We find that, compared with the control group, zones that start to implement the land policy largely reduce misallocation in whichever way we measured it, as efficiency gains, relative MPLa, and relative TFPR (see panel (a1) of Table 5). Interestingly, the point estimates are approximately one-half times larger with this smaller sample than those obtained for the intensive margin in the previous section. Not surprisingly, given the fewer number of observations, the standard errors are larger in the current specification. However, even with fewer number of observations, we still find significant results at the five percent level for MPLa and TFPR. Finally, to validate the difference-in-difference strategy we reconduct a placebo test analogous to that described in Section 4.1.4 which rejects the hypothesis of differential trends between treated and control zones (see panel (a2) of Table 5).

4.2.2 Early reformers vs. late reformers

We estimate the effects of land markets by comparing the behavior of late reformers (treated zones) against early reformers (control zones). To catch early reformers we use zones for which land rentals are consistently above 20% in both 2013/14 and 2015/16. The threshold choice follows the argument that the amount of land rentals in zones that adopt the reform increase gradually. The implied sample comprises approximately one third of our original sample of households. Although it does not affect our results, we increase the sample size of late reformers by redefining these zones as those that have less than 10% of land rentals in 2015/16. We drop the unreformed from the analysis.

The results are in panel (b1) of Table 5. Clearly, compared with the early reformers, late reformers largely and significantly reduce misallocation in whichever way we measure it. The placebo tests validate our results (see panel (b2) of Table 5).

4.3 Short-Run Effects across the Maturity of the Reform

We assess the short-run effects of land markets across the maturity of the reform through the lens of a unified framework that considers the gradual rise in land rental market activity. Specifically, the fact that the land reform has been ongoing for a prolonged period of time allows us to condition on the zone-level maturity of the reform defined by the proportion of land rentals in a given zone, R_z. We assess the following difference-in-difference specification that encompasses the intensive and extensive margins of the two previous specifications:

$$|\log e_{izt}| = \alpha_z + \lambda_t + \sum_m \psi_m d_{zt}^{(m)} + \beta \log \mathrm{TFP}_{iz} + \varepsilon_{izt}, \tag{13}$$

where e_{izt} is an individual measure of the degree of misallocation for farm i in zone z and time t, α_z is a zone fixed effect, λ_t is a year fixed effect. The set of indicator variables $d_{zt}^{(m)}$ depends on the maturity of the reform m which we define in three stages or groups. Precisely, if the share of rented land strictly increases between 2013/14 and 2015/16, then $d_{z,t}^{(1)} = 1$ if $R_{z,2013/14} \in [0, 5.0]$, $d_{z,t}^{(2)} = 1$ if $R_{z,2013/14} \in (5.0, 10.0]$, $d_{z,t}^{(3)} = 1$ if $R_{z,2013/14} \in (10.0, 100.0]$, and 0 otherwise. We choose the thresholds in R_z that define the stages in order to retain

Table 6: Effects of Land Rental Markets: Conditional on Reform Maturity

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
Land Rentals, ψ_m			
$R_{z,2013/14} \in [0,5]$	-0.111	-0.244	-0.179
	(0.041)	(0.039)	(0.038)
$R_{z,2013/14} \in (5,10]$	-0.179	-0.232	-0.208
	(0.055)	(0.051)	(0.051)
$R_{z,2013/14} \in (10,100]$	-0.130	-0.063	-0.052
	(0.052)	(0.049)	(0.048)
Observations	4,712	4,712	4,712
R^2	0.23	0.13	0.16

Notes: Results of Regression (9) for the following measures of farm-level misallocation: (a) efficiency gain $|\log(y^e_{izt}/y^a_{izt})|$, where y^e_{izt} is efficient output of farm i in a zone-level efficient reallocation and y^a_{izt} is actual output in the data, (b) marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\text{MPLa}_{izt}/\overline{\text{MPLa}}_{zt})|$, (c) revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\text{TFPR}_{izt}/\overline{\text{TFPR}}_{zt})|$. Panel data for the Ethiopia ISA 2013/14 and 2015/16 waves are as described in Section 2.

a balanced sample by which each stage consists of approximately one third of our original sample.

The results are in Table 6. We find that although rental markets significantly reduce misallocation at early stages of the reform, these effects dissipate in the most advanced stages of the reform. This is expected as the scope of land reallocation is reduced the closer land allocations are to efficient levels. In the case of efficiency gains, the first stage of the reform ($R_z \in [0,5]$) significantly reduces misallocation, and this effect is even slightly larger at the second stage of the reform ($R_z \in (5,10]$). In the case of relative MPLa and relative TFPR, land markets the largest effect of land markets occurs at the first stage, although effects remain large and significant at the second stage of the reform. The last stage of the reform ($R_z \in (10,100]$) implies smaller and less significant effects of land markets.

4.4 Additional IV Evidence

We recognize that a valid concern on the use of a policy as an exogenous source of variation is the potential endogeneity of the policy implementation itself. The idea is that some local governments might be willing—or bounded—to enforce the land reform quicker or more intensively than others which can potentially yield endogenous differences in R_z. In this context, notice that our pre-trends analysis in Section 4.1.4 suggests that there is no pre-existent factor (including type of local government or institution) that explains the rise in land rentals and the reduction of misallocation. In addition, our examination of the distance to political leadership between treatment and control zones showed no significant differences (Table 2) which adds evidence to our causal interpretation of the empirical difference-in-difference effects of land markets. Nevertheless, we now address the endogeneity concern by using two sets of instrumental variables.[21]

The first instrument that we use is the religious status of local farmers. Religion is a good candidate for an instrument for several reasons. First, more than 95% of the population in Ethiopia adheres to a religion. In our sample, around 42.3% of farmers are Christian Orthodox, 31.2% of Muslim, and 23.4% of Protestants, with the remaining farmers abiding to other (or none) religions. Importantly, the composition of religions varies substantially across zones. For example, we find five zones with no Orthodox farmers, while in other seven zones all farmers are Orthodox. Similarly, there are twenty-three zones with no Muslim farmers. Second, it is very rare to see transitions across religions, which are basically transmitted from parents to children (inherited) and hence ex-ante exogenous. Third, religion is correlated

[21] We further address endogeneity by assessing a controlled experiment on our theoretical model in Section 5.3. Specifically, we implement an exogenous and unexpected policy change in our model that replicates the changes in land rental market activity by zone.

with land rentals which provides a strong first stage. For example, 47.9 percent of Orthodox farmers participate in land rentals, compared to only 21.6 percent among Muslim farmers. This could be due to the lack of the tradition of charging rents among Muslim farmers historically. In fact, we do observe this difference: 68.5 percent of land rentals among Orthodox farmers take place in exchange of either monetary or in-kind payments, while this number is only 50.5 percent among Muslim farmers. If charging rents is culturally more costly for Muslims, then their farmers have potentially less incentives to rent out their farm land. This implies that establishing well-functioned land rental markets can be more costly *ceteris paribus* in the Muslim population. We also find strong correlation at the zone level: the rank correlation between the rental percentages and the Orthodox percentages is 0.61 and is significant at the one percent level. Further, we argue that religion satisfies the exclusionary restriction since the institutional context is such that land rentals are the only channel generating the land transactions (reallocations) that we study. That is, religion can affect resource re-allocations only through land rentals. This is in consonance with our theoretical framework in Section 3 (see also the policy experiment in next Section 5) in which resource allocations are affected by land rentals through an institutional cost that determines the access to land rental markets, χ_z. That is, an instrumental variable strategy rationalized using our theoretical framework implies that religion takes the role of χ_z as institutional—or cultural—cost. Finally, also notice that measurement error in religion denomination should be minor (if not absent). The instrumental variable results using the Orthodox percentage of farm households at the zone level as instrument are reported in panel (a) of Table 7. In line with our difference-in-difference results, we find that more rentals reduce misallocation at the farm level and increase agricultural productivity.

Table 7: Effects of Land Rental Markets: Two IV Strategies

(a) Orthodox Population

Dependent variable:	Farm-Level Misallocation					
	Efficiency Gain		MPLa		TFPR	
	OLS	IV	OLS	IV	OLS	IV
Land Rentals (%), ψ	-0.067	-0.066	-0.092	-0.059	-0.068	-0.046
	(0.010)	(0.018)	(0.009)	(0.016)	(0.009)	(0.016)
Weak ID Test (Cragg-Donald Wald F statistic):						
	-	1242.5	-	1242.5	-	1242.5
Stock-Yogo Weak ID 10% Critical Value: 16.38.						
Observations	2,545	2,545	2,545	2,545	2,545	2,545
R^2	0.13	0.13	0.04	0.03	0.04	0.04

(b) Lagged Land Rentals

Dependent variable:	Farm-Level Misallocation					
	Efficiency Gain		MPLa		TFPR	
	OLS	IV	OLS	IV	OLS	IV
Land Rentals (%), ψ	-0.058	-0.087	-0.057	-0.062	-0.073	-0.073
	(0.011)	(0.019)	(0.010)	(0.016)	(0.010)	(0.017)
Weak ID Test (Cragg-Donald Wald F statistic):						
	-	1239.0	-	1239.0	-	1239.0
Stock-Yogo Weak ID 10% Critical Value: 16.38.						
Observations	2,168	2,099	2,168	2,099	2,168	2,099
R^2	0.06	0.06	0.02	0.03	0.10	0.10

Notes: We report OLS and IV results from a cross-sectional estimation of the following specification $|\log e_{iz}| = \alpha + \psi \log R_z + \beta \log \mathrm{TFP}_{iz} + \varepsilon_{iz}$ for Ethiopia ISA 2013/14. In panel (a) we use the percentage of Orthodox farmers as the instrument of the rental percentage within zones. In panel (b) we use the percentage of rentals of the same zone in the previous period as the instrument of the rental percentage of the current period. We report the elasticity of land rentals and individual farm-level misallocation for these two specifications. Data used is for Ethiopia 2013/14.

The second instrument that we use is the lagged rental ratio of the same zone. In particular, when we estimate the effects of rental on misallocation using the 2015/16 sample, we use the zone-level rentals of the previous previous period (2013/14) as the instrument. The lagged rental ratio affects current misallocation only through its persistent effect on current rental ratio, and hence it satisfies the exclusive restriction. We report the results in panel (b) Table 7. Again, we find similar significant results in that more rentals reduce misallocation.

5 Calibration and Policy Experiment

We now describe our calibration strategy and show the quantitative model results. First, we use our theoretical framework to quantify the extent of misallocation within zones in Section 5.1. Second, we connect the model generated zone-level misallocation to the level of land rentals per zone in Section 5.2. Third, we conduct a policy experiment on our model that replicates the land market reform in Section 5.3. The policy experiment provides a tight comparison between the model-generated effects of land rental markets and the empirical effects of land markets reported in Section 4. Finally, we assess the effects of land markets on inequality in Section 5.4.

5.1 Measuring the Extent of Misallocation

To measure the extent of misallocation within zones we solve our theoretical framework for the *efficient* allocations defined in equation (7) (i.e., under $\chi_z = 0$) separately for each zone. Notice that to conduct this exercise we only need to calibrate two technological parameters, α and γ, governing the factor income shares of the production function (equation (1)). As described in Section 3, we use the micro data to estimate factor income shares (see also Appendix B).

In Table 8 we report several statistics for the efficiency gains—the ratio of *efficient* to actual output as in equation (8)—by zone. On average, the efficiency gain per zone is 1.66-fold with a median of 1.64-fold. This median estimate is tight with bootstrap standard deviation of 0.03 and is significant at the one percent level. This means that reallocating resources from the actual allocation to the efficient allocation across existing farmers increases aggregate output by approximately 66 percent on average.[22] There is substantial variation in efficiency gains across zones with a 95th percentile of 2.81 and a 5th percentile of 1.28. Because we compute the average gain using actual output as weights, the average gain is the country-level gain of eliminating within-zone misallocation. For comparison purposes with previous literature, we also conduct our reallocation exercise nationwide and find that the efficiency gains are 2.00. This implies that eliminating within-zone misallocation accounts for $\log(1.66)/\log(2.00) = 73$ percent of the overall efficiency gain. The remaining 27 percent is accounted for by reallocating resources across zones.

We also report the dispersion in farm-level marginal product of land (MPLa) and in TFPR across farms within zones in Table 8. Recall that efficiency requires the MPLa and TFPR to be equalized across farms within each zone. Instead, we find that there is a substantial amount of dispersion in MPLa, with a standard deviation of 0.84 on average per zone, and a median of 0.81. Also, on average, the standard deviation of (log) TFPR is 0.84 per zone, see Table 8. The median zone estimate is 0.82. As a comparison, Hsieh and Klenow (2009) find this statistic to be 0.63 and 0.67 in the manufacturing sector of China and India.[23] This indicates that the extent of misallocation within zones is severe in Ethiopia. Further, the

[22]These numbers are based on Ethiopia ISA 2013/14, but we also find similar within-zone efficiency gains in Ethiopia ISA 2015/16 with an average efficiency gain per zone of 1.67-fold and a median of 1.59-fold.

[23]Nationwide, the standard deviation of (log) TFPR is 0.99 in our sample. The $75 - 25$ difference is 1.19 in our sample, compared to 0.82 and 0.81 in China and India, respectively. The $90 - 10$ difference is also larger in our sample.

Table 8: The Extent of Misallocation within Zones, Ethiopia ISA 2013/14

	Average	Median	5th pct.	95th pct.
Efficiency Gain	1.66	1.64	1.28	2.81
MPLa (Std.)	0.84	0.81	0.55	1.15
TFPR (Std.)	0.84	0.82	0.61	1.13

Notes: Data for Ethiopia ISA 2013/14 as described in Section 2. We compute the average efficiency gain using actual output as weights, hence the average gain is the country-level gain of eliminating within-zone misallocation.

within-zone correlation between log TFPR and log farm TFP is 0.86 on average, indicating that correlated implicit distortions constitute a strong source of misallocation. This confirms our earlier characterization that more productive farms, unable to operate at larger scales, face larger implicit distortions.

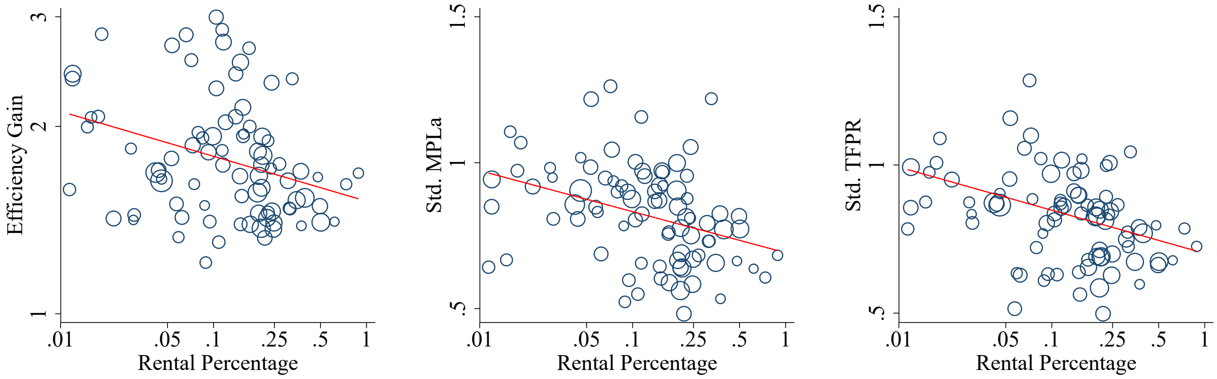
In summary, our analysis provides evidence of substantial factor misallocation in the agricultural sector of Ethiopia in the form of MPLa and TFPR dispersion, and a substantial cost of misallocation in the form of large efficiency gains from reallocation within zones. Further, the variation in the extent of within-zone misallocation is also substantial across zones. Because the land reform policy was implemented in a decentralized manner across zones, with local governments dictating timing and intensity, we investigate the cross-sectional association between land rentals and the extent of misallocation across zones next.

5.2 Cross-Sectional Relationship Between Land Rentals and Within-Zone Misallocation

We now use the variation in land rentals and in the extent of misallocation across zones to explore the association between those two variables.[24] We use the three measures of

[24] Our results also hold at the narrower geographical locations than zones such as woredas.

Figure 6: Land Rentals and the Extent of Misallocation within Zones



(a) Efficiency Gain (b) MPLa (c) TFPR

Notes: We show the within-zone extent of misallocation e_z (in logs) with respect to the fraction of rented land R_z (in logs) by zone. We report 84 zone-year observations that have a positive percent of rental land and more than 10 observations. We also trim zones with the highest and lowest efficiency gain. The within-zone e_z is computed separately for each of the three measures of misallocation described in the test: (a) within-zone efficiency gain (left panel), (b) dispersion in the marginal product of land (MPLa) (center panel), and (c) dispersion of farm-level revenue productivity (TFPR) (right panel). The size of the circles indicate the number of observations in each zone.

misallocation discussed previously: productivity gains from efficient reallocation, dispersion in MPLa, and dispersion in TFPR.

The association between the within-zone efficiency gains and the within-zone percentage of land rentals R_z (i.e. the proportion of rented land of total land in zone z) is illustrated in panel (a) of Figure 6. The solid line corresponds to the predicted value of $\log e_z = \alpha + \lambda_t + \psi \log R_z + \varepsilon_z$, where α is a constant and λ_t is a year fixed effect for the pooled samples of Ethiopia ISA 2013/14 and Ethiopia ISA 2015/16. The estimates are accuracy weighted by the number of observations in each zone. We also replicate this relationship for the other two measures of misallocation, namely the dispersion of MPLa and TFPR; see panel (b) and (c) in Figure 6. The estimated elasticities ψ with respect to R_z are -0.072, -0.061, and -0.063, respectively; all three are significant at the one percent level. Clearly, more land rentals are associated with less misallocation. Notice that the association is sizeable. For example, focusing on efficiency gains, we find that a one percent increase in land rentals, from its average by zone of 10.9, is associated with a 0.66 percent productivity gain given by $-0.072 \cdot \frac{\Delta R_z}{R_z} = -0.072 \cdot \frac{1}{10.9} = -0.66\%$.[25]

Keeping the within-zone level of distortions constant, larger within-zone dispersion of TFP generates higher efficiency gains from reallocation. Then, it is possible that the efficiency gain in a zone is related to the dispersion of TFP in that given zone. To address the potential differences in TFP dispersion across zones, we use the following specification:

$$\log e_z = \alpha + \lambda_t + \psi \log R_z + \beta \text{disp}_z^{\text{TFP}} + \varepsilon_z, \tag{14}$$

where the new element $\text{disp}_z^{\text{TFP}}$ is the dispersion of farm TFP within zone z defined as the 90-10 ratio of farm TFP. We control for the dispersion of farm TFP as it also affects the efficiency gain and the zone-level rentals. We use as weights the number of observations in each zone. Again, the parameter of interest is the elasticity ψ. We find that the point estimate for ψ is -0.049, indicating that zones with a higher share of rented land tend to have lower efficiency gain, even after controlling for the dispersion in TFP. The estimates of the elasticity ψ are similar, -0.027 and -0.030, when we use the dispersion of MPLa and TFPR, respectively, as measures of misallocation. All these three coefficients are significant at the five percent level. Let's emphasize that these results merely show an association between land rentals and the extend of misallocation, but do not provide a causal link. Next, we conduct a policy experiment on our model that establishes an endogenous link between land

[25] See also Appendix F for an alternative assessment of this cross-sectional relationship between land rentals and productivity.

rentals and resource misallocation.

5.3 Policy Experiment

We now use our theoretical framework to quantitatively assess the effects of land rental markets on resource allocation and productivity. We are particularly interested in assessing whether our calibrated model is able to generate the estimated empirical effects of land markets reported in Section 4. To do so, we implement on our model-generated data the same difference-in-difference strategy used in Section 4.

First, recall that the institutional cost χ_z—which represents barriers to accessing land rentals markets such as the granting of land certificates (or their lack of)—is endogenously related to land rentals in our model: A reduction in χ_z endogenously generates an increase in the amount of land rentals (see Figure 2 in Section 3.3). Here, we use this theoretical relationship between χ_z and land rentals to calibrate χ_z in order to match the actual level of land rentals, R_z, separately by zone in 2013/14. This implies adding an outerloop for χ_z to the solution algorithm that solves for the model equilibrium allocations—if the iterative value of χ_z generates larger (smaller) land rentals than those observed in the data for zone z, we increase (decrease) χ_z in the next iteration till convergence (see Appendix C.2). In this manner, we are able to perfectly match the land rentals per zone, R_z, for each of the 67 zones under study in 2013/14.[26] We denote the model-generated output for farm i in zone z at this level of rentals as y^m_{izt}. Then, the efficiency gain associated with this *status quo* level of rentals is given by $|\log e^m_{izt}| = |\log(y^e_{izt}/y^m_{izt})|$. At this point, notice that we can

[26]This implies a median value for χ_z of 1.67 and a range that goes from the 5th percentile of 0.09 to the 95th percentile of more than 100—for the zones for which there are no rentals.

compute how much the specific institutional cost on land rentals χ_z—calibrated to match the rental activity in 2013/14—accounts for the extent of misallocation per zone reported in Section 5.1. This implies computing the ratio $\frac{\log(Y^e_{zt}/Y^m_{zt})}{\log(Y^e_{zt}/Y^a_{zt})}$ where $Y^m_{zt} = \sum_{i \in z} y^m_{izt}$ and recall that Y^e_{zt} and Y^a_{zt} are, respectively, the aggregate efficient and actual output per zone. We find that the institutional cost on land rentals χ_z explains 51% of the extent of misallocation for the median zone (24% and 79%, respectively, for the 5th and 95th percentile zones). Clearly, although the institutional cost in accessing land rental markets, χ_z, cannot fully explain the observed extent of misallocation, it accounts for an important part of this misallocation which adds further relevance to the study of this land reform.

Second, we conduct a policy experiment on the *status quo* allocations to assess the effects of land markets through a land reform that we formalize as an unexpected reduction in the institutional costs, χ_z. Precisely, the land reform consists of changing χ_z so as to match the actual changes in land rental activity by zone between our two last waves of data 2013/14 and 2015/16. We then recompute the *counterfactual* factor input allocations that result from this policy experiment. Importantly, notice that in this policy experiment we do not target the empirical difference-in-difference results neither in our calibration strategy nor in our specification of the policy experiment which is solely based on changing χ_z as to match the change in rentals by zone.

To compare the model-generated effects of land markets with the empirical effects estimated in Section 4, we use the model-generated *status quo* and *counterfactual* allocations in order to estimate the difference-in-difference specification (9) on reform intensity and its quantile specification (10). Our main finding is that the results from the policy experiment on our model are very similar to the estimated empirical effects. Panel (a) of Table 9 shows

Table 9: Policy Experiment on Land Reform Intensity

(a) Benchmark Specification

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
Land Rentals (d_z)	-0.124	-0.112	-0.074
	(0.024)	(0.023)	(0.015)
Observations	4,712	4,712	4,712
R^2	0.40	0.63	0.74

(b) Quantile Specification

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
ψ_{Q1}	-0.012	-0.027	-0.014
	(0.010)	(0.010)	(0.008)
ψ_{Q2}	-0.097	-0.083	-0.048
	(0.013)	(0.011)	(0.008)
ψ_{Q3}	-0.131	-0.161	-0.108
	(0.016)	(0.016)	(0.012)
ψ_{Q4}	-0.229	-0.180	-0.128
	(0.048)	(0.046)	(0.031)

Notes: Results of Regression (9) in panel (a) and of Regression (10) in panel (b) for the following measures of farm-level misallocation: (a) efficiency gain $|\log(y^e_{izt}/y^a_{izt})|$, where y^e_{izt} is efficient output of farm i in a zone-level efficient reallocation and y^a_{izt} is actual output in the data, (b) marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\text{MPLa}_{izt}/\overline{\text{MPLa}}_{zt})|$, (c) revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\text{TFPR}_{izt}/\overline{\text{TFPR}}_{zt})|$. Standard deviations are in the parentheses. Panel data are simulated by the model calibrated to the Ethiopia ISA 2013/14 data.

a significant average effect on efficiency gains of -0.124. Notice that this model-generated effect is not significantly different from the estimated empirical average effect on efficiency gains of -0.132 reported in Section 4.1.1. Similar insights are obtained using relative MPLa and TFPR with significant but somewhat lower effects in the model than their empirical counterparts.

The results for the quantile specification are in panel (b) of Table 9. Recall that the quantiles are specified according to the distance from optimal operational scale. Focusing on the land market effects on efficiency gains, we find that the policy experiment on the model shows the presence of nonlinear effects of land markets that are larger the farther farms are from their optimal operational scale. This was also the case in our empirical estimates in Section 4.1.2. In particular, we do not find significant effects of land markets for farms that are already close to their optimal operational scale (first quantile). The effects grow in size and significance over quantiles as in the empirical counterpart. Indeed, the quantile model-generated effects are not significantly different from their empirical counterparts except for the second quantile. We find similar insights focusing on relative MPLa and TFPR with significant average effects and the same presence of nonlinear effects in the quantile specification.

Using the same back of the envelope calculation that we applied to our empirical results 4.1.3, we find that the model-generated data implies the following land market effects: one percentage point increase in rentals increase aggregate productivity by 12.2/5.1=2.4 percent. That is, the model generated effects are approximately three quarters (75 percent) of that in the empirical counterpart. In this context, notice that the model generated effects are entirely driven by our policy experiment, a change in χ_z that replicates the actual change in land rentals. In the context of the model is an unexpected and exogenous change in χ_z. In this manner, unlike the empirical assessment, the model-based assessment is not tainted by the potential endogeneity issues of the policy or other potential reasons behind the changes in allocative efficiency that follow the reform.

We have also reconducted our analysis on the effects of land reform adoption using unreformed and early reformers as control groups, and late reformers as treated zones, analogously to our empirical assessment in Section 4.2. Again, because we use the same definition of control and treatment groups in model and data, the size of the sample in the model decreases as much as the size of the sample in the data for this difference-in-difference strategy. Recall that this implies using model-generated data for barely 332 households when the control zones are defined as the unreformed zones and the late reformer are the treated zones. We find point estimate effects that, although they are sizeable (-0.080 for efficiency gains, -0.050 for MPLA, and -0.031 for TFPR), they are smaller and less significant than the effects of increasing reform intensity, as it was the case in the empirical assessment in Section 4.2. We believe the that this smaller significance, in both the model estimates and empirical estimates, is partly driven by the small size of this difference-in-difference sample.[27]

Finally, the effects across the maturity of the reform using model-generated data are very similar to their empirical counterpart in Section 4.3. Specifically, we run our difference-in-difference specification in equation (13). In the context of the model we find that the effects of rental markets decrease as the reform advances and land rentals increase. In the case of efficiency gains, we find that the first stage of the reform ($R_z \in [0, 5]$) significantly reduces

[27]Interestingly, although not reported it, the land market effects that arise from model-generated data are not significant using asymptotic standard errors, while they become significant using block-bootstrap standard errors. Notice that either way of computing standard errors does not change our point estimates.

misallocation with a point estimate of -0.134. The effect is larger in the second stage of the reform ($R_z \in (5, 10]$) with a significant point estimate of -0.151. Finally, the in the last stage of the reform ($R_z \in (10, 100]$) the effects are lower with a point estimate of -0.079. Similar insights arise using relative MPLa and TFPR. Again, these effects estimated using model-generated data resemble their empirical counterpart.

5.4 The Effects of Land Markets on Inequality

Even if the effects of land markets imply higher efficiency in resource allocation and productivity, a common and important concern for policy makers is that opening land markets might result in higher inequality (Deininger and Binswanger, 1999; Deininger and Feder, 2001).[28] The idea is that land markets might put plenty of land ownership and, hence, farm income in the hands of few highly productive farms. We now assess the effects of land markets on inequality using the same large-scale land reform in Ethiopia.

The assessment of the effects of land markets on inequality is challenging in terms of data requirements. First, the assessment requires data on both land rental payments paid and received by each farm. Second, the assessment also requires that the sum of rental payments paid by those that rent in land be identical to the total receipts from renting out land, which can also be an important constraint for non-administrative survey data. Unfortunately, although the Ethiopia ISA collects the payments paid by farmers that rent in land, it does not collect the data on income generated from renting out land, which unambiguously limits the empirical assessment on inequality. Fortunately, our model-generated *status quo* and *counterfactual* allocations resulting from our policy experiment (Section 5.3) satisfy the

[28] See also André and Platteau (1998) and Otsuka (2007).

Table 10: Effects of Land Rental Markets on Inequality

Dependent variable:	Zone-Level Inequality Measure			
	Variance of Logs	Gini Index	90-10 Ratio	75-25 Ratio
Land Rentals, ψ	-0.067	-0.004	-0.085	-0.043
	(0.032)	(0.002)	(0.037)	(0.019)
Observations	138	138	138	138
R^2	0.96	0.99	0.98	0.99

Notes: We calculate the zone-level variance of logged farm income, Gini index of farm income, the 90-10 ratio of log farm income, and the 95-75 ratio. We then estimate the impact of rentals specified as Equation (15) at the zone level. Standard deviations are in the parentheses. In our estimation we use the model-generated panel data from the policy experiment described in Section 5.3 that is calibrated to match the *status quo* level of land rentals in Ethiopia 2013/14 and its change between 2013/14 and 2015/16 in the *counterfactual*. Notice that the R^2 tends to be large simply because we control for zone fixed effects.

data requirements. As a result, we now conduct an assessment of land markets on inequality using our quantitative framework and recall that our model has been externally validated by finding model-generated effects of land rental markets on resource allocation and productivity (Section 5.3) that are the bulk of the estimated empirical effects (Section 4).

To assess the effects of land markets on inequality we construct measures of within-zone inequality for farming income separately in the *status quo* and *counterfactual* scenarios. We use the definition of farm income posed in our theoretical framework (Section 3). That is, farming income is the sum of farming production (value added) minus capital factor payments and the land rental costs $c(\bar{l}, l)$, see equation (2). Notice that the land rental costs incorporates the possibility of non-negative income generated from renting out land, see equation (3). We then run the following difference-in-difference specification on our measures of farm income inequality:

$$\text{Inequality}_{zt} = \alpha_z + \lambda_t + \psi d_{zt} + \varepsilon_{zt}, \tag{15}$$

where the treatment zones are defined as those for which there is an increase in reform intensity (as in Section 4.1). Table 10 shows the results for four different measures of inequality: the variance of *logged* farm income, the Gini index, the 90-10 ratio, and the 75-25 ratio. Across all inequality measures we find the same message. An increase in land rental market activity reduces zone-level inequality.

6 Further Insights

Although the reduction in misallocation due to land rentals is economically substantial, not all rentals necessarily imply larger efficiency. In Section 6.1, we explicitly explore the role of land rentals that operate through the formal market versus informal land rentals that operate through personal connections or networks, such as through relatives and friends. In Section 6.2, we further investigate the impact of land rental markets on the adoption of new technologies such as fertilizers, tractor and animals.

6.1 Formal versus Informal Land Rental Markets

A nice feature of our data is that detailed information on the land rental arrangements is available. This information helps in attributing a land rental to either the formal market or the informal market. In particular, the survey data includes information about both the rental contract—that stipulates the rental payments agreed before cultivation between the renter and the rentier—and the actual rental payments paid after harvest. Clearly, the amounts stipulated in the rental contract and the actual payments paid after harvest do not need to be the same. For example, households might not honor their contractual arrangements and partially or fully default ex-post, for example, in the event of a weak harvest.

We also have information about whom the land is rented from (e.g., relative, friends, etc.). Indeed, the vast majority of land rentals occur between relatives and friends: among the households that rent in land 82 percent rent land from relatives (46 percent) and friends (36 percent). This suggests that personal connections may be a key determinant of rental transactions. If this is the case, then reallocations through rentals may not necessarily be efficient in directing resources to their best uses because they may obey other goals such as redistribution or the provision of social insurance which can be related to household proximity in kin (Kinnan and Townsend, 2012) or social stratification (Munshi and Rosenzweig, 2016).

Since land rentals from relatives and friends are not necessarily informal, we use rental payments stipulated between renter and rentier in the rental contract to distinguish between formal and informal land rentals. The rental contract is basically defined by the arranged rental price for a given plot and period of time (typically, for a single rainy season) before cultivation. Our focus on the rental payments specified in the contract is motivated by the notion that the actual post-harvest payments can be affected by default or renegotiation. Then, the idea is that if a rental contract specifies a plot to be rented for free, then it is likely that this land rental is not market-based and that other considerations are at play. Following this idea, we define informal land rentals as those that are estipulated to be for free (zero rental payments) in the rental contract and formal rentals as those for which the rental contract estipulates a non-zero rental payment.[29] Notice that our definition of formal

[29]Extending the definition of informal rentals as those with small nominal payments agreed in the rental contract delivers similar results.

Table 11: Effects of Formal versus Informal Land Rental Markets

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
Formal Rentals (d_{mz})	-0.140	-0.207	-0.090
	(0.034)	(0.032)	(0.031)
Informal Rentals (d_{nz})	-0.004	-0.053	-0.054
	(0.033)	(0.031)	(0.031)
Observations	4,712	4,712	4,712
R^2	0.23	0.13	0.16

Notes: Results of econometric specification (16) with the following measures of farm-level misallocation: (a) efficiency gain $|\log(y^e_{izt}/y^a_{izt})|$ where y^e_{izt} is efficient output a farm i of a zone-level efficient reallocation and y^a_{izt} is actual output in the data, (b) marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\text{MPLa}_{izt}/\overline{\text{MPLa}}_{zt})|$, (c) revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\text{TFPR}_{izt}/\overline{\text{TFPR}}_{zt})|$. Standard deviations are in the parentheses. Panel data for waves Ethiopia ISA 2013/14 and 2015/16 described in Section 2.

rentals—which adds estipulated payments in cash and in kind—includes sharecropping contracts (Shaban, 1987; Sadoulet et al., 1997; Burchardi et al., 2018) as long as the ex-ante agreed amount of shared crops between the renter and the rentier is nonzero—independently of the proximity in kin or social stratification.

Based on this definition of informal and formal land rentals, we construct the indicator variable d_{mzt} to denote an increase of market land rentals of a zone. That is, we set $d_{mzt} = 1$ if formal rentals increase in a zone and $d_{mzt} = 0$ otherwise. Similarly, we construct the indicator variable d_{nzt} to denote an increase of non-market land rentals of a zone. Using these two dummies in our benchmark specification (9) controls for the effect of non-market rentals:

$$|\log e_{izt}| = \alpha_z + \lambda_t + \psi_m d_{mzt} + \psi_n d_{nzt} + \beta \log \text{TFP}_{iz} + \varepsilon_{izt}. \tag{16}$$

We find that, in all three measures of farm-level misallocation, the effects of formal rentals on misallocation are large and significant, see Table 11. The effect of formal rentals, captured by ψ_m, is approximately twenty-five percent larger than our benchmark panel results in Section 4. In contrast, the effect of informal rentals, which is captured by the coefficient ψ_n, is not only smaller but also not significant.

We conclude that reductions in the extent of misallocation due to changes in operational scale via rentals mostly operate through market forces and not through informal land rentals. That is, the effectiveness of rentals in allocating resources is still limited by other aspects of the institutional environment that are behind the use of informal rental markets.

6.2 The Effects of Land Markets on Technology Adoption

Ethiopia is a country at a preliminary stage of development and this reflects on the low levels of technology adoption. In particular, we find that only 4.8 percent of farmers use tractors (either owned or rented), 51.2 percent use fertilizers, and 61.5 percent use livestock in agricultural production.[30]

We separately investigate the effects of land rental markets on the extensive and intensive margins of technology adoption such as fertilizers use, livestock, and tractors.[31] First, we explore the extensive margin by positing a probit regression among households, using an indicator for the adoption (or not) of a given technology. We estimate this specification separately for fertilizers, livestock, and tractors. Specifically, let us denote the indicator of using a given technology as $\mathbf{f}$: $\mathbf{f}_i = 1$ indicates that household i uses any positive amount of

[30]This represents lows levels of technology adoption. See Yang and Zhu (2013) for a study of the modernization in agriculture and long-term growth.

[31]The Ethiopia ISA data about the use of capital (livestock and tractors) is restricted to land preparation activities. Land preparation lends itself to the adoption of capital as a substitute for labor since it is power intensive but not control intensive (Pingali, 2007).

a given technology. To help illustrate the problem, consider the following equivalent latent variable model. Suppose there exists an auxiliary random variable $\mathbf{f}^*$ specified as

$$\mathbf{f}_i^* = \alpha + \psi_m d_{mi} + \psi_n d_{ni} + \beta \log \mathrm{TFP}_i + \gamma \left| \log \left(\frac{\mathrm{TFPR}_i}{\overline{\mathrm{TFPR}}} \right) \right| + \varepsilon_i. \quad (17)$$

We can view $\mathbf{f}$ as an indicator for whether this latent variable is positive: $\mathbf{f} = 1$ if $\mathbf{f}^* > 0$. In this regression, d_{mi} is an indicator of whether or not farmer i rents any positive amount of land through the formal market and d_{ni} is an indicator of whether or not farmer i rents in land through the informal market (see previous definitions in Section 6.1). Our key parameters of interest are then ψ_m and ψ_n. We also control for farm TFP (TFP_i) and for farm TFPR (relative to the economy-wide average), which is as a summary measure of farm misallocation. Intuitively, higher farm TFP facilitates the adoption of better technology, while higher farm-level distortions reduce the return to adopting it.

The results are in Table 12, panel (a). Farms with formal land rentals are more likely to use fertilizers in agricultural production than farms without rentals. This association, summarized by the estimate for ψ_m, is large and significant. Specifically, consider a farm of average TFP and TFPR; our estimate implies that such a farm is 14.3 percent more likely to use fertilizer if it operates with rented land.[32] In contrast, land rentals are not associated with the use of fertilizers when renting is done through non-market rentals as ψ_n is small and not significantly different from zero. Similar insights arise when looking at the probability of using livestock in agricultural production. That is, market rentals significantly increase the probability of using livestock, while non-market rentals have little effect. The case of

[32] The probability of 14.3 percent is computed using our estimated ψ_m from probit Regression (17) reported in Table 12, panel (a), evaluated at the mean.

Table 12: Effects of Land Rental Markets on Technology Adoption

(a) Extensive Margin: Probit Specification

	Fertilizers	Livestock	Tractors
Market Rentals (d_{mi})	0.421	0.617	-0.110
	(0.064)	(0.075)	(0.128)
Non-Market Rentals (d_{ni})	0.040	0.051	-0.110
	(0.129)	(0.085)	(0.164)
Observations	2,357	2,356	2,356
Pseudo R^2	0.02	0.10	0.00
Δ Prob. (Formal Rentals) (%)	14.4	20.4	-0.8
Δ Prob. (Informal Rentals) (%)	1.5	1.9	-0.8

(b) Intensive Margin: DID Specification

	Fertilizers	Livestock	Capital
Market Rentals (d_{mz})	0.249	-0.050	-0.039
	(0.086)	(0.077)	(0.079)
Non-Market Rentals (d_{nz})	-0.142	0.037	0.107
	(0.090)	(0.075)	(0.078)
Observations	2,459	2,241	4,712
R^2	0.31	0.26	0.41

Notes: Results of probit specification (17) in panel (a) and of difference-in-difference specification (18) in panel (b). Each specification estimated for different measures of technology adoption: fertilizer use, livestock use in agricultural production per unit of labor, and tractors per unit of labor. For the difference-in-difference specification, we use capital per unit of labor (as described in Section 2.1). Standard deviations are in parentheses. Panel (a) uses the Ethiopia ISA 2013/14 data, and panel (b) uses the panel Ethiopia ISA 2013/14 and 2015/16 described in Section 2.

tractors, however, is different since land rentals are not significantly associated with a higher probability of using tractors in agricultural production. We come back to this result below. Moreover, although not reported in Table 12, the estimation of the other parameters confirm our conjecture that farm TFP is positively associated with fertilizer use, while farm-level distortions are negatively associated with it.

Exploiting the panel dimension of our data set to estimate the effects of land rentals on the intensive margin of technology adoption, we focus on households that have already adopted the technology in the 2013/14 wave, and discuss how land rentals affect the intensity of technology use in the 2015/16 wave. We consider the following difference-in-difference specification, which is analogous to equation (16),

$$\log \tilde{f}_{izt} = \alpha_z + \lambda_t + \psi_m d_{mzt} + \psi_n d_{nzt} + \beta \log \mathrm{TFP}_{iz} + \gamma \left| \log \left(\frac{\mathrm{TFPR}_{izt}}{\overline{\mathrm{TFPR}}} \right) \right| + \varepsilon_{izt}. \qquad (18)$$

where α_z is zone fixed effect, λ_t is year fixed effect, and d_{mzt} is an indicator for market land rental increases across waves in zone z, and d_{nzt} is an indicator for non-market land rental increases across waves in zone z. We also control for farm TFP and farm TFPR as a summary measure of farm misallocation.

Our results are shown in Table 12, panel (b). We find that an increase in land rentals generates an increase in fertilizer use intensity, with a large and significant estimate $\psi_m = 0.249$. That is, our results suggest that the take up rate on fertilizer use increases with land markets—which also brings the operational scale of farms closer to optimum.[33] Notice that

[33]This result potentially alleviates the puzzling low take-up rate of fertilizers in poor countries emphasized by Duflo et al. (2011) if one entertains the possibility that without land transactions—or without the possibility of changing the amount of farm land—there are no incentives to take on fertilizers; an argument that needs further study.

this positive effect of land rental markets on fertilizers is consistent with the cross-sectional probit results. In contrast, the effects of land rentals on agricultural capital or on livestock used in agricultural production are not significant along the intensive margin. These results can be partly explained by short-term rental contracts and small plot size. Rental contracts can still be of very short term in nature in Ethiopia partly due to various restrictions imposed by local government (Deininger et al., 2008) which can create disincentives for long-term investments (Goldstein and Udry, 2008). In addition, while fertilizers can boost agricultural productivity almost independently of the size of the cultivated plot, this is not the case for large animals, tractors, and other sizeable capital which, unless rented on a daily or hourly basis, are more likely to pay off in large operational scales (e.g., Chen, 2019). Indeed, the average farm size remains extremely small (a bit more than one hectare per household), and is largely unaffected even after the reform.

7 Robustness and Extensions

We provide a set of robustness checks and discuss some extensions. First, we discuss a potentially important qualification of our findings with regards to the presence of output market distortions. Second, we extend our analysis adding potential misallocation of the labor input. Third, we study the role of crop composition on the extent of misallocation. Fourth, we provide some additional discussion on potential measurement issues.

7.1 Output Market Distortions

It is important to recognize that other distortions, in addition to distortions on factor inputs, may be in place. Our emphasis has been on connecting misallocation with restrictions to land markets in Ethiopia, as well as on establishing a causal link from land rentals to misallocation. Even if Ethiopia's land certification reforms have been successful in providing tenure security as their primary objective, we have documented that strong restrictions in rentals remain in place and that, even if not everywhere enforced, rental activity remains tenuous. However, to the extent that there may be other frictions in the economy—such as poor infrastructure which would make markets in remote rural locations difficult to access—that may be driving the misallocation we document, it is relevant to assess the extent to which the land market is the dominant source of misallocation in the data as opposed to other frictions.

To this effect, we exploit the availability of data on farm distance to markets as a proxy for other frictions such as product market distortions and assess the extent to which these variables are related to farm-specific measures of distortions. In particular, we extend our benchmark difference-in-difference specification (9) to include farm distance to nearest market denoted by m_i as an additional control variable. This implies the following specification:

$$|\log e_{izt}| = \alpha_z + \lambda_t + \psi d_{zt} + \beta \log \mathrm{TFP}_{iz} + \gamma \log \ m_{iz} \ + \varepsilon_{izt}.$$

We find that controlling for output market distortions does not alter our benchmark results. The estimated coefficient ψ (and standard errors) for efficiency gains, TFPR, and MPLa barely change, with -0.132 (0.033), -0.151 (0.030), and -0.192 (0.031), respectively. The coefficients on log distance for dependent variables of efficiency gains, TFPR, and MPLa,

Table 13: Effects of Land Rental Markets on Misallocation and Productivity with Labor Input

(a) Benchmark Specification

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
Land Rentals (d_z)	-0.084	-0.168	-0.070
	(0.046)	(0.033)	(0.028)
Observations	4,716	4,716	4,716
R^2	0.16	0.16	0.13

(b) Quantile Specification

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
ψ_{Q1}	0.012	-0.129	-0.053
	(0.036)	(0.028)	(0.019)
ψ_{Q2}	-0.110	-0.078	-0.082
	(0.038)	(0.036)	(0.024)
ψ_{Q3}	-0.090	-0.113	-0.050
	(0.051)	(0.040)	(0.029)
ψ_{Q4}	-0.127	-0.386	-0.155
	(0.096)	(0.078)	(0.059)

Notes: Results of Regression (9) in panel (a) and of Regression (10) in panel (b) for the following measures of farm-level misallocation: (a) efficiency gain $|\log(y^e_{izt}/y^a_{izt})|$, where y^e_{izt} is efficient output of farm i in a zone-level efficient reallocation and y^a_{izt} is actual output in the data, (b) marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\text{MPLa}_{izt}/\overline{\text{MPLa}}_{zt})|$, (c) revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\text{TFPR}_{izt}/\overline{\text{TFPR}}_{zt})|$. Standard deviations are in the parentheses. Panel data for the Ethiopia ISA 2013/14 and 2015/16 waves are as described in Section 2. The results shown in this table are under the setup where labor enters production explicitly.

are not always significant with estimates of -0.030 (0.017), -0.019 (0.016), and -0.044 (0.016), respectively. These results should not be entirely surprising since we have shown that the bulk of misallocation occurs within narrow geographical areas that share similar market access.

7.2 Adding Labor Input

That the functioning of labor markets in poor countries is far from perfect is well-known (Rosenzweig, 1978, 1988; Rosenzweig and Wolpin, 1985; Behrman, 1999). So far, we have abstracted from labor in our analysis because most farm labor is family labor and hence have avoided the notion of splitting families in reallocation. In this section, we show that our results are robust to explicitly including labor in the production function. Recall that in our benchmark production function output (y_i) and inputs (k_i, l_i) are all normalized to labor input. Alternatively, we consider an expanded production function where we explicitly include the labor input:

$$y_i = s_i^{1-\gamma}(k_i^{\alpha} n_i^{\theta} l_i^{1-\alpha-\theta})^{\gamma}, \tag{19}$$

where n_i is labor input and $\theta\gamma$ is the corresponding factor share. In this case, the farm productivity can be calculated as $s_i = \left[\frac{y_i}{(k_i^{\alpha} n_i^{\theta} l_i^{1-\alpha-\theta})^{\gamma}}\right]^{\frac{1}{1-\gamma}}$, and the planner's solution requires $k_i^e = \frac{s_i}{\sum_i s_i}K, \quad n_i^e = \frac{s_i}{\sum_i s_i}N, \quad l_i^e = \frac{s_i}{\sum_i s_i}L$, where $N = \sum_i n_i$ denotes the aggregate labor endowment. The efficient aggregate output per zone is $Y^e = \sum_i y_i^e = \left(\sum_i s_i\right)^{1-\gamma}(K^{\alpha} N^{\theta} L^{1-\alpha-\theta})^{\gamma}$. Analogously, farm revenue productivity (TFPR) is now defined as $\text{TFPR}_i \equiv \frac{y_i}{k_i^{\alpha} n_i^{\theta} l_i^{1-\alpha-\theta}}$.

In this alternative specification, we have three parameters to calibrate: γ, α, θ. Note that the labor income share is now given by $1-\gamma+\theta\gamma$, where $1-\gamma$ is the profit of the farm and $\theta\gamma$ is the share of labor input. We therefore set $1-\gamma+\theta\gamma = 0.464$ to match the labor share of 0.464 as in our benchmark specification. Recall that family labor accounts for 75.3 percent of total farm labor. We then choose the first component $1-\gamma$ to be 75.3 percent of the total labor share, which means $\gamma = 0.651$. The capital share, $\alpha\gamma$ is 0.147, and hence we choose $\alpha = 0.202$.

Our results remain largely unchanged in this alternative specification. On average, the efficiency gain per zone is 1.83, compared to 1.66 in our benchmark case. The results of our difference-in-difference analysis when we add labor are in Table 13. We obtain similar results to our benchmark. Land rentals significantly reduce misallocation and the effects are again non-linear. To provide a quantitative interpretation of the size of efficiency gain, we can do the back-of-envelope calculation as in Section 4.1.3, and we find that one percentage more rentals reduce misallocation by 2.7 percent, compared to 3.2 percent that we found in Section 4.1.3.[34]

7.3 Crop Choice and Within-Crop Misallocation

Farmers in Ethiopia cultivate a variety of crops with maize, sorghum, and tea leaves being among the most produced crops by farms. Since our production function specification is common across farm households who may be producing different crops, differences in composition of production can generate dispersion in marginal products across farm households. To address this issue, we explore the extent of misallocation within each crop using our plot-level data.

The data records the crop cultivated in each plot operated by a household. We then focus on an individual crop indexed by c. We keep all land plots cultivating crop c, aggregate inputs and outputs of these plots to the household level, and then repeat the analysis in Section 5.1 to calculate the extent of misallocation and zone-level efficiency gain.

[34]Notice that in assessing the effects of land markets on resource allocation and productivity, we reallocate factor inputs (including labor) within zones. That is, we are not allowing for reallocation gains potentially generated from (internal) migration we think deserves further exploration. For such analysis in different contexts, see the recent work of Munshi and Rosenzweig (2016) for India and of Bryan and Morten (2019) for Indonesia. For a cross-country analysis, see Hendricks and Schoellman (2018).

Table 14 panel (a) reports the results for five different crops, which are the most widely cultivated in Ethiopia. We find that within crops, both the extent of misallocation measured by the dispersion in log MPLa or log TFPR, and the efficiency gain from reallocation are fairly similar to our baseline farm aggregate. For instance, more than half of all farmers produce maize and, for this crop, the dispersion in log TFPR is 1.03 and the efficiency gain is 1.79-fold (compared with 0.84 and 1.66-fold in our baseline).

Table 14: Crop-Level Analysis

(a) Misallocation within Crops

Crop	Number of Farms (%)	Cultivated Land (%)	Efficiency Gain within Zones	Dispersion in MPLa_i within Zones	Dispersion in TFPR_i within Zones
Maize	56.6	17.5	1.79	1.02	1.03
Sorghum	42.7	18.6	1.74	0.84	0.88
Tea Leaves	40.2	13.5	1.51	0.84	0.86
Coffee	29.3	16.6	2.16	1.13	1.12
Wheat	25.2	8.7	1.61	0.91	0.94

(b) Effects of Land Rental Markets on Crop Choice

Dependent variable:	Fraction of Cash Crops		Kinds of Crops
	Definition 1	Definition 2	
Land Rentals (d_z)	-0.027	-0.000	0.232
	(0.016)	(0.007)	(0.163)
Observations	4,714	4,714	4,714
R^2	0.34	0.14	0.35

Notes: Panel (a) lists the five most common crops in Ethiopia. Column 1 reports the percentage of household farms cultivating at least one plot with a particular crop. Column 2 reports the percentage of land used to cultivate a given crop. The last three columns report the efficiency gains and the dispersion of MPLa and TFPR within zones and then weighted by zone-level output, as defined in Section 5.1, when we focus only on farm plots of a single crop. Data for the Ethiopia ISA 2013/14. Panel (b) reports the results of Regression (20) for the following left-hand-side variables: fraction of farm-level output that is food crops, in two versions of definitions, and the number of different crops. Standard deviations are in the parentheses. Panel data for the Ethiopia ISA 2013/14 and 2015/16 waves are as described in Section 2.

We also explore if rentals affect the choice of crops. In particular, we want to study whether more rentals cause farmers to cultivate more cash crops. We hence divide crops into two kinds: food crops and cash crops. We use two versions of definition of food crops. In the first definition, we define maize, sorghum, wheat, rice, barley, millet, and oaks to be food crops, while all others are cash ones. The second definition, we are more strict in cash crops that only tobacco, sunflower, sesame, rapeseed, soya beans, and onion are defined as cash crops, which are not directly used for food consumption, while others are considered as food crops. We then calculate for each household the fraction of total output that is in food crops, denoted as $frac_{izt}$, and then run the following difference-in-difference regressions among households:

$$frac_{izt} = \alpha_z + \lambda_t + \psi d_{zt} + \beta \log \mathrm{TFP}_{iz} + \varepsilon_{izt}. \tag{20}$$

The results are in panel (b) of Table 14. We can see that rentals have little impact on farm crop choices. We also repeat our regression using the number of crop kinds of each household farm on the left hand side of the regression and we obtain similar results.

7.4 Further Measurement Issues

An important concern in the literature that measures the extent of misallocation is the possibility of measurement error in individual inputs and outputs driving dispersion in the TFP and marginal products of production units. It is therefore relevant to recognize this measurement issue in agricultural production and TFP, and at the same time, recognize the difficulties in fully addressing it. In the context our exercise, two remarks are in order regarding the potential role of mismeasurement on the estimated effects of land markets.

Theoretical versus empirical effects. First, an important aspect of our analysis is that we have showed that the model-generated difference-in-difference effects of land markets on resource allocation and productivity (Section 5.3) match the empirical difference-in-difference effects (Section 4) quite well without targeting them. This result provides confidence that the effects of land markets on productivity are not simply the result of measurement error because our theoretical framework does not *per se* generate measurement error. Specifically, the effects measured through our theoretical framework are the result of a controlled experiment that solely changes χ_z from a calibrated *status quo* level of land rentals to a *counterfactual* level of land rentals. This controlled experiment leaves no scope for other reasons—including measurement error—to surface in the explanation of our model-generated difference-in-difference effects.

The extent of versus changes in misallocation. Second, a recurrent concern in the literature that measures the extent of misallocation is that the estimated permanent farm productivity (and, hence, its dispersion) could be subject to measurement error stemming from factor input and/or output mismeasurement in agriculture. Although this issue is potentially relevant to quantify the extent of misallocation (Restuccia and Santaeulàlia-Llopis, 2017; Gollin and Udry, 2017), it is important to highlight that our main focus is not on the extent of misallocation but on its changes across zones and across time due to changes in land rental market activity.[35] Therefore, while the level of dispersion in farm

[35] See also De Magalhães and Santaeulàlia-Llopis (2018) for a set of specific issues related to the measurement agricultural production in ISA data.

Table 15: Alternative Farm-Level TFP Measures

Farm-level TFP Proxies, s_i:	Efficiency Gain	Rank Corr. with Baseline s_i	Observations
(1) Benchmark $\widetilde{s}_i = (s_i^{2013} s_i^{2015})^{\frac{1}{2}}$	1.66	—	2,921
(2) $\widetilde{s}_i^1 = ((\Pi_j s_{ij}^{2013})^{\frac{1}{J}}, (\Pi_j s_{ij}^{2015})^{\frac{1}{J}})^{\frac{1}{2}}$	1.70	0.71	2,921
(3) $\widetilde{s}_i^2 = (\text{median}_j(s_{ij}^{2013}), \text{median}_j(s_{ij}^{2015}))^{\frac{1}{2}}$	1.70	0.71	2,921
(4) $\widetilde{s}_i^3 = (\max_j^2\{s_{ij}^{2013}\}, \max_j^2\{s_{ij}^{2015}\})^{\frac{1}{2}}$	1.71	0.56	2,921

Notes: For each farm household, we construct four alternative measures of productivity based on the panel data of plot-level productivity of operated plots: the geometric mean, median, and second largest value, in addition to the geometric mean of farm productivity between the 2013/14 and 2015/16 waves. The number of observations differ slightly across cases because we apply a consistent trimming of the farm-TFP distribution as described in Section 3.

productivity may reflect some classical measurement error, the changes in the dispersion of farm productivity over time and across differing zones (treatment and control) are less likely to do so. The main reason for that being that we keep permanent farm productivity—a fixed effect that captures unobserved heterogeneity—constant at *status quo* across periods in our policy evaluation in both the empirical and the theoretical analysis. This, however, opens the question on whether the estimated effects of land rentals depends on the *status quo* measurement of farm-level TFP. For that reason, we re-conduct our analysis using a set of alternative measures of permanent farm-level TFP next.

The effects of land markets with alternative measures of farm-level TFP. Recall that our basic unit of production is the farm household. Since a household generally operates a farm spanning several plots of land in the data—an average of more than seven plots in the case of Ethiopia—we aggregate both factor inputs and outputs at the household-farm level to calculate farm-level TFP. An important advantage of our aggregation at the farm level is that potential unmeasured plot-level shocks and classical measurement error on inputs and outputs are mitigated.[36] However, at the same time, our aggregation has the caveat that it abstracts from potentially genuine efficiency gains from within-household factor input reallocations across plots.[37] That is, although our aggregation reduces the impact of measurement error, at the same time, it leaves out potentially true variation that could be relevant to measure misallocation. Our benchmark measure of farm-level productivity is the permanent component—a fixed effect that captures unobserved heterogeneity—of TFP computed using our panel data using the production function discussed in Section 3.

We now construct a set of alternative proxies to measure permanent farm-level TFP constructed from plot-level data:

$$s_i^1 = (\Pi_j s_{ij})^{\frac{1}{J}}, \quad s_i^2 = \text{median}_j(s_{ij}), \quad s_i^3 = \max_j^2\{s_{ij}\}. \tag{21}$$

where s_{ij} is the plot-level TFP computed for household-farm i and plot j. The first measure of household-farm TFP, s_i^1, is the geometric mean of plot-level productivity (across all plots, J); the second measure s_i^2 uses the median of plot-level productivity; the third measure s_i^3 uses the second highest value. As it was the case of our benchmark measure, these alternative measures in equation (21) are based on the assumption that a household should have the same productivity across plots. That is, the actual variation in plot-level productivity within a household is entirely attributed to measurement error. A potential caveat of this identification assumption is that some variation in plot-level productivity can reflect actual

[36] We discuss this further in our Appendix G

[37] For example, this could be either because there is heterogeneity in the diminishing returns on the number of plots an individual can manage or because there is more than one manager (e.g., husbands and wives) per household—which is not uncommon in Ethiopia and other Sub-Saharan Africa countries—with potentially different managerial ability.

Table 16: Effects of Land Rental Markets: Alternative Farm-Level TFP Proxies

(a) Farm-Level TFP Proxy = Geometric Mean of Plot-Level TFP

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
Land Rentals (d_z)	-0.151	-0.161	-0.156
	(0.037)	(0.035)	(0.034)
Observations	4,704	4,704	4,704
R^2	0.34	0.10	0.09

(b) Farm-Level TFP Proxy = Median Plot-Level TFP

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
Land Rentals (d_z)	-0.156	-0.173	-0.178
	(0.037)	(0.035)	(0.035)
Observations	4,694	4,694	4,694
R^2	0.37	0.11	0.09

(c) Farm-Level TFP Proxy = Second Highest Plot-Level TFP

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
Land Rentals (d_z)	-0.093	-0.162	-0.168
	(0.038)	(0.037)	(0.037)
Observations	4,688	4,688	4,688
R^2	0.42	0.07	0.14

Notes: Results of Regression (9) for three proxies of farm-level TFP: the geometric mean of plot-level TFP in panel (a); the median plot-level TFP in panel (b); and the second highest plot-level TFP in panel (c). For each of these three proxies we use the following measures of farm-level misallocation: the efficiency gain $|\log(y^e_{izt}/y^a_{izt})|$, where y^e_{izt} is efficient output of farm i in a zone-level efficient reallocation and y^a_{izt} is actual output in the data, the marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\text{MPLa}_{izt}/\overline{\text{MPLa}}_{zt})|$, and the revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\text{TFPR}_{izt}/\overline{\text{TFPR}}_{zt})|$. Standard deviations are in the parentheses. Panel data for the Ethiopia ISA 2013/14 and 2015/16 waves are as described in Section 2.

within-household misallocation. In any case, we take the geometric mean, the median, or the second highest value of plot-level productivity to mitigate potential measurement error.

Do the alternative estimates of permanent farm productivity imply different measurements of the extend of misallocation? We find that this is not the case. Using the alternative farm-level productivity measures in equation (21) we find within-zone efficiency gains that are very similar to our benchmark results, see Table 15. The (geometric) average produces an efficiency gain of 1.70-fold, which is slightly larger than our benchmark specification of 1.66-fold. The median yields an efficiency gain of 1.70-fold, and the second max of 1.71-fold. The Spearman's ranking correlation between the alternative measures of productivity and our baseline farm-level productivity are high: 0.71 for the (geometric) average plot, 0.71 for the median, and 0.56 for the second max.

Most importantly for our study, do the alternative estimates of permanent farm productivity change the estimated effects of land rentals on resource allocation and productivity? We find that this is not the case. Table 16 shows the results of implementing our benchmark difference-in-difference strategy in equation (9) for the three proxies of farm-level TFP in equation (21): the geometric mean of plot-level TFP in panel (a); the median plot-level TFP in panel (b); and the second highest plot-level TFP in panel (c). We find that across all these proxies of farm-level TFP the effects of land markets on misallocation resemble our benchmark results in Table 3. This is the case for all three measures of misallocation: efficiency gains, relative MPLa and relative TFPR.

Land quality differences between owned and rented land. Finally, another concern is whether there are any land quality differences between rented and non-rented land. Notice that we control for land quality throughout our analysis so our computations of efficiency gains already incorporate direct data on land quality on eleven dimensions (rainfall, slope, elevation, terrain roughness, nutrient availability, nutrient retention capacity, rooting conditions, oxygen availability to roots, excess salts, toxicity, and workability). However, we are interested in the possibility that rented land is of higher quality. This might have implications for the effects of land rentals on aggregate productivity. We assess this possibility using our plot-level data. We construct land quality at the plot level in the same way we constructed land quality at the household level in Appendix A and then compare land quality between rented and non-rented plots. The Welch's t-test shows that land quality q in cultivated rented plots and in non-rented plots is not statistically significant from each other.

8 Conclusions

We show that land rentals provide a useful mechanism to overcome imbalances between the allocation of land-use rights and the efficient operational scale of farms. Our assessment is based on a large-scale land reform in Ethiopia that grants households the right to rent land. The context is relevant because land sales are prohibited by law, and land-use rights are ex-ante distributed among rural farm households in a fairly egalitarian basis. Hence, land rentals are the only channel that allow for the reallocation of farms' operational scale.

Our main finding is that land rentals substantially reduce resource misallocation and increase agricultural productivity. Our evidence builds on a difference-in-difference approach that exploits policy-driven variation in land rental market activity across space and time, an IV strategy, and a calibrated macroeconomic model with heterogeneous household farms that quantitatively replicates the empirical effects. In addition to the positive effects of land markets on resource allocation and productivity, we find that these effects are accompanied by a reduction in inequality, which provides an important dimension for the assessment of land policy and its implementation.

Despite the strong positive effects of land rentals on resource allocation and agricultural productivity, land markets are still underdeveloped in Ethiopia. The limited use of land rentals can arise from various frictions which may include restrictions on other factor inputs, remaining imperfections in land markets (e.g., purchases and sales are prohibited) or weak legal institutions that limit the credibility of the land reform. In particular, we argue that the fact that most land rentals occur through the informal market—among relatives and friends—with less effect on improving resource allocation and productivity are indicative of imperfections in the institutional environment which deserve further investigation.

Finally, although our analysis strictly belongs to the context of a specific large-scale reform for Ethiopia, we do think that our results generally highlight the importance of land reforms in poor countries that specifically address the tradability of the land through rentals to promote better resource allocation and not simply tenure security—which has been the main focus in most reform episodes. We hope that our work generates further research on the effects of land market activity and its limitations in other contexts.

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Online Appendix

A Ethiopia LSMS-ISA Data

Agricultural output. Farm output is recorded in physical quantities (kilograms) of different crops.[38] In the 2013/14 wave of survey, the most common crops in Ethiopia based on the percentage of households who produce it are maize (57 percent), sorghum (43 percent), tea leaves (40 percent), coffee (29 percent), and wheat (25 percent).[39] To aggregate farm production of different crops, we use common crop prices. For our purposes, the key is that aggregate production at the farm level reflects physical variation in output. Valuing output at common prices therefore allows us to compare output across farms, reflecting variation in quantities produced. Less important is what common price we use. Since we observe the prices of crops traded at local markets, we compute for each crop the median price among all transactions and use it as the common price of this crop. The value of the crop output of a farm is estimated by multiplying the physical quantity produced with its common price. We then sum up the values of all crop types produced by the farm to obtain the value of gross output of each farm. We also use common prices to estimate the value of intermediate inputs used by farms, such as fertilizers and seeds, in a similar way. Note that some fertilizers and seeds are from the farmers' home production; we evaluate these home-produced goods using common market prices as well. Again, the key in these assumptions is that the aggregate measure of intermediate inputs used on a farm tracks physical variation in inputs as best as possible. We calculate the value added of a farm by subtracting the value of intermediate inputs from the value of gross output. We use this measure of value added in our analysis as the net farm output.

Rain. To measure productivity, it is important to exclude transitory variation in output from value added. In agricultural production, the most important shock is precipitation. Rainfall information is provided in the data, recorded as the annual precipitation in millime-

[38] Some farmers may not have finished harvesting at the time of survey. In those cases, they report the percentage of harvest that is pending. We adjust for that to estimate their total harvest.

[39] We restrict our analysis to crops only and hence abstract from livestock as the production cycles of livestock are usually longer than one year, which is our data period.

ters, and we use it to identify shocks in rainfall. We create 10 dummies representing different levels of rainfall. Then, we regress the calculated farm value added on those dummies and obtain the residual of this regression as the value added excluding the transitory variation due to rainfall shocks. This is the measure of farm value added we ultimately use in our analysis.

Land. Land input of a farm (i.e., farm size) is the sum of the size of all land plots operated by this farm. In the 2013/14 wave, the size of 93.8 percent of land plots is accurately measured by GPS or, in case of small fields, by compass and rope at a precision of 0.1 square meters, while the size of the remaining land plots is reported by farmers.[40] Farms are in general very small in Ethiopia. The average farm size in our sample is around 1.3 hectares, compared to 169.2 hectares in the United States as reported in 2007 U.S. Census of Agriculture. The farm size distribution is skewed to very small sizes: 64.7 percent of households in our sample operate farms smaller than one hectare, 86 percent of households operate farms smaller than two hectares, and only two percent of households operate farms larger than five hectares. We note that a plot of land is treated as a part of a particular farm if it is operated by that farmer, regardless of whoever has the use rights of the land. In other words, the size of the farm is the operational scale and not the ownership or use rights of land. Therefore, when computing farm size, we include rented-in land plots and exclude rented-out plots for each household.

Land quality. The survey also records land quality and other geographical characteristics for each plot of land. For each plot, we have information on its elevation, slope, terrain roughness, nutrient availability, nutrient retention, rooting conditions, excess salts, toxicity, and workability. The issue is how to combine these measures of land characteristics into one aggregate measure of land quality. We regress log value added per labor hour on these variables of land quality, controlling for log capital and land input per labor hour. This regression estimates how each dimension of land quality affects farm value added per labor hour. Then, we take the coefficients from this regression to evaluate the land quality index q for each farm. This is an upper bound measure of land quality as some inputs may be

[40] Note that in this section, we describe sample statistics in the 2013/14 wave as examples, but they are similar overall in the 2015/16 wave.

correlated with the quality of the land and hence is conservative in our analysis of the extent of misallocation.

Capital. Farm capital has three components: agricultural tools, transportation tools, and some livestock. Agricultural tools include sickles, axes, pick axes, traditional or modern ploughs, and water pumps. We observe the physical quantity of these tools owned by each farmer, as well as their prices at local markets. Again, we construct common prices, defined as the median of sell prices, to evaluate these agricultural tools. Transportation tools include hand-pushed or animal-drawn carts and bicycles. The price of transportation tools are not directly available in the data, so we estimate their values using local prices from the internet.[41] The livestock used for agricultural crop production are a bit more complicated. The survey records the three most common livestock in Ethiopia, cattle, goats, and sheep, as well as their farm use. In our measure of capital, we only include cattle that are for agricultural or transportation purposes, and exclude goats and sheep, which are mainly used for meat, wool, or milk. We also observe the prices at which farmers sell their cattle. Given this, we construct common cattle prices separately for male and female cattle, to evaluate livestock value. Finally, we sum up the values of agricultural tools, transportation tools, and cattle as our measure of farm capital.[42]

Labor. The data provide labor input for every plot of land of a farm, in both the planting season and the harvest season. Labor input includes farmers' family labor, hired labor, and unpaid labor from other households. Family labor is recorded in hours (the data reports hours per day, days per week, and number of weeks per season); hired labor and unpaid labor, however, are only recorded in days. We assume that hired men work the same hours per day as male family members, and hired women and children work the same hours as female and children family members, respectively. Furthermore, labor quality of women and

[41] We assign the prices of transportation tools as follows: one hand-pushed cart is worth about 6 traditional ploughs; one animal-drawn cart is about 9 traditional ploughs; one bicycle is about 17 traditional ploughs. Note that very few farmers have these transportation tools, so excluding them in the measure of capital would only change our results slightly.

[42] To deal with a set of farmers who have zero measured capital but report cultivated land and positive production, we follow Adamopoulos et al. (2017) in imputing an amount of capital to all farms representing a common set of very small tools and structures used by farmers that are not recorded in the data. The amount we assign to each farmer is set to equal ten percent of the median capital-land ratio of farms within the zone, multiplied by the amount of land input of the farm. We have verified that our results are not sensitive to the size of adjusted capital or to dropping these households.

children maybe lower than that of men, reflected by their lower wage rates. We therefore adjust for the hour inputs of women and children downwards based on the median wage ratios relative to men to obtain the male-equivalent hours. We also assume that unpaid labor from other households work the same hours per day as hired workers of the same identity: for example, unpaid men work the same hours per day as hired men, and we apply the same quality adjustment as well. Finally, we construct farm labor input as the sum of hours from all three types of labor for all land plots of this farm in both seasons. We find that, out of total labor input, 75.3 percent is supplied by household members, 14.7 percent by hired labor, and 10.0 percent by unpaid labor from other households.

B Factor Income Shares

We document how we estimate factor shares using our Ethiopia data. The factor shares are calculated as the share of cost of each factor in production.

Labor share.—We observe the wage payments for hired labor, separately for male, female, and children. We then calculate the cost per day for these three types of labor by taking the median wage rate of each type. For household members and free labor from other households, we do not observe the cost. We hence impute the cost by assigning the same wage rate as hired labor of the same type. For example, we assume that using male household members has the same cost as using the same amount of hired male individuals. By doing this, we calculate the labor cost of each farm. We then take the ratio between this labor cost and the farm output (value added), and take the median (0.464) as labor income share.

Land share.—We observe the land payments, both cash and in-kind, for those rentals. Note that there is a substantial portion of rentals that are non-market as we described in Section 6.1. We therefore calculate land share using the portion of rentals that are market, i.e., the payment is not zero. The cost of land is then as the ratio between rental payments and rental size. We take the median of this to be our measure of land price. Then for all land plots, regardless of rented or own land, we apply this price to calculate the implied cost of land. We next aggregate this land cost to the farm level to obtain the shadow land cost of each farm, including both rented land and own land. Finally, we calculate the ratio between this implied land cost and the farm output (value added), and take the median (0.389) as land income share.

Capital.—We do not directly observe the capital cost. We therefore use the residual as capital share, which is $1 - 0.464 - 0.389 = 0.147$.

To summarize, we estimate that capital, labor, and land shares are 0.147, 0.464, and 0.389, respectively. Note that estimation of factor income shares in agriculture varies in the literature. Valentinyi and Herrendorf (2008) find that in the United States, the capital, labor, and land shares in agriculture are 0.36, 0.46, and 0.18, respectively. Restuccia and Santaeulàlia-Llopis (2017) use micro data from Malawi and estimate the capital, labor, and land shares to be 0.190, 0.419, and 0.391, respectively. This discrepancy may arise from the fact that Malawi has a lower level of mechanization in agriculture compared to the United States. In fact, Chen (2019) argues that the capital-output ratio (and therefore, the capital income share) in agriculture tends to increase as an economy develops. Ethiopia is typically considered to be at a stage of development similar to Malawi, and our estimated factor shares are also close to those of Restuccia and Santaeulàlia-Llopis (2017).

C Solution Algorithm and Calibration Strategy for χ_z

We describe the solution algorithm that we implement to solve for the model equilibrium allocations and the algorithm that we use in our calibration strategy to pin down χ_z by zone.

C.1 Solution Algorithm

Given a set of parameter values (α, γ, χ_z), and an initial joint distribution of land endowments, capital and permanent productivity $\Phi(s, \bar{l}, \bar{k})$:

1. Guess land and capital factor prices, respectively, q and r.

2. Solve the farm profit maximization problem (equation (2)). That is, find the farm-specific optimal demands of land and capital (l_i^*, k_i^*) that solve the first order conditions (equation (5)).

3. Check whether land and capital market clears for each zone, that is,

$$\sum_i l^*(s_i, \bar{l}_i) - \sum_i \bar{l}_i = 0$$
$$\sum_i k^*(s_i, \bar{l}_i) - \sum_i \bar{k}_i = 0.$$

4. If factor markets clear, then STOP. Otherwise, update the guess of factor prices (q, r) and GO TO step 2.

Notice that to generate the update in step 4 if the aggregate demand of land exceeds the aggregate supply of land, then we need to increase the rental price of land, q. Analogously for capital. Also, notice that there is no analytical solution to the first order condition in step 2. We numerically search for roots.

C.2 Calibration Strategy for χ_z

In Section 5.3, we describe our calibration strategy that aims at finding the institutional cost χ_z such that the model land rentals match the actual land rentals by zone. Precisely, we apply the following calibration algorithm.

1. Guess the institutional cost by zone, χ_z.

2. Solve the model by zone, applying the solution algorithm in Section C.1.

3. Compute the model-generated land rentals as equation (6) by zone.

4. If the model-generated rentals by zone equate their data counterparts, then STOP. Otherwise, update guess of χ_z and GO TO step 2.

To generate the update in step 4. if the model-generated rentals are smaller (larger) than their data counterparts, then we need to decrease (increase) the institutional cost χ_z. Notice that this procedure is computationally intense and that, further, we need to follow it separately for each zone. To ease the computational burden we parallelize our computation with 56 CPU cores.

D Effects of Land Rental Markets: Higher Reform Intensity

One aspect that we emphasized when describing the land rental reform is that there is substantial heterogeneity in growth of rentals across zones: while rental increases in some zones by less than one percent, a few other zones have increases of more than 15 percent. This heterogeneity is not captured by our benchmark specification (9) in which the dummy d_{zt} simply separates increases and non-increases in land rentals across waves. To exclude those zones where rental increases marginally, we re-define d_{zt} as a dummy that takes value one in the second period if rentals in zone z increase by at least two percent, and zero otherwise.[43] We find that the effects of land rentals on resource misallocation are larger with this specification. The relevant coefficients are -0.156 for farm-level efficiency gains, -0.236 for MPLa, and -0.212 for TFPR.

Table 17: Effects of Land Rental Markets: Higher Reform Intensity

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
Land Rentals (%), ψ	-0.156	-0.236	-0.212
	(0.035)	(0.032)	(0.032)
Observations	4,714	4,714	4,714
R^2	0.22	0.13	0.17

Notes: Results of econometric specification (9) for the following measures of farm-level misallocation: (a) efficiency gain $|\log(y^e_{izt}/y^a_{izt})|$, where y^e_{izt} is efficient output of farm i in a zone-level efficient reallocation and y^a_{izt} is actual output in the data, (b) marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\text{MPLa}_{izt}/\overline{\text{MPLa}}_{zt})|$, and (c) revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\text{TFPR}_{izt}/\overline{\text{TFPR}}_{zt})|$. Standard deviations are in the parentheses. Panel data for the Ethiopia ISA 2013/14 and 2015/16 waves are as described in Section 2. We restrict our treatment group to be those zones where rentals increase by at least two percent.

[43] Our results also hold if we require rental increases by at least one percent or three percent.

E Controlling for Household-Level Observables

Table 2 shows that the treated group and the control group are largely comparable over many household-level observables. Nevertheless, we repeat the difference-in-difference regression specified in equations (9) and (10) explicitly controlling for a set of household-level observables in Table 2 such as the household head's age, gender, education, marriage status, health status, and the household's size and distance to market. Notice that further controlling for land quality measures, such as elevation or slope, does not change our results since they are already taken into account when removing land quality from farm level measures of output. The results are displayed in Table 18. We can see that the results are very similar to those in Table 3. Including household-level observables as controls does not alter the effects of land rental markets on resource misallocation.

F Alternative Analysis of the Cross-Sectional Relationship Between Land Rentals and Misallocation

We explore an alternative approach to assess the effects of rental markets on resource allocation and productivity. Note that in our analysis, we define farm size using the operational scale, i.e., the actual operated land of farmers including rentals, instead of the owned land. Now let us consider another counter-factual analysis. Suppose we completely shut down land rental markets, so that farmers cannot rent in or rent out any land. In this case, the farm size will coincide with owned land, denoted as $\bar{l}$. We can use the actual capital input as well as our estimated productivity to calculate the level of output associated with this alternative land allocation as

$$\hat{y}_i = s_i^{1-\gamma}(k_i^{\alpha}\bar{l}_i^{1-\alpha})^{\gamma}.$$

We can then calculate the aggregate output of this hypothetical land allocation, denoted as $\hat{Y}^a = \sum_i \hat{y}_i$, and compare it to the actual aggregate output $Y^a = \sum_i \hat{y}_i$ to obtain the efficiency gain of land rentals that actually happens in the economy: $\hat{e} = \hat{Y}^a/Y^a$. In our 2013/14 sample, we find this ratio to be 0.934. That is to say, if we completely shut down

Table 18: Effects of Land Rental Markets: Reform Intensity

(a) Benchmark Specification

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	TFPR	MPLa
Land Rentals (d_z)	-0.078	-0.124	-0.136
	(0.034)	(0.031)	(0.031)
Observations	4,534	4,534	4,534
R^2	0.24	0.17	0.14

(b) Quantile Specification

Dependent variable:	Farm-Level Misallocation		
	Efficiency Gain	MPLa	TFPR
ψ_{Q1}	-0.045	-0.085	-0.077
	(0.030)	(0.031)	(0.029)
ψ_{Q2}	-0.026	-0.098	-0.080
	(0.034)	(0.037)	(0.033)
ψ_{Q3}	-0.119	-0.105	-0.079
	(0.041)	(0.037)	(0.040)
ψ_{Q4}	-0.238	-0.306	-0.299
	(0.071)	(0.066)	(0.063)

Notes: Results of Regression (9) in panel (a) and of Regression (10) in panel (b) for the following measures of farm-level misallocation: (a) efficiency gain $|\log(y^e_{izt}/y^a_{izt})|$, where y^e_{izt} is efficient output of farm i in a zone-level efficient reallocation and y^a_{izt} is actual output in the data, (b) marginal product of land (MPLa_{izt}) relative to the zone-level average, $|\log(\text{MPLa}_{izt}/\overline{\text{MPLa}}_{zt})|$, and (c) revenue productivity (TFPR_{izt}) relative to the zone-level average, $|\log(\text{TFPR}_{izt}/\overline{\text{TFPR}}_{zt})|$. Standard deviations are in the parentheses. Panel data for the Ethiopia ISA 2013/14 and 2015/16 waves are as described in Section 2.

rentals, the aggregate output will be only 93.4% of the actual output. In other words, the existing rentals increase productivity by around 7.07%. That is, one percent of rentals is associated with 7.07/10.9 = 0.65% more productivity. Notice that this figure is very similar to the cross-sectional relationship between land rental markets and productivity reported in Section 5.2. We also obtain a confidence interval of this statistics $\hat{e}$ through bootstrap estimates, which is $[0.890, 0.978]$ with a median of 0.938 and a standard deviation of 0.027. That is, the ratio of interest is statistically different from unity.[44]

Most likely, this calculation underestimates the effect of rentals due to selection (Lagakos and Waugh, 2013). To see this, notice that we can only compare the allocation of rentals for the currently existing farmers. That is, we ignore the notion that some farmers may completely rent out their land and migrate to the non-agricultural sector, as we cannot observe these farmers in our data. This extensive margin can be quantitatively important for resource allocation (Chen, 2017; Adamopoulos et al., 2017).

G Farm TFP, Plot TFP and the Extent of Misallocation

Our paper focuses on the effect of land markets. In particular, we study of the changes over time and across zones of the extent of misallocation as opposed to the study of the (cross-sectional) extent of misallocation itself. This focus on spatial and time variation implies that the presence of measurement error in the extent of misallocation is less of a concern for our study (see Section 7.4). Measurement error, however, remains an important issue for the study of the extent of misallocation.

In the specific context of agricultural production and ISA data, a potentially important type of measurement error is recall bias as these surveys are rolled over the entire year whereas there is usually only one rainy season. Restuccia and Santaeulàlia-Llopis (2017) use ISA panel data to assess the effect of recall bias in output and factor inputs—as well

[44] Note that in our sample, we observe more rent in than rent out. As a result, we rescale land input for everyone to make sure $\sum_i \bar{l}_i = \sum_i l_i$. If we rescale that only for those farmers who rent out (at a different rate to make sure the resource constraint holds), then we would get the statistics to be 93.9% instead of 93.4%.

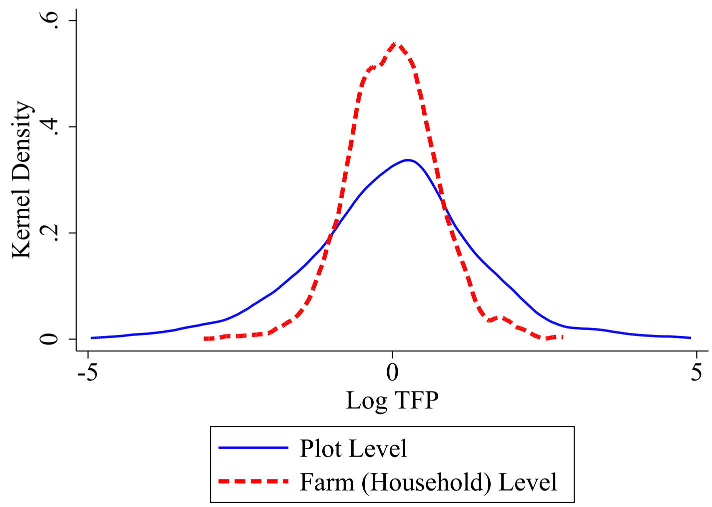
as other types of measurement error in output and factor inputs such as selling value of capital versus physical capital indexes and GPS measures of plot size versus self-reported measures of plot size—and find small effects on the implied extent of misallocation. More recently, Gollin and Udry (2017) emphasize plot-level variation across and within farms by estimating measurement error in inputs and outputs assuming that individual farmers are equally efficient in operating each and all plots that they manage. This emphasis in plot-level variation constrasts with our focus on farm-level variation as per our benchmark unit of analysis, the housheold-farm.[45]

We now assess how our benchmark permanent measure of farm-level TFP that determines the extent of misallocation fares against the plot-level TFP measures. We use plot-level data insofar it helps generate a household-farm level measure of productivity. Precisely, since a household generally operates a farm spanning several plots of land in the data—an average of more than seven plots in the case of Ethiopia—we aggregate both factor inputs and outputs at the household-farm level to calculate farm-level TFP. An important implication of our aggregation at the farm level is that potential plot-level shocks and classical measurement error on inputs and outputs are averaged out. However, at the same time, our aggregation has the caveat that it ignores potentially genuine efficiency gains from within-household factor input reallocations across plots. For example, this could be either because there is heterogeneity in the diminishing returns on the number of plots an individual can manage or because there are more than one managers (e.g., husbands and wives) per household—which is not uncommon in Ethiopia and other Sub-Saharan Africa countries—with potentially different managerial ability. That is, although our aggregation reduces the impact of measurement error, at the same time, it ignores potentially genuine variation that could be relevant to measure misallocation. Note that our estimate of permanent measure of farm-level TFP uses data from both 2013/14 and 2015/16 waves, which also helps reduce the impact of potential shocks and classical measurement errors.

To illustrate how our benchmark measure of farm-level TFP contrasts with the plot-level measure of TFP we plot the density of farm-level TFP and the density of plot-level TFP

[45] As in our analysis, in Restuccia and Santaeulàlia-Llopis (2017) the basic unit of production is also the farm household, not the plot.

Figure 7: Distributions of Farm- and Plot-Level TFP, Ethiopia ISA 2013/14



Notes: Distributions of farm-level and plot-level total factor productivity (TFP). The dispersion in plot-level productivity, represented by the standard deviation of log TFP, is 1.394, whereas for the farm-level productivity is 0.797.

in Figure 7. To assign a unique productivity per plot we used plot-level inputs and plot-level outputs assuming the same technology used in our benchmark analysis in Section 3.In particular, the land and labor inputs and the value of output (value added) are available for each plot of land. The capital stock, however, is slightly more complicated since is measured at the household level. Furthermore, it is reasonable to assume that the capital (for example, a plough) of a given household can be used in multiple plots owned by this household. To assign capital services used in each plot, we assume that plot-level capital services are proportional to the size of plots (i.e., a larger plot uses more capital than a smaller plot).

Consistently with a large microeconomic development literature, we find a lot of plot-level variation in productivity, even within farm households. Importantly, not all the plot-level TFP dispersion shows in our benchmark farm-level measure. Precisely, the dispersion in plot-level TFP is 75 percent larger than the dispersion in farm-level TFP. In terms of MPLa and TFPR we also find significantly larger dispersion using the plot-level TFP measures than the farm-level measures, respectively, 65% and 67% larger. We also find that the extent of misallocation at the plot level is substantially larger (with efficiency gain of nation-wide relocation equals to 4.72) than in our benchmark that uses farm-level productivity (with efficiency gain of nation-wide relocation equals to 2.00). That is, the extent of misallocation using plot-level variation in TFP is 2.36 larger than the extent of misallocation that uses our benchmark permanent measure of farm-level TFP.

Importantly, a key feature of the land institution that we study is the weak connection between operated land and productivity, reflected in a near zero correlation between farm land input and farm TFP. Interestingly, this pattern of misallocation is not much different when characterized at the plot level.[46] This implies that the reallocation gains are magnified at the plot level compared with household level reallocations simply because of the larger dispersion in plot-level TFP.

Finally, regarding our analysis of the effects of land markets on resource allocation and productivity, notice that it is unfeasible to conduct our difference-in-difference analysis at the plot level because the ISA data does not provide us with panel dimension for plots.

[46]Similarly, the correlation between log TFPR and log TFP is 0.92 when the unit of production is the plot and 0.87 when the unit of production is the farm household.