Property rights, land misallocation and agricultural efficiency in China

Amalavoyal Chari, Elaine M Liu, Shing-Yi Wang, Yongxiang Wang

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A.V. Chari[1], Elaine M. Liu[2], Shing-Yi Wang[3], and Yongxiang Wang[4]

[1]University of Sussex
[2]University of Houston and NBER
[3]University of Pennsylvania and NBER
[4]University of Southern California

May 2020

Abstract

This paper examines the impact of a property rights reform in rural China that allowed farmers to lease out their land. We find the reform led to increases in land rental activity in rural households. Our results indicate that the formalization of leasing rights resulted in a redistribution of land toward more productive farmers. Consequently, output and aggregate productivity increased by 8 and 10%, respectively. We also find that the reform increased the responsiveness of land allocation across crops to changes in crop prices.

*We gratefully acknowledge funding from the ESRC-DFID. This paper has benefitted from comments from Santosh Anagol, S. Anukriti, Jonathan Conning, Chang Tai-Hsieh, Rachel Heath, Melanie Khamis, Adriana Kugler, Annemie Maertens, Laura Schechter, and seminar participants at the Agricultural and Development Conference (at Yale University), Columbia University, Hunter College, Pennsylvania State University, Stanford University, SUNY Buffalo, University of Virginia Darden, University of Delaware, University of Southern California, Virginia Tech, Wharton and the World Bank. All errors are our own.

1 Introduction

Growth in agricultural productivity has long been viewed as central to the process of structural transformation and economic growth (Lewis 1955, Ranis and Fei 1961, Gollin, Parente and Rogerson 2002). Yet, productivity in agriculture remains remarkably low in most developing countries, and this can (at least mechanically) account for most of the overall differential in labor productivity between rich and poor countries (Caselli 2005, Restuccia, Yang and Zhu 2008, Gollin, Lagakos and Waugh 2014). Recent studies suggest that the relative inefficiency of agriculture in poor countries may be a result of frictions that produce a misallocation of productive resources (Lagakos and Waugh 2013, Adamopoulos and Restuccia 2014). This idea extends a large literature that emphasizes factor misallocation as a source of low productivity in the manufacturing sector (e.g. Restuccia and Rogerson 2008, Hsieh and Klenow 2009, Midrigan and Xu 2014, Chari 2011), and is also predated by a long-standing literature on agriculture in developing countries that has focused on the relative efficiency of small and large farms (e.g. Sen 1962, Benjamin 1995, Binswanger, Deininger and Feder 1995, Barrett, Bellemare and Hou 2010).

In the context of agriculture, the misallocation hypothesis is supported by two robust stylized facts: the preponderance of small family farms in developing countries, suggestive of a misallocation of land within the agricultural sector, and the preponderance of workers in the agricultural sector, indicative of a sectoral misallocation of labor. A natural question, and one that is central for policy, is the extent to which this misallocation can be rectified by reducing transaction costs in agricultural land markets. Answering this fundamental question has proved difficult in the absence of a suitable policy experiment. Our paper offers the first credible evidence on this issue by exploiting a property rights reform in China that facilitated land market exchanges.[1] We analyze the impact of the Rural Land Contracting Law (RLCL) in China, a reform that gave farmers legal rights to lease their land while re-iterating existing protections for the security of land rights.[2] The RLCL provides a unique

[1]In a concurrent working paper, Chen, Restuccia and Santaeulalia-Llopis (2017) utilize a leasing reform in Ethiopia to demonstrate a positive relationship between land leasing and productivity, but in the absence of data prior to the leasing reform, their analysis requires the assumption that post-reform changes in observed leasing activity are exogenous to the outcomes of interest.

[2]This occurred in a context where farmers do not have full ownership rights to their land, but rather only use rights granted by the local government.

opportunity to estimate the importance of land market imperfections in generating misallocation.

Well-defined leasing rights over land should facilitate a reallocation of land from less productive to more productive producers, thereby improving aggregate efficiency. In practice, there are at least two reasons why formalization of such rights may not have a significant impact. First, there may be other market failures or constraints that prevent the efficient reallocation of land, and which may not (at least in the short run) be relieved by restoring the missing market for land. Second, the importance of formally-defined rights is itself a subject of debate. While some recent studies do suggest an important role for well-defined property rights (e.g. Banerjee, Gertler and Ghatak 2002, Goldstein and Udry 2008), there is also a prominent view that informal institutions can adequately substitute for lack of formal land rights (see, for example, Migot-Adholla et al 1991, and critiques by Peters 2004 and Udry 2011). However, the debate on the role of property rights has largely focused on the implications of tenure security for agricultural investments and productivity; our paper offers a distinct contribution in our focus on the allocative implications of exchange rights.[3]

To evaluate the effect of the RLCL, we collected novel data on the province-level timing of implementation of the central law which was announced in 2003.[4] We combine the province-level implementation data with panel data on inputs and outputs of agricultural households collected by the Chinese Ministry of Agriculture (called the National Fixed Point Survey or NFP) from 2003 to 2010. In addition to the long panel dimension, this data set is somewhat rare among agricultural household surveys in its large sample size and broad geographic coverage. To address a key limitation of the NFP which has no information about land quality, we use data on land quality that we collected in a survey in 2012 among a 10% sub-sample of households in the NFP.

We begin our analysis with difference-in-differences estimates of the impact of the reform that exploits the staggered timing of implementation across provinces. We find evidence of an increase in land rental activity following the reform that grew over time; after the reform, the amount of land

[3]One predecessor is Ravallion and van de Walle (2006), who analyze whether the 1993 Land Law in Vietnam which bundled selling rights with a titling system resulted in a more efficient allocation of land. However, our data and identification strategy can more directly assess the efficiency effects of improving property rights and exchange rights.

[4]Deininger and Jin (2009) and Zhao (2018) examine the same property rights reform in China, but focus on different outcomes (land reallocations by the government). Furthermore, their identification is based on a before-after 2003 comparison whereas we utilize the staggered nature of implementation across provinces.

leased increased by over 7%. At the village level, overall output and the aggregate productivity of land also increased significantly by 8 and 10%, respectively. This is consistent with the notion that reducing frictions in the land market should result in efficiency-enhancing reallocation of land. To test for such reallocations, we use a production function approach to estimate farmer productivity after controlling for a variety of inputs.[5] We find that the reform increased the amount of land farmed by relatively more productive farmers, while reducing the amount of land cultivated by relatively less productive farmers. This conclusion is robust to using alternative measures of farmer productivity, including agricultural profits. A simple decomposition suggests that nearly 88% of the observed increase in aggregate productivity can be attributed to input reallocations associated with the reform.

We also observe a corresponding reallocation of labor, which is a complementary input to land: the amount of hired labor increased on relatively more productive farms, suggesting a within-village reallocation of labor. Interestingly, we do not find significant evidence of inter-sectoral labor reallocation, either in terms of labor moving out of agriculture (within the village) or into migration (out of the local area).[6]

While the literature on misallocation has largely focused on static inefficiency, an implication of factor market rigidities is that they can also hinder farmers' ability to respond to economic fluctuations. We test this hypothesis by examining whether the reduction in land transaction costs due to the reform allowed farmers to respond better to changes in agricultural prices, an important source of risk in the agricultural sector.[7] We find that an increase in the price of a crop induces greater reallocation of land (within the village) toward that crop in the post-reform period (relative to the pre-reform period), consistent with the hypothesis that the reform reduced frictions in the efficient allocation of land resources.

We undertake a range of checks to verify that we are indeed estimating the effect of the RLCL. The principal threat to identification in our analysis is that the timing of program implementa-

[5] This builds on a large existing literature on agricultural production function estimations. See Gollin and Udry (2017) for an overview.

[6] This contrasts with other papers that demonstrate that improving private property rights changes labor market outcomes (Field 2007, Wang 2012).

[7] This exercise also relates to an existing literature in agricultural economics that estimates the response of yields to crop prices (see for example, Choi and Helmberger 1993, Houck and Gallagher 1976, Menz and Pardey 1983).

tion may have been endogenous to changes in provincial agricultural outcomes. We first use the panel nature of the data to verify that the outcomes of interest were not trending differently prior to implementation in reformed provinces relative to provinces that have yet to reform. Next, we examine the determinants of province-level implementation, and find that changes in rural income and agricultural employment and prior levels of government land reallocations do not predict the timing of implementation. A related concern is that implementation of the RLCL may have coincided with other agricultural reforms. During the period under study, the other important policy change that affected rural households was the elimination of the agricultural tax. We include the time-varying provincial agricultural tax rate in our regressions, and this inclusion has almost no impact on our estimates.

This paper adds to our understanding in a number of different ways. First, the observed increases in aggregate output and productivity following the reform are economically significant, and confirm that the right to lease land is an important dimension of property rights. Second, our study makes a contribution to the misallocation literature, which, in the absence of suitable policy experiments, has relied on structural modeling to infer the losses due to misallocation. By leveraging the RLCL, we are able to estimate the importance of misallocation without the need for strong assumptions. It is not straightforward, however, to directly compare our estimates to those in the literature, because existing studies typically estimate the gains from a full elimination of distortions. In contrast, the RLCL has resulted in only a partial reduction in land misallocation. That the resulting productivity gain is as large as it is (we observe an approximately 10% increase in aggregate productivity as a result of a 7% change in renting), suggests that the marginal gains from reallocation are sizeable. This is an important finding that in turn suggests that the gains from full reallocation could be even more substantial. Indeed, we show that applying a similar methodology as in Adamopoulos et al (2017) yields a suggested counterfactual efficiency gain of 73% corresponding to a complete elimination of land distortions. It may well be that these larger efficiency gains could be achieved by a reform that included other property rights such as sales rights or mortgage rights, or by addressing other sources of factor misallocation in agriculture.

The paper proceeds as follows. Section 2 provides detailed institutional background on land tenure laws and reform in China. Section 3 describes the data. Sections 4 and 5 present the empirical analyses, and Section 6 concludes.

2 Institutional Background

In 1979, the Household Production Responsibility System (HRS) was created to dismantle the existing collective organization of agricultural production and to give households control of farming decisions and output. After 1979, farmers had private use rights to agricultural plots but these land rights were relatively insecure as local governments could reassign plots until the late 1990s. In 1998, the Land Management Law granted farmers 30-year formal land contracts from their village governments, providing security of land tenure. This paper focuses on the property rights reform that occurred with the official announcement of the Rural Land Contracting Law (RLCL) in 2003. In addition to re-iterating the existing policy of 30-year contracts between village governments and farmers, the RLCL provides farmers the legal right to rent out and rent in land, outlining rules for leasing, transferring leases, and how to address land leasing disputes. Prior to 2003, there were instances of informal land rental agreements, including contracts based on verbal agreements among family and neighbors. The 2003 reform offered legal security to both parties of a leasing contract.[8]

Regarding the RLCL, Li (2003) writes, "This landmark law represents the most important legal breakthrough for securing land rights for China's 210 million farm households since the adoption of the HRS." The RLCL makes no statements about inheritance rights and does not reverse a prior prohibition of using land as collateral. Thus, the main distinction between the RLCL and previous land reforms was the introduction of legal protections offered to rural households who lease land.

As shown in Appendix Figure A.1, there was a large drop in government-led land adjustments starting in 1998 (following the Land Management Law), but there was very little subsequent change in government-led land readjustments occurring after 2003.[9] The share of villages engaging in major

[8]In case disputes over rental contracts and private negotiation between the two parties fail, they can ask the village or town government to assist in the negotiations. If that fails, either party can initiate arbitration or a lawsuit in the local court system.

[9]These aggregate statistics are calculated using the Village Democracy Survey of 2006. The main data of our analysis (NFP) do not have questions on perceptions about security or the occurrence of reallocations.

government-led land reallocations was under 4% from 2001 to 2006 and zero in 2007 and 2008.[10]

While the central government adopted the RLCL in 2003, it also stated in Article 64, "The standing committees of the people's congresses of the provinces, autonomous regions and municipalities directly under the Central Government may, in accordance with this Law and in light of the actual conditions of their administrative areas, work out measures for implementation of this Law." This follows many other market reforms that started after 1978 where the central government issued general guidelines on the priorities, and local governments were encouraged to implement and to experiment within the guidelines (Xu, 2011).

We discuss a few articles of Jilin province's implementation of the RLCL in 2005 to provide some examples of the scope of changes at the provincial level. Provinces can add regulations; for example, Article 14 of Jilin's law states that the county-level and town-level governments must build a database registering the land leases, and publicize information on this leasing registry. The provincial law can also remove requirements stipulated at the central level; for example, Article 27 of the central RLCL states a requirement of a two-thirds vote to adjust contract terms between villagers and the local government in the case of natural disaster, but Jilin does not allow for re-adjustment in this scenario at all.

According to a World Bank (2002) report on land tenure in China, local authorities are the major obstacle to the implementation of central initiatives over agricultural land tenure. In a report by Li (2003), a former Chinese representative for the Rural Development Institute also suggests that local capacity and cooperation are key to the successful implementation of the RLCL. Thus, we use the dates of the provincial-level implementation of RLCL as the relevant time in which agricultural households could exercise these new land rights.[11] By the end of 2010, 22 provincial governments had made official announcements about the local implementation of RLCL (as shown in Appendix Table A.1).

[10]Similarly, in data we collected in 2012 (described in Section 3.2), only 2.5% of households had experienced a government land reallocation in the past five years.

[11]There is evidence that provincial implementation was enforced as there are many examples of court rulings which cite the provincial implementation of the RLCL in determining the court's decision. For example, in the 2012 legal case of Tong Min Chu Zi (Case Number 0175) where there was a dispute over a land rental contract between two villagers, the court ruling cites several articles in the implementation of the RLCL in Jiangsu Province (2004) in finding that the defendant (who rented in the land) violated the law by changing the use of the land from agricultural to commercial and must return the land to the plaintiff (who rented out the land).

Anecdotal evidence supports the idea that farmers responded to the reform by changing their leasing behavior and substantially increasing their income. After the law was implemented in the province of Ningxia in 2015, a newspaper article describes an interview with a farmer named Xueying Wang: "he rented out all 29 mu of his land to a large-scale farm at the price of 700 RMB per year. Thus, he can get 19,600 RMB per year from the land, and at the same time, his wife is working for that farm and she can earn 24,000 RMB per year... His income doubled compared to the past [before receiving the right to contract out land]" (Ningxia Daily 2015). This case also highlights the potential changes in the distribution of land within villages following the reform.

3 Data

The main analysis combines agricultural outcomes and inputs from a household-level panel data set with data that we assembled on the timing of provinces' implementation of the RLCL.

3.1 National Fixed Point Survey

Our primary data source is the National Fixed Point Survey (NFP), a panel survey collected by the Research Center of Rural Economy (RCRE) of the Chinese Ministry of Agriculture, beginning in 1986. We use annual waves of data between 2003 to 2010 for data comparability as the questions and the structure of the survey changed substantially in 2003. For the period of 2003-2010, our dataset covers more than 19,000 households in 399 villages from 32 provinces. NFP villages were selected for representativeness based on region, income, cropping pattern, population, and non-farm activities. Within each village chosen, a random sample of households was drawn to be included in the survey. In relatively rare cases in which the entire household moves permanently, the household attrites from the survey and is replaced by another household. The NFP data contain detailed information on household agricultural production, employment, and income.

Benjamin, Brandt and Giles (2005) demonstrate that the data are of high quality and provide a detailed overview of the data.[12] The key advantages of the data for our analysis are the panel

[12] Benjamin, Brandt and Giles (2005) offer a detailed discussion of the representativeness of the NFP data by comparing it to aggregate statistics based on the agricultural census (NBS). They demonstrate that the NFP is similar to the NBS along many dimensions, including share of households that are agricultural and household size.

structure and the detailed information on agricultural inputs and outputs at the household-crop-year level. The NFP does not have any information broken down at the plot level or the specific terms of land rental contracts such as contract length or payment terms. While the NFP does not contain any information on land quality, we are able to link in a supplementary data set that we collected on a subset of NFP households to address the lack of land quality data.

Summary statistics for the main outcome variables are presented in Table 1. The structure of the household survey is that some questions are asked at the household level regarding the past year, while most questions on agricultural input and output are asked at the household-crop level. Panel A provides summary statistics on variables available in the NFP at the household-year level. The average household in the data cultivates 12.4 mu of land (or about 2 acres).[13] We construct a measure of the amount of land rented in by households (in mu) by taking the difference between two questions asked in the survey: the household's reported farmland (which includes assigned and rented in land) and the amount of land assigned to them by the village government.[14] On average, the amount of area rented in is 2.2 mu, which is approximately 18% of the amount of land cultivated. For the last years of the data, 2009 and 2010, we also have information about the change in the amount of land rented *out* to individuals and to firms.[15] In these years, the amount of land rented out to individuals is much greater (0.33 mu) than to firms (0.03 mu). Total household income (from all sources) is on average 29,000 RMB.[16]

We have a few measures of labor supply decisions of household members. Unlike most of the other variables that are asked at the household level, these labor supply questions are asked for each member of the household, where out of the 365 days of the last year, they are asked the number of days that they worked off-farm (not in agriculture) in the same township, as a migrant worker and on-farm (in agriculture). We aggregate the individual-level measures for each household and year. Over half of the households in the sample had at least one individual engage in migration and 42%

[13]The full distribution is shown in Appendix Figure A.2.

[14]For a small number of observations ($\sim 0.1\%$), the constructed measure of area rented in is negative. Dropping these observations from the analysis makes little difference to any of the results.

[15]The questions about land rented out to individuals and firms only asks about new land rented out in the past year. If a household is renting out land but the contract began two years ago, this flow variable would not pick it up.

[16]This is approximately 3600 USD. We convert income and cost variables into real 2002 RMB using a province-level consumer price index from the Regional Economy Database.

of households engaged in off-farm labor in the past year. Across all individuals in the household, households reported spending an average of 234 days working as migrants and 126 days working in off-farm activities in the local area.

In order to examine the aggregate effects of the reform, we aggregate some measures from the household-level to the village-level. Panel B presents aggregate agricultural revenue and aggregate agricultural revenue divided by land (the latter represents aggregate land productivity). While the survey reports total agricultural revenue, we construct a measure that purges any price variation across time, village or crop. To do this, we construct revenue by combining output in weight with national crop-level prices in each year. This effectively purges these variables of changes in local prices, and allows us to interpret reform effects in real terms.

In the questions on agricultural production, the survey asks households to report all inputs and outputs at the crop level.[17] In addition to having information on output in terms of revenue, we also have the physical amount of output (as measured in kilograms). As shown in Panel C, on average, a household utilizes 4.7 mu of land per crop. This measure includes both their own land as well as land that they have rented in. They spend 86 RMB per crop per year on machine costs - these costs refer solely to the costs of operating the machinery, in terms of oil, fuel, etc. We also have a separate measure of the total value of agricultural assets owned by the household (which is not crop-specific). We treat this as a household-level measure of capital stock, while noting the important caveat that actual capital usage may deviate significantly depending on the thickness of the rental market for agricultural machinery. Labor inputs, which includes household labor and hired labor, averages at 68 days per crop. When we estimate agricultural production functions (Section 4.3), we aggregate other agricultural inputs which individually contain many zero values. These include chemical and organic fertilizer, pesticides, irrigation, small tools, agricultural covers, animals and other costs.

[17]These refer to production on land that the household cultivates. If they rent out their land, the inputs and outputs on land they rent out and do not cultivate themselves are not included.

3.2 Land Quality Data

One drawback of the NFP data is that it lacks information on land quality. To address this issue, we supplement the NFP data with data on land quality from a survey that we fielded in 2012 among a subset of NFP households, called the Rural Firm and Household Land survey. The goal was to study the link between commercial firms and farmers. The villages and households were chosen based on the reported land rental activity in the prior years' rounds of the NFP. Merging this into the main NFP data set, we have a subset of 2326 households across 247 villages and 20 provinces. The key variable for our analysis here is households' self-reports on the quality of the land plots that they cultivate. These self-reports are categorical, with each plot being labeled as high, medium, or low quality. We average the plot-level measures of quality to obtain a household-level measure of average soil quality.[18]

3.3 Crop Price Data

We collected provincial-level agricultural price indices from China Rural Statistics Yearbooks for the period 2002 to 2009. To serve as a measure for global prices, we also collected the corresponding U.S. agricultural crop price indices over the same period from the Food and Agriculture Organization (FAO) of the United Nations. All the indices are set to 1 for the year 2001. Appendix Figure A.3 presents the average price indices across all provinces for each crop. Panel A shows the price indices (averaged across provinces) for each of the five staple crops and Panel B shows the corresponding price indices for each of the four cash crops for which we have price data.[19] The figures show there is more variation in the prices of cash crops than staple crops. This is because cash crop prices are driven by global market forces while the government may intervene to control staple crop prices.

3.4 Timing of Reform Implementation

We collected information on the local province-level implementation of the RLCL that was passed at the national level in 2003 from several different sources. The main source is PkuLaw, a

[18]This measure only excludes the quality of plots that the household rents in from others as the land rented in in 2012 may not have been cultivated by the household in the time period of our main analysis.

[19]There is no price index for hemp, and we do not assign a single price to categories in the NFP data that refer to several crops such as the "other staple crops" category.

database that provides comprehensive coverage of local laws and regulations in China. We used the following keywords to search in the database: "Tudi Chengbao (land contracting)", "Tudi Liuzhuan (land subcontracting)". For completeness, we also searched several other law databases, including Xihu Law Library (www.law-lib.com), Beijing Zhongtian Nuoshida Technology Company Law Database (www.law-star.com) and Zhengbao Online Education Company's database (www.chinalawedu.com). After this initial search, we read through all of the legal documents found. For consistency across provinces, we discarded the ones that are issued by governments below the provincial-level since we focus on province-level variation. Next, we dropped documents that are issued by departments rather than by the provincial government directly (e.g. some documents are issued by lower-level provincial departments such as Jiangsu Department of Agriculture that discuss the implementation of the policy). Finally, we filtered out documents that were not about the implementation of the RLCL.[20] These multiple stages of filtering leave us with the final set of legal documents that allow us to codify the timing of local implementation of the 2003 RLCL for each province, summarized in Appendix Table A.1.

To examine the question of why some provinces implemented the reform at different times than others, we collected province-level data from four sources. The first source is the Chinese Statistical Yearbooks.[21] Second, we have averages by province on the share of households that experienced a land reallocation by the village based on data collected by RCRE in the Village Democracy Survey.[22] Third, we compiled data on land disputes, by counting news articles with on land disputes between 1998 and 2003 on Wisenews, which is the largest database of newspapers in China.[23] Finally, we compiled data on the straight-line distance between each provincial capital

[20]In other words, the keywords appear in the title or the main body, but the document is not about implementation of the RLCL.

[21]While there are many variables in the Chinese Statistical Yearbooks, we limit our analysis to those that are available for every province and year. We also exclude variables that we highly correlated with others; for example, there are several different measures of rural income that are highly correlated. In the regressions in columns 3 and 4 of Appendix Table A.2, we exclude the smaller categories of GDP and expenditures for parsimony but the results are similar if we include all of them.

[22]We are grateful to Nancy Qian for sharing this data with us. This survey was conducted in 2006 and thus reduces the time periods available for the analysis. The survey asked households retrospective questions about the village government reallocated their land. Padro i Miquel, Qian and Yao (2012) describe the data in more detail.

[23]The specific keywords for land disputes that we used are: tudi chengbao jiufen (land contracting dispute), tudi chengbao anjian (land contracting lawsuit), tudi liuzhuan jiufen (land subcontracting dispute) and tudi liuzhuan anjian (land subcontracting lawsuit).

and Beijing using Google maps.

In Appendix Table A.2, we estimate an equation where the sample is limited to the periods including the reform year and the years prior, and the dependent variable is an indicator for the reform year. We examine whether characteristics at the province level predict the timing of the reform. Most variables are insignificant. The results suggest that increases in urban income are correlated with the province implementing the reform; in column 1, a 10% increase in urban income corresponds with a 5.3% increase in the probability of reform and this estimate is significant at the 5% level.[24] However, with the inclusion of the year and province fixed effects in the even columns, the relationship is no longer significant or even positive. Given that we are looking at the impact of the reform on agricultural outcomes, it is reassuring that rural income, agricultural employment and the frequency of land reallocations and land disputes are not significantly correlated with the timing. These results provide some reassurance against the idea that agricultural outcomes are directly driving the decision of the provincial government regarding the reform.

Second, we examine whether the timing of reform adoption in a province is predicted by changes in the distribution of land or dispersion in land productivity. More specifically, we regress the timing of the provincial adoption of the reform (which is an indicator) on the standard deviation of land holdings and the standard deviation of marginal productivity. These measures are constructed at the village level from the NFP data and the construction of marginal productivity is described in more detail in Section 4.3. In Appendix Table A.3, the results indicate that the pre-reform distribution of land and productivity does not predict the timing of reform adoption.

4 Estimating the Impact of the Property Rights Reform

4.1 Effects on Household-level Rental Activity

We begin by examining whether the implementation of the property rights law affected land renting activity. Our empirical strategy exploits the staggering of the implementation of the reform across

[24] One possibility is that provincial leaders were hoping that the implementation of the reform would facilitate rural-to-urban mobility. While our results in Table 4 suggest this reform did not increase migration out of the rural areas, it may still be the case that the timing decisions were driven by this motive.

provinces to identify the effect of the reform. For this household level outcome, we estimate the following equation for each household h in province p and in year t:

$$y_{hpt} = \alpha + \beta_0 PostReform_{pt} + \beta_1 ReformYear_{pt} + \theta Tax_{pt} + \gamma_t + \gamma_h + \epsilon_{hpt} \quad (1)$$

where $PostReform_{pt}$ is an indicator variable for the years following the implementation of the reform (not including the reform year itself) and $ReformYear_{pt}$ equals one in the year that the reform was implemented in province p.[25] The regression controls for household and year fixed effects, denoted by γ_h and γ_t respectively. The standard errors are clustered at the province level.

A common concern with difference-in-difference estimates is that there are other changes happening at the same time that are driving the results. For this concern to be valid, the roll out of the other change across provinces would need to follow the implementation of the RLCL. The one other significant law change for rural households around this period is the reduction and elimination of the agricultural tax.[26] All the provinces in our sample began reducing this tax in 2004. Any aggregate change before versus after 2004 would therefore be removed with the year fixed effects in our regressions. Nevertheless, as an added precaution, we control for the tax rate (Tax_{pt}) which declines at different rates across provinces starting in 2004 as a control in all the regressions.[27]

In Table 2, the outcome is the inverse hyperbolic sine (IHS) function of area rented in and out.[28] We estimate equation 1 above for three samples, starting with the full sample of NFP households (column 1). The estimates indicate a 7% increase in the area of land rented in, significant at the 10% level. This may however represent an underestimate of the impact of the reform, given that a number of households in the sample do not cultivate land at all, and we would not expect them to be renting in land. Accordingly, in column 2, we restrict the sample to farming households only. As expected, the point estimates are now larger (an approximately 10% increase in area rented in)

[25] As shown in Appendix Table A.1, many of the reforms were implemented in October or November of the calendar year. Given that the reform tended to be implemented late in the year, we separate the effects of the reform year from the years after the implementation.

[26] See Wang and Shen (2014) for more background on the agricultural tax. Another term often used to describe the agricultural tax in China is an output quota.

[27] All of the results in the paper are robust to the exclusion of this control.

[28] The IHS function is similar to a logarithmic transformation but is well defined for values of zero. Thus, we use it for continuous outcomes whose distribution includes a preponderance of zeros and a long right tail. We use the logarithmic transformation for continuous variables without any zeros.

and more strongly significant.

A potential concern with this estimation is that the composition of households changes over the sample period, both because of villages or households being added to the survey sample, as well as because of villages or households attriting from the survey. To address this concern, we further restrict the sample to a balanced panel of households that are present during the entire sample period (column 3). The point estimates remain stable, albeit somewhat less precisely estimated due to the large reduction in sample size.

To look at pre-reform and post-reform trends, we estimate a specification that uses three leads and lags around the implementation:

$$y_{hpt} = \alpha + \sum_{k=-3}^{2} \beta_k Reform_{pt,k} + \theta Tax_{pt} + \gamma_t + \gamma_h + \epsilon_{hpt} \tag{2}$$

where $Reform_{pt,k}$ is an indicator variable that indicates the period relative to the reform implementation in the province. Thus, $Reform_{pt,-2}$ refers to two years prior to the year of implementation and $Reform_{pt,2}$ refers to two years after implementation. The omitted category is $k = -1$. The sample here is restricted to the six waves around the implementation year in each province that adopted the reform, but additionally includes provinces that did not adopt the reform (at least as of 2014). The inclusion of this "pure control" group is important as it helps to mitigate the underidentification problem that arises in event studies without such a control group (Borusyak and Jaravel 2017). The specification in equation 2 above allows us to test the identification assumption that the timing of the implementation of the reform in each province is exogenous to our outcomes of interest.

Figure 1 displays the estimates corresponding to equation 2 where the dependent variable is the IHS of the amount of land rented in.[29] We observe a shift in both the magnitude and significance of the coefficients after the implementation of the reform. Furthermore, there are no significant trends in the outcome prior to the implementation. This provides support for the identification assumption that the timing of the implementation of the reform is not driven by changes in land rental activities in the province.

[29]The corresponding coefficients are shown in column 1 of Appendix Table A.4.

Given that we are interested in the reallocation of land within villages, there is a possible concern with an estimation strategy that uses a sample of households in the village rather than a village-level land census. We might be concerned that land rental transactions are occurring but that land is being transferred to individuals outside the sample. This concern is mitigated to the extent that the sample of households is representative of the village (i.e. it neither over- nor under-represents the individuals who are renting-in land). A related concern is that land is being leased to agricultural firms (which are not surveyed in the household-based survey), in which case we would be under-estimating the effect of the reform on renting behavior.

We undertake several checks to evaluate these concerns. First, we use survey questions on the amount of land rented out in the 2009 and 2010 waves.[30] Column 4 of Table 2 shows that the reform led to a 7% increase in the amount of land rented out. Reassuringly, this estimate matches well with the impact of the reform on renting-in activity (column 1 in Table 2). Second, we directly examine the impact of leasing to agricultural firms by using the survey question on the amount of land rented out to firms in the 2009 and 2010 waves. Column 5 of Table 2 shows a small (1.08%) negative effect on the amount of land rented to firms. As shown in column 6, the increase in overall renting out is almost entirely accounted for by renting out to other individuals, rather than to firms. Third, we examine whether the total village-level area of land reported under use by households in the sample declines. In a village-level regression of the impact of the reform on the logarithm of total village land area, there is no significant change in the area of land under use in the survey sample.[31] These results provide reassurance that any bias in our estimates due to biases in household sampling is unlikely to be substantial.

As an additional robustness check, we estimate reform effects on the restricted sample of provinces that adopted the reform. The results are reported in Appendix Table A.5, which also shows the corresponding estimates for all the subsequent main outcomes examined in this paper. Because this sample has only 23 provinces (as opposed to 32 provinces in the full sample), one may be concerned about possible bias in the standard errors due to the small number of clusters. We

[30]Given that there are only two waves of data available for these questions, the identification of reform effects on land rented out is based on provinces that implement their reforms in 2009 or 2010.

[31]The coefficient estimate on $PostReformYear$ is -0.0247 with a standard error of 0.043.

address this concern by following the randomization inference procedure suggested by Bertrand, Duflo and Mullainathan (2004). First, we randomly assign a year of reform implementation to each province, while keeping fixed the distribution of reform events over time. We then estimate the difference-in-difference specification to obtain the corresponding placebo treatment effect, and then repeat this procedure 100 times, obtaining a distribution of placebo treatment effects, against which we compare the treatment effect obtained from the actual treatment assignment. This allows us to obtain p-values and tests of statistical significance. In Appendix Table A.5, we report the p-values obtained from this procedure, while the panels of Figure A.4 show the cumulative distribution function of placebo treatment effects for each of the outcome variables. Overall, across all outcomes as well as for the land renting outcome specifically, we observe that the point estimates obtained on the restricted sample are similar to those obtained in the full sample, and the estimated effects are clearly significant when compared to the distribution of placebo effects (albeit with some loss of statistical significance in one or two cases).

4.2 Effect on aggregate output and productivity

We now examine the effect of the reform on aggregate agricultural output and productivity. Aggregate output is defined as village-level aggregate revenue, measured at national average prices, as explained in Section 3.1. We refer to village-level aggregate revenue per mu as aggregate productivity (which can also be interpreted as aggregate yield). We estimate the following equation, where the unit of observation is village v in province p and year t:

$$y_{vpt} = \alpha + \beta_0 PostReform_{pt} + \beta_1 ReformYear_{pt} + \theta Tax_{pt} + \gamma_t + \gamma_v + \epsilon_{vpt} \tag{3}$$

where the standard errors continue to be clustered at the province level. Table 3 presents the estimates where the dependent variables are the logarithms of real village-level revenue and revenue per unit of land. The point estimate in column 1 indicates that land reform has increased aggregate output by approximately 8 percent. Unsurprisingly, this is also mirrored by a 10% increase in the aggregate productivity of land (column 2). These estimates are significant at the 10% and 1% levels, respectively. Figure 2 graphs the estimated leads and lags of reform effects. It shows that there were no significant changes in these variables that anticipated reform implementation, but there were significant, positive increases in these outcomes after the reform implementation.

4.3 Constructing Total Factor Productivity and Marginal Product Measures

To test the model's predictions in terms of land reallocation, we first construct measures of total factor productivity (TFP) and the marginal product of land (MPL) using the detailed survey information on crop-specific inputs and output. We assume a Cobb-Douglas crop-specific production function that can be written in logs as follows:

$$y_{icvt} = \alpha_c \log L_{icvt} + \beta_c \log N_{icvt} + \gamma_c \log K_{icvt} + \delta_c \log M_{icvt} + \phi_{icvt} \tag{4}$$

where y_{icvt} denotes log (physical) output of farmer i growing crop c in village v in year t; L_{icvt}, N_{icvt}, K_{icvt} and M_{icvt} denote the area, labor days, machinery cost and all other input costs, respectively.[32] The logarithm of total factor productivity (TFP) is given by ϕ_{icvt}. Consistent estimation of the parameters of the production function depends on what we assume about unobserved TFP, because this is likely to be correlated with input decisions (Marschak and Andrews 1944). We assume that TFP can be decomposed into (i) a fixed farmer-crop component that captures the farmer's fixed ability to farm a given crop, (ii) a farmer-year component that captures time-varying shocks to productivity that are common to all crops grown by the farmer (such as a health shock), (iii) a time-varying component that is common to all farmers in the village that are growing crop c (such as weather shocks or pest infestations), and (iv) an idiosyncratic shock that is specific to the farmer and the crop in a given year:

$$\phi_{icvt} = \phi_{ic} + \phi_{it} + \phi_{cvt} + e_{icvt}. \tag{5}$$

Because farmers grow multiple crops in any given year, we can estimate the regression specification (equation 4) jointly for all crops (in a single regression where the coefficients on inputs

[32] Appendix Table A.6 shows that input intensity does not change significantly following the reform. This result isn't necessary for consistent estimates of the production function, but it is interesting to note that the reform doesn't lead to an increase in mechanization.

are allowed to be crop-specific), while absorbing ϕ_{ic}, ϕ_{it} and ϕ_{cvt} by farmer-crop, farmer-year and village-crop-year fixed effects.[33] Assuming that the idiosyncratic component of TFP, e_{icvt}, is unobserved by the farmer at the time that input decisions are made, the inclusion of these fixed effects addresses omitted variable bias arising from the dependence of input choices on the farmer-observed component of TFP.[34]

Appendix Table A.7 presents the estimated (crop-specific) production function coefficients. On average across all crops (i.e. if we restrict elasticities to be equal across crops), the elasticity of output with respect to land is 0.47, which is slightly larger than estimates for China obtained from aggregate data that range from 0.35 to 0.38 (Chow 1993, Cao and Birchenall 2013), although it should be noted that our estimates of elasticities exhibit substantial variation between crops.

We use the estimates from the production function to construct two measures of productivity. First, we calculate a household-crop-year specific measure of marginal productivity: under the Cobb-Douglas assumption, this is defined as the average productivity of land (i.e. output per unit of land) multiplied by the elasticity of output with respect to land where the latter is obtained as the coefficient on log land in the production function regression. Second, we construct a household-crop measure of total factor productivity by obtaining the estimated ϕ_{ic} fixed effect from the production function regression.

Before concluding this section, we implement some robustness checks on the production function estimates. In Appendix Table A.8, we assess the importance of selection due to entry and exit, by estimating the production function using a balanced sample of farmer-crop combinations that span the period 2003 to 2010. The production function coefficients are largely similar to those obtained before, indicating that selection driven by unobserved productivity shocks is not a serious problem.

In Appendix Table A.9, we examine the sensitivity of the estimates to a more stringent estimation strategy, in which, in addition to the inclusion of the fixed effects described above, the inputs are also instrumented by their lagged values (Arellano and Bover 1995). This specification

[33] The inclusion of household-year fixed effects prevents the direct inclusion of the household-level measure of total agricultural assets. We instead use the crop-specific machinery costs as a proxy for capital usage.

[34] Note that the inclusion of farmer-year and village-crop-year fixed effects also absorbs any changes in the village-level average TFP arising from the land reform, thus allowing us to consistently estimate the production function while using data on the entire sample period.

further allows input choices to be correlated with the unobserved e_{icvt}. The coefficient estimates are generally similar to those obtained under the simpler specification, but are less precisely estimated and occasionally negative. The estimated TFP residuals are highly correlated with the estimated TFPs obtained from the simpler fixed effects specification, with the correlation coefficient being 0.993.

Lastly, we examine the sensitivity of the estimates to adopting a more general trans-log production function, which also includes interactions between the inputs in the regression. The results indicate that the trans-log function does not fit the data any better than the Cobb-Douglas function; the R-squared in the Cobb-Douglas regression is 0.968, compared to 0.969 in the case of the translog function.[35] The estimated TFP residuals from the two regressions are also very highly correlated, with a correlation coefficient of 0.97. It appears, therefore, that the Cobb-Douglas is a reasonable approximation to the true production function in our data.

4.4 Evidence for land reallocation

The observed increases in land rental activity and in aggregate output and productivity are consistent with the hypothesis that the reduction of transaction costs associated with leasing land increased aggregate output by allowing for a more efficient allocation of factors across producers. In this section, we directly examine how the allocation of land has changed following the RLCL.

We begin with a descriptive examination of land reallocations. We focus on two hypotheses suggested by theory: In the absence of misallocation, the marginal product of land would be equalized across producers, and secondly, land assignments should be positively related to TFP. We examine these hypotheses in turn. Panel A of Figure 3 graphs household farm area against the household's pre-reform marginal product of land, 2 years prior to and 2 years after reform, where the household's pre-reform marginal product of land is calculated by taking the household-crop-specific average of marginal revenue product of land over pre-reform years and then averaging these over crops. It is important to emphasize that in this exercise marginal products are being held fixed at pre-reform levels, and only the land allocation is changing. After the reform, we observe

[35]These results are available upon request.

that high-MP farmers cultivate more land while low-MP farmers cultivate less, consistent with the hypothesis that land is being transferred to those who are the most efficient at the margin.[36] Panel B of Figure 3 shows the pattern of land reallocation with respect to household TFP.[37] Prior to the reform, farm area correlates only weakly with TFP, whereas the efficient allocation of land (derived in Section 4.7) requires high-TFP farmers to cultivate more land than low-TFP farmers; after the reform, we do indeed observe high-TFP farmers cultivating more land, with low-TFP farmers cultivating less. Thus, the descriptive evidence strongly suggests an efficiency-improving reallocation of land following the RLCL.

We now establish this result more rigorously by testing for within-village land reallocations, using the following estimating equation:

$$l_{icvt} = \alpha + \sum_{j=1}^{4} \beta_j \psi^j_{icvt} + \sum_{j=1}^{4} \delta_j (PostReform_{pt} \times \psi^j_{icvt}) + \sum_{j=1}^{4} \theta_j (ReformYear_{pt} \times \psi^j_{icvt}) + \eta_i + \eta_c + \eta_t + \epsilon_{ivct} \quad (6)$$

where ψ^j_{icvt} is an indicator for whether the (pre-reform) marginal productivity of farmer i growing crop c in year t is in quartile j of the distribution of pre-reform marginal products in village v.[38] [39] The dependent variable, l_{icvt}, is the logarithm of crop area. The regression includes household fixed effects, crop fixed effects, and year fixed effects. As before, standard errors are clustered at the province level. In this regression, the δ_j coefficients capture the heterogeneous effects of the reform on crop area with respect to marginal productivity.

Figure 4 graphs the estimated coefficients, along with the associated 90% confidence intervals.

[36] Although not directly of interest in our study, the inverse size-productivity observed relationship in Figure 3 is the focus of a large literature, following Sen (1962). The extent to which the relationship reflects market imperfections is still debated (see, for example, Barrett, Bellemare and Hou 2010).

[37] Household TFP is constructed as the household-level average over crops of the fixed household-crop specific component of TFP estimated in Section 4.3.

[38] In the Chinese context, it is reasonable to assume that land is not traded across village boundaries, so that the village is the natural level at which the land market is defined.

[39] An implication of constructing the productivity quartiles on the basis of pre-reform productivity is that we automatically restrict the sample to farmers who were present before the reform (i.e. we do not capture entry). To the extent that entry matters as a channel of reallocation, we may underestimate the amount of land reallocation occurring. As we show in Section 4.6.2, however, entry and exit make only a small contribution to aggregate productivity growth following the reform.

The corresponding regression results are reported in column 1 of Table A.10. The reform effects show a clear pattern across productivity quartiles; there is a sharp reduction in cultivated area for those in the bottom quartile, and corresponding increases in farm area among those in the top two quartiles.[40] These results are a striking confirmation of efficiency-improving land reallocation following the RLCL. In the panels of Figure 5, we plot the leads and lags of reform effects, separately for each of the four productivity quartiles. The estimated effects are consistent with the overall pattern in Figure 4, and there is little evidence of any significant land reallocations occurring prior to the introduction of the RLCL.

We now carry out a number of checks on this result. A potential concern with the interpretation of the results is that differences in measured productivity may reflect differences in soil quality across farms that are unobserved to us, whereas the hypothesis we would like to test is that land reallocation is driven by differences in productive ability.[41] To address this concern, we directly control for interactions between the reform dummies and the farm-average soil quality measure elicited in our own survey for a sub-sample of 10% of NFP households. The results are reported in column 2 of Appendix Table A.10. Reassuringly, the results indicate that even after controlling for soil quality, land reallocation follows the gradient of measured productivity.

We also examine whether the results are robust to using alternative measures of productivity. We start by estimating the specification in equation 6 above, but this time using the time-invariant component of TFP from the production function regression, ϕ_{ic}, to construct the productivity quartiles. The results are reported in column 3 of Appendix Table A.10. Before doing so, we adjust the TFP measure for the fact that the fixed TFP component is being estimated off of a different number of observations for different producers (due to the sample being unbalanced as a result of

[40] Because we expect land to be reallocated from less-productive to more-productive farmers, we have examined how land allocation changes at different parts of the productivity distribution. A qualitatively similar result is also obtained by treating productivity as a continuous variable. The results are reported in Appendix Table A.11 where we find that there is a negative relationship between farm area and productivity in the pre-reform period, and the slope of this relationship becomes less negative as a result of the reform, implying the same pattern of reallocation.

[41] For instance, if the reform increased the productivity of some farmers so as to move them from the third to the fourth quartile, and if these farmers had large amounts of land to begin with, this might increase the average crop area associated with farmers in the top quartile, even if no actual reallocation were to accompany the increase in productivity. Changes in measured productivity may also be triggered by increased land transactions: If the land transactions that occur after the reform systematically transfer either high or low quality plots from low to high productivity farmers, such transfers would have the effect of changing measured productivity (since land quality is unobserved to the econometrician and therefore enters into measured productivity).

changes in sample composition as well as households moving into and out of crops). We obtain this adjustment by estimating the fixed component by applying a Bayesian shrinkage procedure, as in Chetty, Friedman and Rockoff (2014). The results are presented in column 3 of Appendix Table A.10. For comparison, we also present the results using the unadjusted TFP fixed effects, in column 4.

Lastly, we replace the productivity quartiles with quartiles constructed from the distribution of pre-reform agricultural profits per mu, where agricultural profits are calculated in a simple way by subtracting costs from revenue.[42] The regression results are reported in column 5 of Appendix Table A.10. Overall, we find evidence of efficiency-improving reallocation, across all the alternative measures of productivity. We conclude that this is indeed a robust result, and not an artefact of the way in which we are measuring productivity.

In addition to the amount of land cultivated, we can also examine whether the reform had heterogeneous effects on rental activity. Because the outcome variable, the amount of land rented in, is only available at the household-year level (and not the household-crop-year level), we estimate an analogous household-level regression to equation 6, with household and year fixed effects, where the productivity quartiles are based on (pre-reform) household average marginal productivity, measured as before. The coefficients on the productivity quartiles are graphed in Figure 6, along with the associated 90% confidence intervals. The pattern of rental activity is again consistent with the previous evidence on land reallocation. The increase in renting-in following the RLCL is mainly concentrated among households in the top quartile of the distribution, with no statistically significant increase in renting-in among households with low levels of productivity. Furthermore, we break down renting in behavior by both productivity quartiles and by the leads and lags in Figure 7. These results confirm that there are no significant changes in renting in land before or after the reform for the lower quartiles; but that there is a large and significant shift in renting in land for the two upper quartiles in the periods immediately following reform implementation.

[42]This calculation assumes that the opportunity cost of family labor applied to the farm is zero. While we could estimate wage rates using information on the cost and number of days of hired labor in the data, in nearly one-third of villages in our sample, zero households in the sample hire any labor in the pre-reform years and we would lose a substantial portion of the sample.

4.5 Effects on Machine Use and Labor

In Table 4, we examine whether land reallocations are accompanied by reallocation of machinery and labor. Column 1 reports the results from estimating equation 6 using the IHS function of value of total agricultural assets as the dependent variable. Because the dependent variable is measured at household level, we define the quartiles in this regression based on household-level marginal product (as defined in the previous section). In column 2, we use the IHS function of machinery cost as the dependent variable - this is a crop-specific variable and the quartiles are accordingly based on crop-specific marginal productivities.

We do not observe significant evidence of an increase in either of the two capital measures complementing the increase in land in the higher quartiles of the productivity distribution. We do however find a significant increase in the use of hired labor in the highest quartile (column 3 of Table 4); the anecdotal evidence presented in Section 2 suggests the increased demand for labor among these farmers may be met by an increase in wage labor supplied by low-productivity farmers.[43]

It is also interesting to examine whether there is an increase in local off-farm (i.e. non-agricultural) and migrant labor supply on the part of the low-productivity farmers. Columns 4 and 5 present estimates with the dependent variables being the IHS function of migrant labor days (column 4) and the IHS function of off-farm labor days (column 5).[44] There are no significant changes in the probability of migration or off-farm labor activities after the reform. We speculate that the lack of evidence of any increases in migration following the property rights reform may be explained at least in part by the institutional barriers to migration in China.[45]

4.6 Decomposing the Productivity Gains from the Reform

The previous evidence suggests that land reallocation may be an important source of aggregate productivity gains following the reform. We now attempt to quantify the contribution of the real-

[43]Because there are no survey questions on labor supplied for agricultural wage work, we cannot test this hypothesis directly.

[44]Note that unlike machinery costs and hired labor inputs, which are reported at the household-crop level, these are household-level regressions because migration is measured at the household level.

[45]See Kinnan, Wang and Wang (2018) for an overview of the household registration (hukou) system in China.

location channel to the observed increase in aggregate productivity. We implement two approaches to decompose the productivity gains from the reform.

4.6.1 Decomposition of aggregate productivity

First, we consider the following decomposition of aggregate productivity suggested by Olley and Pakes (1996):

$$I_{vt} = \sum w_{ict}\phi_{ict} = E(\phi_{ict}|v,t) + \sum (w_{ict} - E(w_{ict}|v,t))(\phi_{ict} - E(\phi_{ict}|v,t)) \quad (7)$$

where the index I_{vt} denotes village-level aggregate TFP at time t, and is defined as the output-share weighted average of log TFPs, with w_{ict} denoting the real output-share of the i-th farmer-crop. The aggregate index is decomposed into two components: a component that measures the productivity of the average farmer-crop, $E(\phi_{ict}|v,t)$, and a covariance term that measures the extent to which size (measured here by output) is correlated with TFP.[46] As Bartelsman, Haltiwanger and Scarpetta (2013) show, the Olley-Pakes covariance term is a reliable measure of the efficiency of resource allocation that tends to vary systematically with changes in the economic regime. Our expectation is that land reform should result in an increase in the Olley-Pakes covariance, as land gets reallocated from low- to high-productivity farmers.

We can now estimate reform effects on the separate terms of the decomposition to understand the source of aggregate productivity growth. Columns 1 through 5 of Table 5 report the regression results.[47] In column 1, the point estimates indicate an aggregate TFP increase of approximately 7.6% (significant at the 5% level), which is consistent in magnitude with the observed increase in total output. As shown in column 3, there is a significant increase in the covariance term, which accounts for approximately 88% of the increase in aggregate TFP. The remaining increase in aggregate TFP is attributed to an increase in average productivity, although this effect is both small in magnitude and not statistically distinguishable from zero (column 2).

[46] The "covariance" term in this decomposition is really N times the covariance between output shares and TFP, where N is the number of household-crops. That is, the decomposition can be seen to be a simple rewriting of the expression for the covariance between output shares and TFP.

[47] The corresponding estimates showing the leads and lags are presented in Appendix Table A.4.

To shed more light on the reallocational flows, we apply a decomposition of the covariance term:

$$\begin{aligned} cov^{OP} &\equiv \sum(w_{ict} - E(w_{ict}|v,t))(\phi_{ict} - E(\phi_{ict}|v,t)) \\ &= N * cov(w_{ict}, \phi_{ict}) \\ &= N * E[cov(w_{ict}, \phi_{ict}|c)] + N * cov[E(w_{ict}|c), E(\phi_{ict}|c)] \end{aligned} \tag{8}$$

where, to reduce notational clutter, we have now suppressed the conditioning on v and t. The number of household-crops is denoted by N. The equation above shows that the Olley-Pakes covariance term can be decomposed into within-crop and across-crop components.

Columns 4 and 5 of Table 5 report the results from regressing these components on the reform indicators. More specifically, columns 4 and 5 display results for the within-crop and across-crop components, respectively. The point estimates suggest that between-crop reallocation is similar in magnitude to within-crop reallocation, although only the latter is statistically significant.[48] It appears therefore that an important source of productivity gain from the reform arises from a correction of both within-crop and across-crop distortions.[49]

Lastly, the panels in Figure 8 plot the coefficient and associated confidence intervals corresponding to the leads and lags of reform effects, for each of the terms of the Olley-Pakes decomposition. The corresponding estimates are in Appendix Table A.4. We do not observe any significant changes in any of the variables prior to the reform.

4.6.2 Decomposition of aggregate productivity growth

The Olley-Pakes decomposition of aggregate productivity allows us to look at the effect of the reform on a static measure of allocational efficiency. Improvements in this static measure are taken to imply a reallocation of market shares and inputs from less- to more-productive farms. However, a more direct way to assess the contribution of reallocation is by considering a decomposition of

[48]This result stands in contrast with findings from the firm literature, where within-industry reallocation tends to dominate (e.g. Davis and Haltiwanger 1999, Foster, Haltiwanger and Krizan 2001), but the present setting differs not only in that we are studying the agricultural sector which may exhibit fundamentally different patterns of reallocation from the manufacturing sector, but also in that we are arguably studying a transition between steady states.

[49]To be clear, the increase in between-crop reallocation most likely reflects reallocation across farmers who are growing different crops, rather than between-crop reallocations of land within farms.

aggregate productivity growth proposed by Baily, Hulten and Campbell (1992):

$$\begin{aligned}\Delta I_{vt} &= \sum w_{ict}\phi_{ict} - \sum w_{ic,t-1}\phi_{t-1} \\ &= \sum_{i\in S} w_{ic,t-1}(\phi_{ict} - \phi_{ic,t-1}) + \sum_{i\in S} \phi_{ict}(w_{ict} - w_{ic,t-1}) + \sum_{i\in E} w_{ict}\phi_{ict} - \sum_{i\in X} w_{ic,t-1}\phi_{ic,t-1}\end{aligned} \tag{9}$$

where S denotes the set of farms surviving from period $t-1$ to t; E denotes the set of new entrants in period t; and X denotes the set of farms that exited at the end of period $t-1$. The first term on the right-hand side of equation 9 is referred to as the within-farm component of productivity growth, as it reflects the contribution of within-farm changes in productivity. The second term reflects the contribution of market share reallocation across farms, and is referred to as the between-farm component. The last two components capture the contributions of entry and exit.

We now estimate the effect of the reform on each of the components of this decomposition in order to shed light on the sources of productivity growth. The difference-in-differences estimates are reported in Columns 6-10 of Table 5. We find that the reform has resulted in a significant effect on aggregate productivity growth, due primarily to a higher rate of between-farm reallocation. Of the remaining sources, the effect of within-farm productivity improvements is the largest in magnitude (but not statistically significant), with entry and exit contributing little to overall productivity growth. The importance of the between-farm component is consistent with our previous results using the Olley-Pakes decomposition, and underlines the key role of reallocation following the reform.

As before, we also estimate leads and lags of program effects (Appendix Table A.4), with the associated coefficients and confidence intervals graphed in Figure 9. In the context of the BHC decomposition, it is particularly interesting to look at dynamic treatment effects since the decomposition refers to *changes* in productivity. *A priori*, we might expect the RLCL to have a short-term effect on productivity growth by correcting the existing misallocation of land, leading to a new steady-state allocation. Indeed, we do observe that the magnitude of the reform effect declines slightly after the second year post-reform, although we should note that this decline is not statistically significant.

4.7 Benchmarking effect sizes

The evidence indicates that the RLCL led to land reallocation and aggregate productivity increases. In this section, we attempt to place our estimates in the context of the existing literature on misallocation. A distinction between our estimates and those of the misallocation literature is that we are effectively estimating the efficiency gains corresponding to a partial reduction in misallocation, whereas the literature has generally considered the effect of removing all misallocation. We make two observations in this regard. First, the implied marginal effect in our setting is clearly substantial: We observe a 10% increase in aggregate efficiency associated with a 7% increase in the amount of land rented out. This is a large effect, but is not inconsistent with the pre-reform dispersion in marginal productivity: For instance, in the pre-reform period, producers at the 90th percentile are more than 6 times as productive (at the margin) as producers at the 10th percentile, suggesting that even small reallocations of land could have a significant impact on aggregate efficiency.

Second, our estimates beg the question of how large the potential efficiency gains from full reallocation would be. To answer this question, we follow the approach in Adamopoulos et al (2017) and compare the actual distribution of land to the efficient distribution of land. We assume that each farmer's production technology is given by:

$$Y_i = \Phi_i L_i^{\alpha} \tag{10}$$

where Φ_i denotes farmer i's total factor productivity, and L_i denotes land. Aggregate output in the village economy can be written in the form:

$$Y = (\sum_i \phi_i s_i^{\alpha}) L^{\alpha} \tag{11}$$

where s_i denotes farmer i's share of land. The efficient land shares are calculated by solving the planner's problem of allocating the total land (L) between the farmers of heterogeneous TFP, so as to maximize total income. The efficient allocation can be shown to be given by:

$$s_i^* = \frac{\Psi_i}{\sum \Psi_i} \tag{12}$$

where $\Psi_i = \Phi_i^{\frac{1}{1-\alpha}}$.

An implicit assumption in the foregoing exercise is that individual TFPs do not change. In particular, this rules out a situation in which the reallocation of land itself changes farmer TFP. As the results of our previous decomposition exercises (Table 5) indicate, this assumption is indeed reasonable.

We now use our estimates of TFP to compute the efficient allocation and the efficient quantity of output, Y^*, and compare it to the observed quantity Y^{obs}. The counterfactual output gain $\frac{Y^*-Y^{obs}}{Y^{0bs}}$ averages 0.73 across villages and pre-reform years, with a standard error of 0.07. In other words, this calculation indicates that moving to the efficient allocation of land would on average increase output by 73% (with a 95% confidence interval of 62.38% to 89.20%).[50]

5 Responses to Price Changes

Land reform may not only correct the allocation of land in a static sense, but may also increase the responsiveness of land allocation to productivity shocks. In this section, we consider price changes (across different crops) as a particular type of shock. The idea that the land reform may have increased farmers' ability to respond to such shocks is also suggested by our finding that there is more across-crop reallocation following the reform. We test this hypothesis by examining how the reform interacts with crop price changes to impact the allocation of land across crops.

Specifically, we estimate the following equation for a crop c in household h, village v, province p and year t:

$$\begin{aligned} Y_{hcvpt} = \alpha + \beta_0 Price_{c,t-1} * PostReform_{pt} + \beta_1 Price_{c,t-1} * ReformYear_{pt} + \\ \beta_2 PostReform_{pt} + \beta_3 ReformYear_{pt} + \nu_{ct} + \gamma_{ic} + \epsilon_{hcvpt} \end{aligned} \tag{13}$$

A key concern with these regressions is whether local agricultural prices are exogenous to the reform. Thus, the measure of prices that we use is the lagged, demeaned U.S. crop price index for

[50]This would be a substantial gain, although we should note that this estimate is somewhat larger than the 41% efficiency gain computed by Adamopoulos et al (2017) from removing both land and capital distortions in Chinese agriculture (using data over the period 1993-2002, and a different method of TFP estimation).

$Price_{c,t-1}$. This variable is a crop-specific price index where each crop price in 2001 is set as 1. We use a one-year lag for multiple reasons. Agricultural decisions are often months in advance of harvest in which case the relevant price on which farmers can make planting decisions is from the prior year. This also sidesteps the issue that contemporaneous prices may reflect endogenous agricultural production decisions. The regressions also include crop-year fixed effects and individual-crop fixed effects. All of the regressions are clustered at the province level. The outcomes here are those that are measured at the household-crop-year level by the survey.

We also estimate the equation with the leads and lags around the implementation of the reform:

$$Y_{vcpt} = \alpha + \sum_{k=-3}^{2} (\beta_k Price_{c,t-1} * Reform_{pt,k} + \eta_k Reform_{pt,k}) + \nu_{ct} + \gamma_{ic} + \epsilon_{hcpt} \quad (14)$$

where $Reform_{pt,k}$ is an indicator variable that indicates the period k relative to the reform implementation in the province. In the regressions of equation 14, the sample is restricted to the six waves around the implementation of the reform. The reference year is the one prior to the reform implementation ($k = -1$).

In addition to using the U.S. crop prices in reduced form equations, we also use them as instruments for Chinese province-level crop prices. The first stage is shown in Table 6, where the dependent variable in column 1 is the price of the four cash crops in Chinese provinces for which we have price data (oilseed, sugar, cotton, and tobacco).[51] The dependent variable in column 2 is the price of the five staple crops (corn, potato, rice, soybean and wheat). The results in column 1 indicate that price movements for cash crops in China move closely with changes in U.S. prices, confirming that much of the variation in these prices is driven by global markets. In contrast, the Chinese prices for staple crops do not move as closely with U.S. prices. This highlights some key concerns about staple crops in this analysis. First, if the Chinese government sometimes intervenes in these markets, agricultural production decisions in these crops may be less likely to be driven by market dynamics and market prices (Deng 2009). Second, we may have some concerns that the

[51] We exclude fruits and vegetable products, which are cash crops, because the NFP data lumps questions on all fruits and vegetables into two categories so we are unable to match these broad categories to a single agricultural price. We also do not have price data for hemp, so we exclude this from the analysis. As shown by the number of observations in Appendix Table A.7, there is much less activity in hemp relative to all other crops.

timing of the reform at the provincial level responds to local economic conditions, which are largely defined by output in staple crops. Third, we may be concerned about the reverse, that crop prices are driven by the reform. Thus, we look at the estimates of Equations 13 and 14 for a sub-sample of only cash crops.

5.1 Results on Price Changes

In Table 7, we estimate equation 13 where the outcomes are different measures of land used for cultivation of cash crops. The dependent variable in columns 1 and 2 is the inverse hyperbolic sine function of the amount of land cultivated in each crop, while it is an indicator for any land cultivated for each crop in columns 3 and 4. In both the reduced form estimates (column 1) and instrumental variables estimates (column 2), we see that the reform leads to more land under cultivation of a specific crop in areas with positive changes in prices of that crop in the years after the reform is implemented. In the reduced form estimates, a standard deviation increase in the price index of a crop (0.45) corresponds with a 2.7 percent increase in the area a household allocates to that crop after the reform relative to before the reform. The estimate is significant at the 10% level. In column 3, we also see a 1.4 percent increase in the probability that any land is used for production in that crop corresponding to a standard deviation change in crop price, and these estimates are significant at the 5% level. Columns 2 and 4 show that the magnitude of responses to prices is higher when we use U.S. crop prices as instruments for Chinese prices and these estimates are significant at the 10% level or higher. In all of the specifications, the impact of price changes in the year of implementation (β_1) is positive but only significant at the standard levels when the outcome is the inverse hyperbolic sine function of area.

In the estimates of equation 14 presented in Appendix Table A.12, we see the sign and the significance of the coefficients shift immediately following the implementation of the reform for both outcomes. These results suggest that by allowing land to be legally rented across villagers, households are better able to optimize the amount of land devoted to the production of different crops in response to price changes.

6 Conclusion

Our paper examines the importance of property rights institutions for the efficient allocation of agricultural land. By exploiting provincial-level variation in the implementation of a central reform in China that formalized leasing rights, we are able to evaluate the impact of the rural land reform. We find that the reform increased leasing transactions and led to increases in agricultural output of 8%. We show that this increase was driven by a significant reallocation of land from less- to more-productive farmers.

In many developing countries, much emphasis continues to be placed on land structures with communal ownership or on policies that aim for equality in the distribution of land across farmers rather than on allowing free exchange in land markets. Our research demonstrates that households cannot fully solve the contracting problem in informal ways and that legal protections for exchange rights are important for the efficient allocation of resources, including land and labor. An important related question, that we leave for future research, is the potential trade-off between efficiency and equity accompanying such changes in institutions.

While the observed increases in output and productivity are by no means small, it is worthwhile to place them in the context of the scale of the productivity differential between China and developed countries: For example, FAO data indicate that in 2003 cereal yields in Belgium were 75% higher than in China. Indeed, diagnostic measures of misallocation suggest that the potential gains from full reallocation could be approximately 73%. One explanation for the relatively limited impact of the RLCL may lie in the fact that land market rigidities only constitute one set of frictions in the agricultural sector, so that substantial gains remain yet to be realized. A related explanation is that trading in agricultural land markets is associated with a unique set of coordination problems that cannot be solved by traditional markets (Bryan et al 2017); in this view, reducing bilateral transaction costs may not be sufficient to induce the full extent of reallocation.[52]

The estimated marginal effects of the RLCL are large. As our benchmarking exercise indicates,

[52] It may also be the case that the measures of misallocation used in the literature tend to overstate the extent of efficiency gains achievable through reallocation, at least when they are applied to the agricultural sector, a point that is emphasized by Gollin and Udry (2017). See also Bils, Klenow and Ruane (2017) and Rotemberg and White (2017) who show, in the context of manufacturing data, that measurement error and data inconsistencies tend to inflate the importance of misallocation.

we observe a 10% reduction in misallocation resulting in an 8% increase in aggregate output, implying a substantial productivity differential between the farmers who are leasing-out and leasing-in land. This differential is indeed consistent with the distribution of marginal productivities in the data: for instance, in the pre-reform period in our data, the farmers at the 90th percentile of the distribution are more than 6 times as productive (at the margin) as the farmers at the 10th percentile. The potential for large marginal effects is also implied in Hsieh and Klenow (2009) who find the 90-10 ratio in Chinese manufacturing in 2005 is approximately 5:1. An important contribution of our paper is to show evidence for such reallocational gains in the context of an actual policy reform.

Despite a unique history and set of land institutions, agricultural production in China is typical of a number of developing countries in many important aspects, most notably the predominance of small-holder farms. The median farm is small (under 2 acres), and is largely operated using household labor, with rates of mechanization being low. Therefore, this is an instructive setting in which to study the effects of easing land market frictions. At the same time, it is important to situate the results within the specific context of agricultural production in China. Although communal ownership of land is a common feature of a number of agrarian societies, the specific norms and institutions governing the use of and ability to transact in land tend to vary widely, and it may be difficult to draw generalizable conclusions from our results. Second, our study specifically pertains to the formalization of leasing rights, but this is only one of a host of land-related policies that have been attempted in such countries (other policies include land titling, restrictions on land sales, and tenancy control), each of which is associated with a distinct set of efficiency and equity considerations. Third, the success of land reforms hinges on the perceived (as well as actual) authority of the government. As Pande and Udry (2006) note, *de jure* property rights with respect to land may (and frequently do) differ significantly from *de facto* rights which usually derive from customary law. Thus, the formalization of leasing rights may have either weaker or stronger effects in other contexts, depending on the institutional setting.

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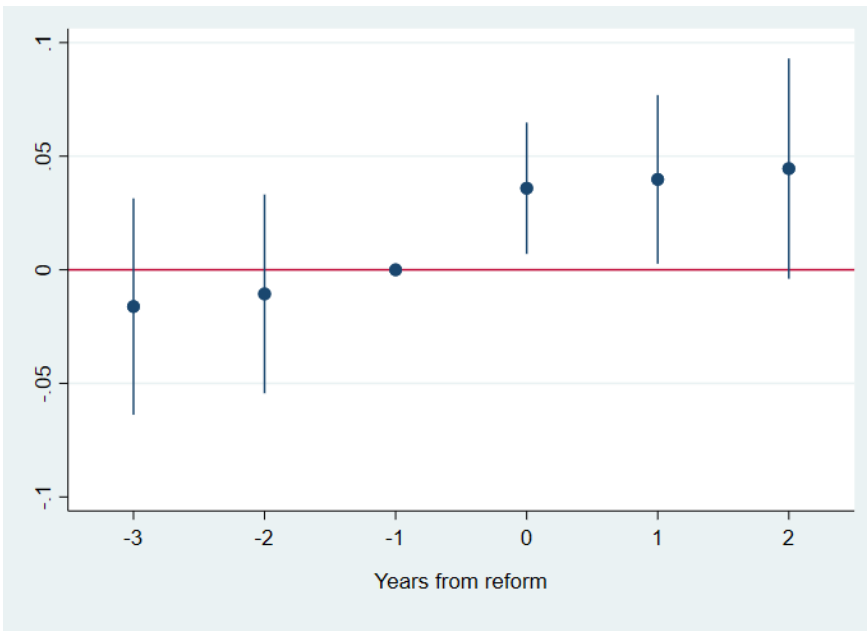
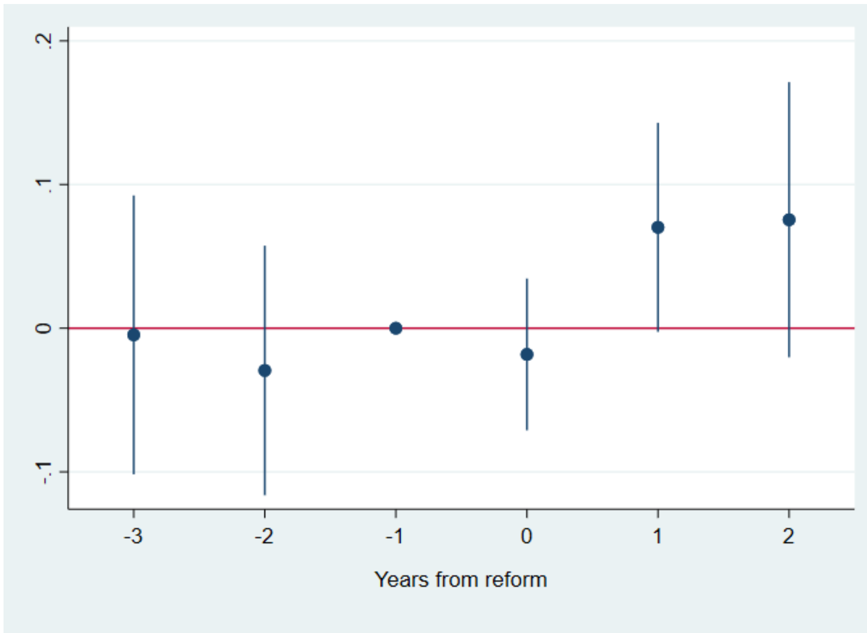
Figure 1: Leads and Lags of Reform Effects on Land Renting



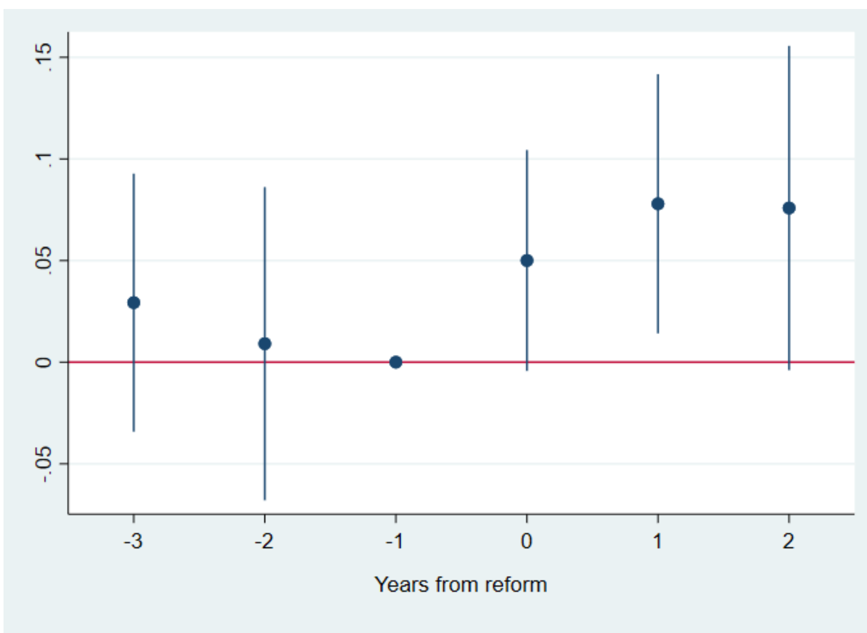
Notes: The figure plots the coefficients and associated 90% confidence intervals from estimating the leads and lags regression in equation 2, where the dependent variable is the IHS function of land rented in. All effects are relative to the year prior to the reform ($k = -1$).

Figure 2: Leads and Lags of Reform Effects on Aggregate Revenue and Productivity

(a) Aggregate Revenue



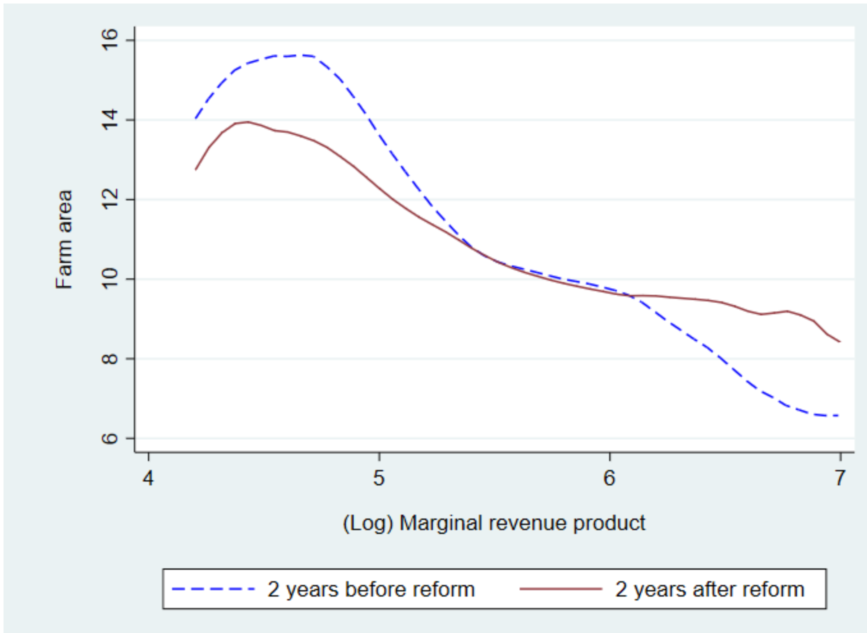
(b) Aggregate Revenue per Mu



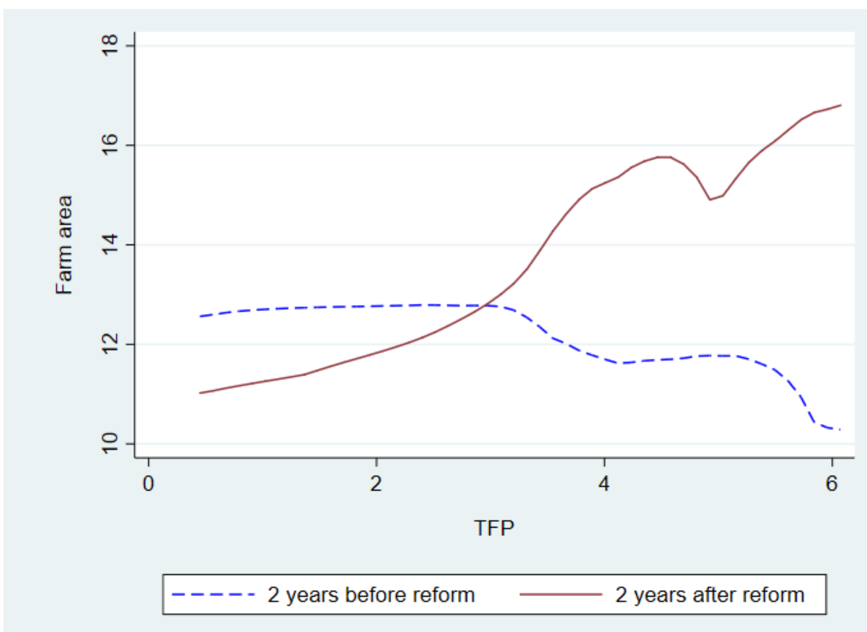
Notes: The panels in the figure plot the coefficients and associated 90% confidence intervals from estimating the leads and lags of reform effects, where the dependent variables are village-level agricultural revenue and village-level agricultural revenue per mu (both in logarithms). All effects are relative to the year prior to the reform (k=-1).

Figure 3: Relationship between Farm Area and Productivity Before and After the Reform

(a) By Marginal Revenue Product

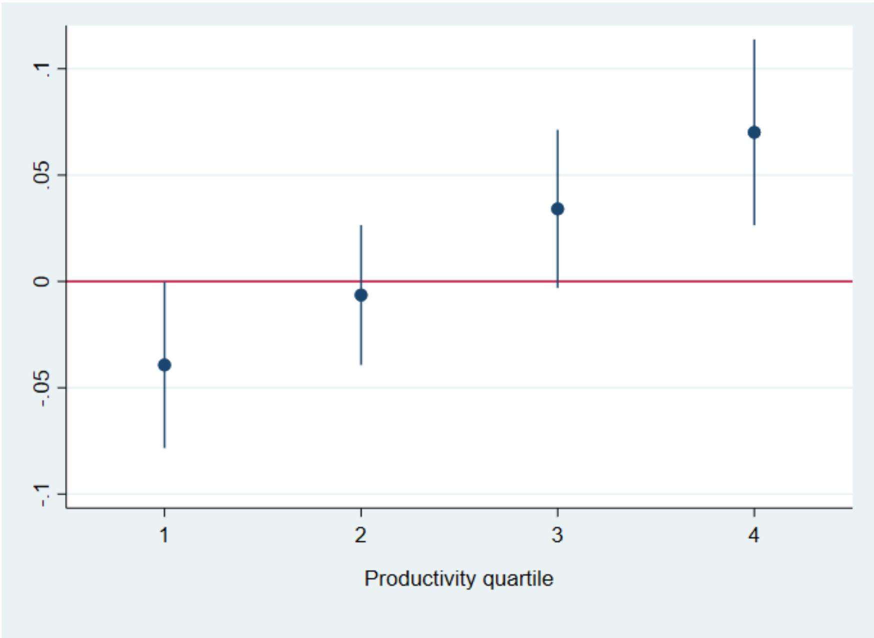


(b) By Total Factor Productivity



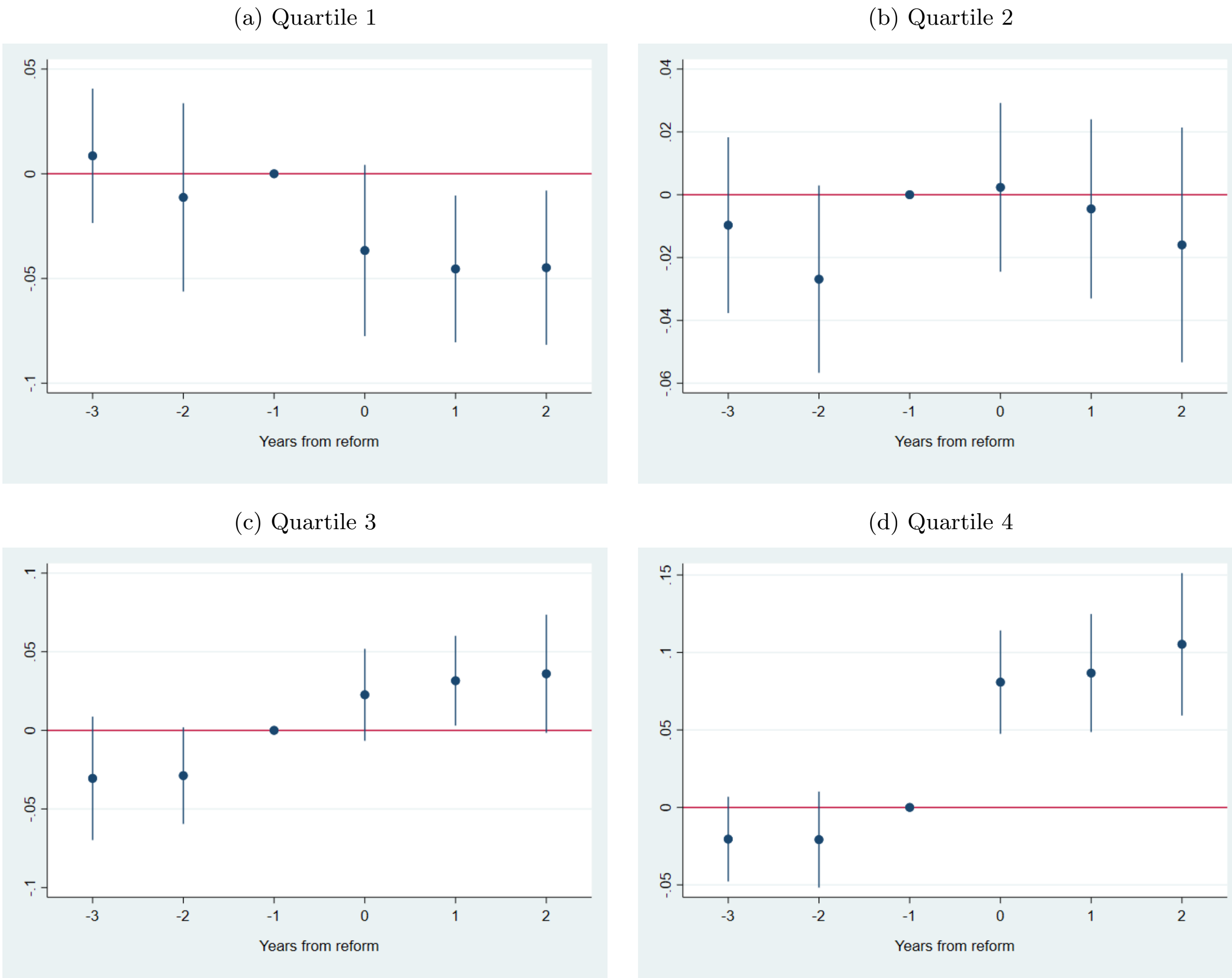
Notes: The figures show local polynomial regressions of household farm area on measures of household-level productivity. In panel (a), household productivity is the households marginal productivity averaged across crops and pre-reform years. In panel (b), household productivity is the households fixed TFP component from the production function regression in equation 4, averaged across crops.

Figure 4: Reform effects on farm area, by productivity quartiles



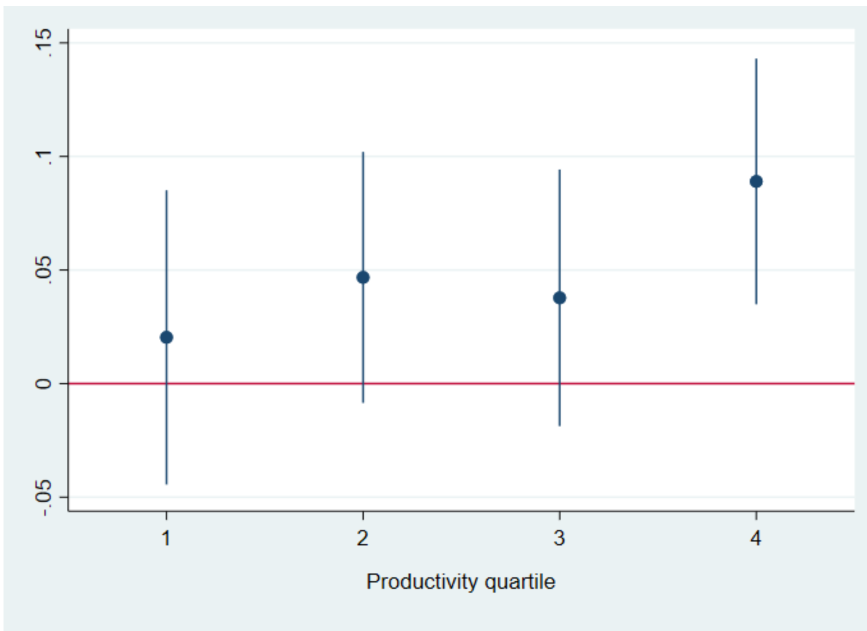
Notes: The figure shows the coefficients and associated 90% confidence intervals from regressing the logarithm of crop-level area on productivity quartiles, where the quartiles are constructed from the village-crop level distribution of pre-reform marginal productivities.

Figure 5: Leads and Lags of Reform Effects on Farm Area, by Productivity Quartiles



Notes: The panels in the figure plot the coefficients and associated 90% confidence intervals from estimating the leads and lags of reform effects separately for each of the marginal productivity quartiles, where the dependent variable is the logarithm of crop-level area. All effects are relative to the year prior to the reform (k=-1)

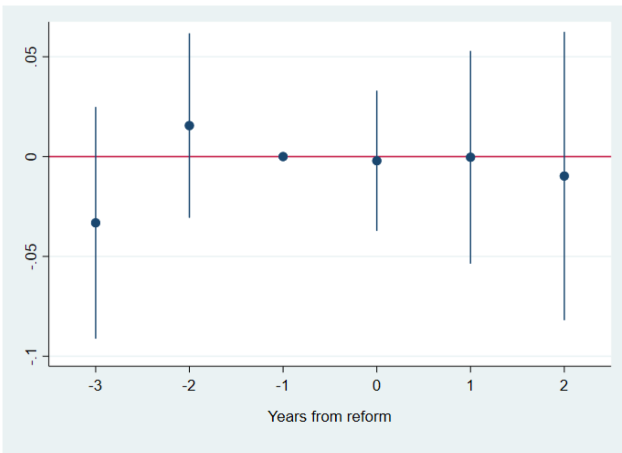
Figure 6: Reform effects on land rented in, by productivity quartiles



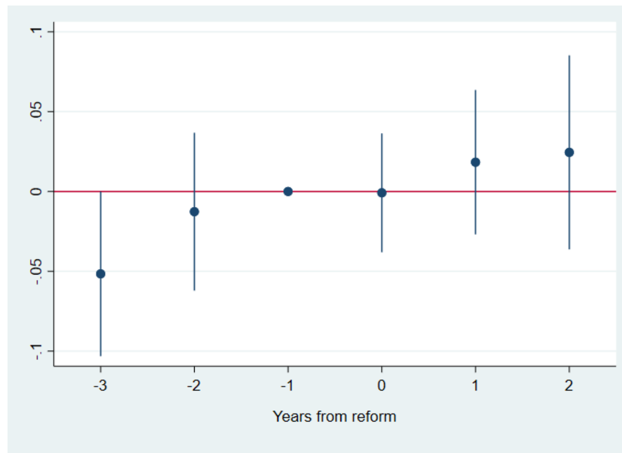
Notes: The figure shows the coefficients and associated 90% confidence intervals from regressing the IHS function of land rented in on productivity quartiles, where the quartiles are constructed from the village-level distribution of household-level fixed TFP.

Figure 7: Leads and Lags of Reform Effects on Area of Land Rented In, by Productivity Quartiles

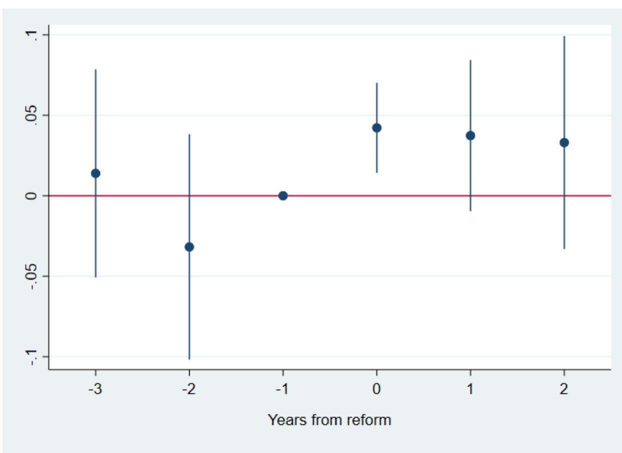
(a) Quartile 1



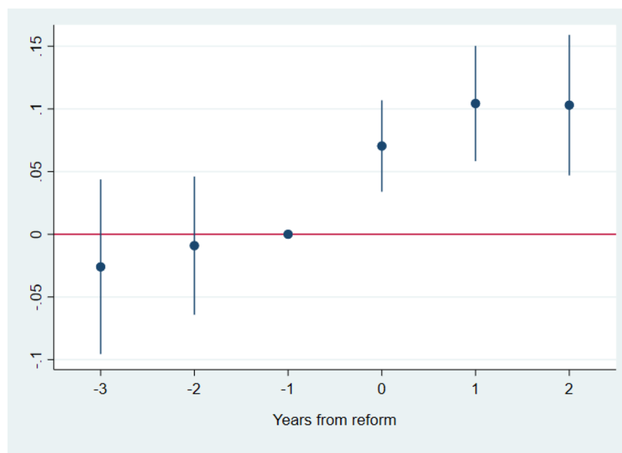
(b) Quartile 2



(c) Quartile 3

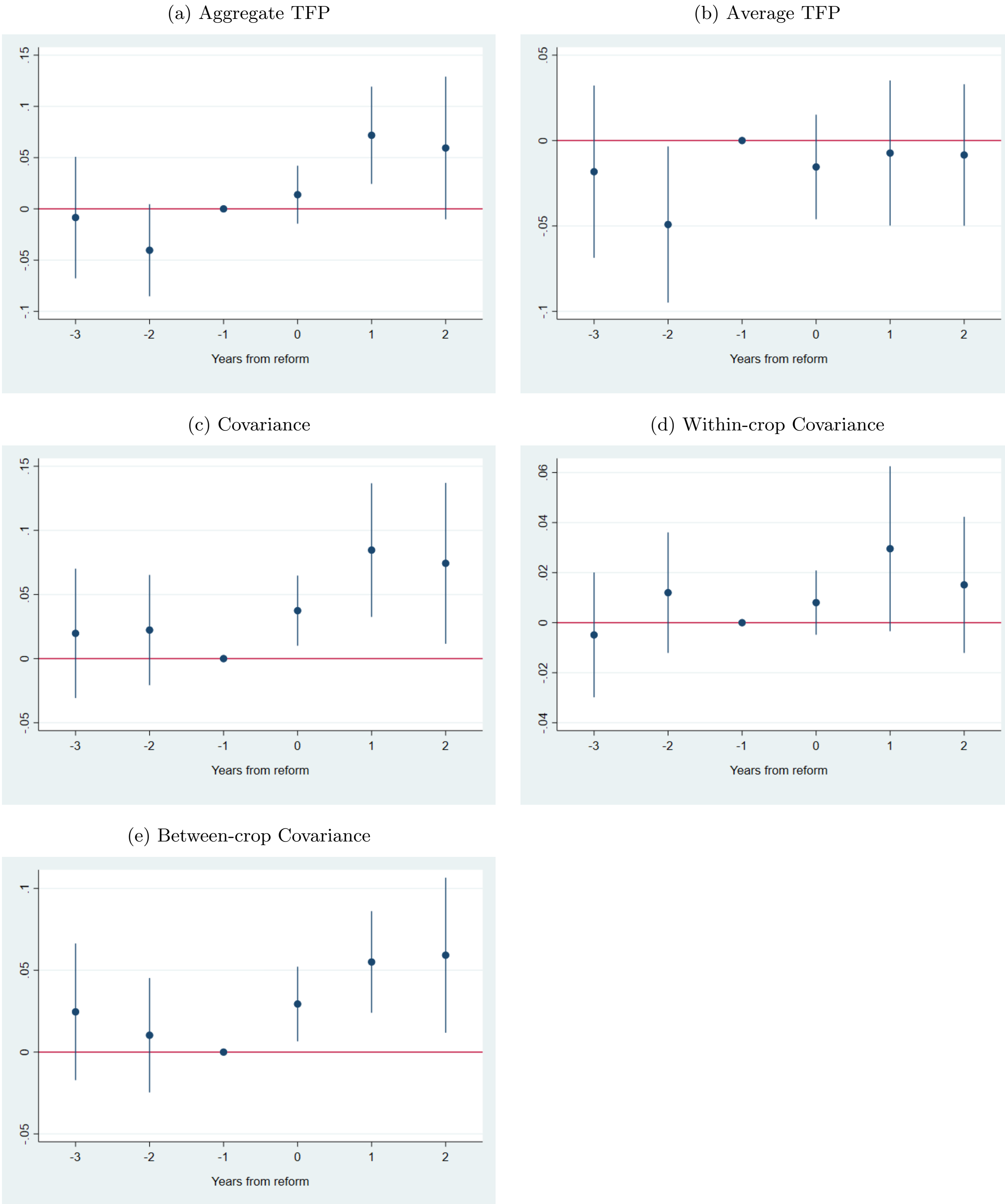


(d) Quartile 4



Notes: The panels in the figure plot the coefficients and associated 90% confidence intervals from estimating the leads and lags of reform effects separately for each of the marginal productivity quartiles, where the dependent variable is the IHS function of the area of land rented in. All effects are relative to the year prior to the reform (k=-1)

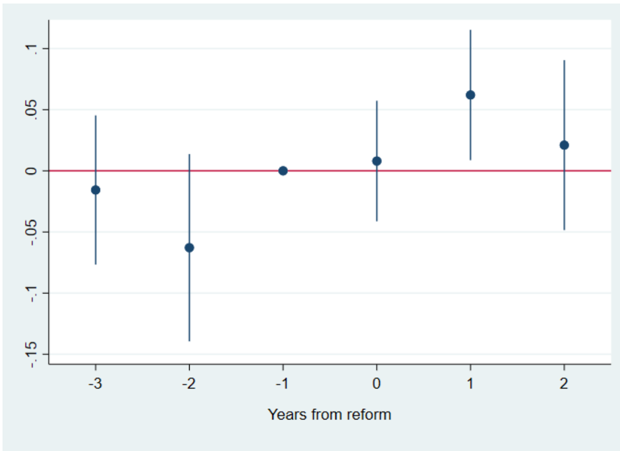
Figure 8: Leads and Lags of Reform Effects on Components of the Olley-Pakes Decomposition



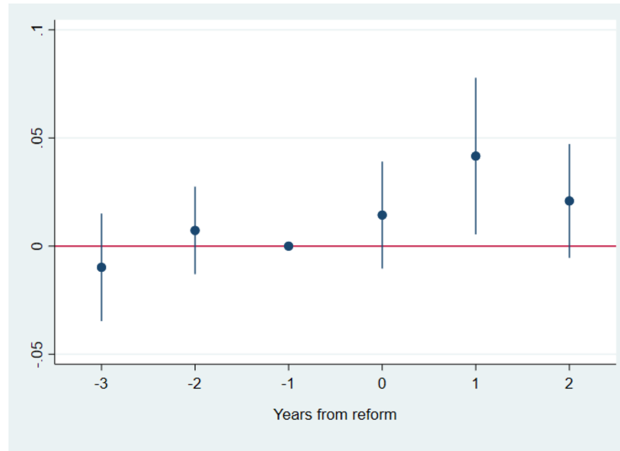
Notes: The panels in the figure plot the coefficients and associated 90% confidence intervals from estimating the leads and lags of reform effects, separately for each of the components of the Olley-Pakes decomposition of aggregate TFP. All effects are relative to the year prior to the reform (k=-1).

Figure 9: Leads and Lags of Reform Effects on Components of Aggregate TFP Growth

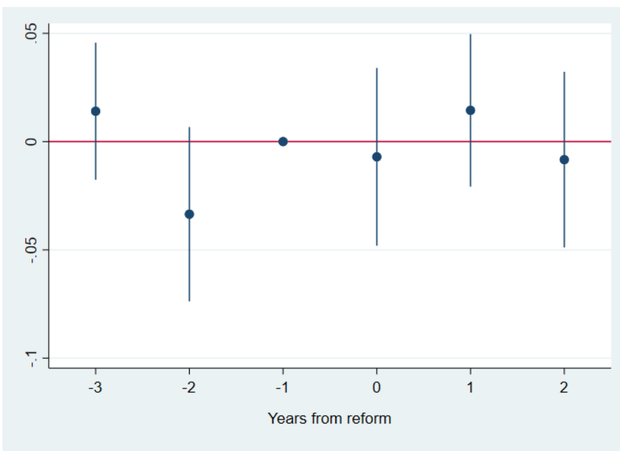
(a) Aggregate TFP Growth



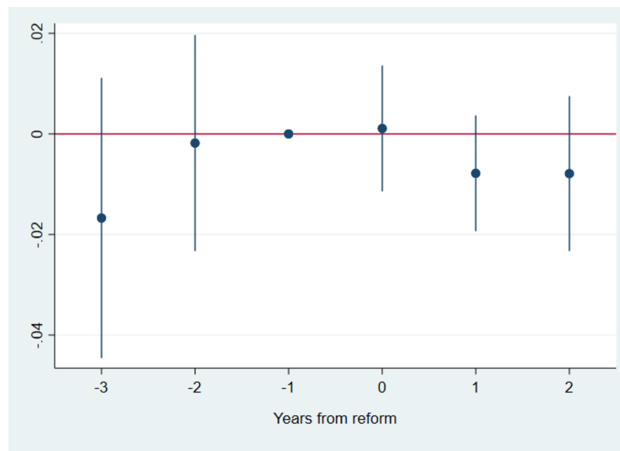
(b) Between Farm



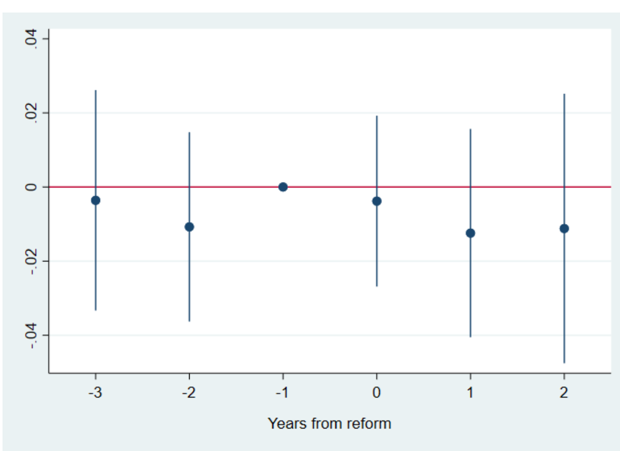
(c) Within Farm



(d) Entry



(e) Exit



Notes: The panels in the figure plot the coefficients and associated 90% confidence intervals from estimating the leads and lags of reform effects, separately for each of the components of the Baily-Hulten-Campbell decomposition of aggregate TFP growth. All effects are relative to the year prior to the reform (k=-1).

Table 1: Summary Statistics

	Mean	Std Dev	Observations
Panel A: Variables by Household-Year			
Area (mu)	12.37	330.5	157315
Land Rented in (mu)	2.227	131.2	157315
Land Rented to Individuals (mu)	0.329	2.433	41577
Land Rented to Firms (mu)	0.0280	0.347	41577
Total Income	28821.2	82331.1	156395
Any Migration	0.552	0.497	156441
Number of Migrant Work Days	233.7	295.5	156441
Any Off Farm Work	0.420	0.493	156441
Number of Off Farm Work Days	126.3	225.9	156441
Total Agricultural Capital (RMB)	1758.8	43781.9	157315
Panel B: Variables by Village-Year			
Aggregate Revenue	1031183.9	21739192.6	2486
Aggregate Revenue per mu	4319.0	155214.2	2485
Panel C: Variables by Household-Crop-Year			
Output	6242.9	1558191.8	412603
Area (mu)	4.717	203.9	412603
Machine Inputs (RMB)	85.74	529.1	412603
Labor Inputs (days)	68.10	4790.3	412603
Other Inputs (RMB)	672.0	74541.3	412603

Notes: Income and expenditures are in real 2002 renminbi. Panel B presents variables that we aggregate to the village level in the analysis. Panel C presents information on variables that are analyzed at the crop level.

Table 2: Impact of Reform on Area Rented In and Rented Out

	Renting In			Renting Out		
	Full Sample	Restricted	Balanced	Total Area	Firms	Individuals
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reform Year	0.0734*	0.0959**	0.110*	0.0726***	-0.0108	0.0680***
	(0.0394)	(0.0462)	(0.0548)	(0.0247)	(0.00657)	(0.0186)
Reform Year	0.0713**	0.0795**	0.0693**	0.0577***	-0.00831**	0.0392***
	(0.0286)	(0.0315)	(0.0327)	(0.0123)	(0.00328)	(0.00929)
Observations	157315	128416	50811	41577	41577	41577

Notes: Each observation is a household-year. The dependent variables in Columns 1-3 are the IHS function of the area rented in, and IHS function of the area rented out in Columns 4-6. The sample in Column 1 includes all household-years. Column 2 restricts the sample to farming households. Column 3 further restricts the sample to households that were present in all periods. Columns 4 shows the total land rented out, column 5 the area rented out to firms and column 6 the area rented out to individuals. The sample in Columns 4-6 includes only the 2009 and 2010 waves. The regressions include household fixed effects, year fixed effects and the agricultural tax rate. Standard errors clustered at province level are reported in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 3: Impact of Reform on Aggregate Output and Productivity

	Aggregate Revenue	Aggregate Revenue per Mu
	(1)	(2)
Post Reform Year	0.0792*	0.104***
	(0.0418)	(0.0290)
Reform Year	-0.00562	0.0454
	(0.0370)	(0.0330)
Observations	2232	2232

Notes: The dependent variables are the logarithms of village-level aggregate revenue and aggregate revenue per mu respectively. The unit of observation is a village-year. All regressions include village fixed effects, year fixed effects, and the agricultural tax rate. Standard errors clustered at province level are reported in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 4: Impact of Reform on Capital and Labor Outcomes by Productivity Quartiles

	Agricultural Capital	Machine Cost	Hired Labor Days	Migrant Days	Off-Farm Days
	(1)	(2)	(3)	(4)	(5)
Post Reform X Quartile 1	0.0519	-0.0195	0.0187	-0.0174	-0.0879
	(0.0927)	(0.0733)	(0.0129)	(0.0774)	(0.0870)
Post Reform X Quartile 2	0.0581	-0.0361	0.0194	-0.0568	-0.0494
	(0.0901)	(0.0813)	(0.0131)	(0.0819)	(0.0944)
Post Reform X Quartile 3	-0.0360	-0.00507	0.0202	-0.0186	-0.0438
	(0.0878)	(0.0804)	(0.0143)	(0.0670)	(0.0924)
Post Reform X Quartile 4	-0.100	0.0263	0.0253*	-0.00675	0.0249
	(0.0920)	(0.0840)	(0.0141)	(0.0510)	(0.0968)
Observations	113209	275540	275540	113209	113209

Notes: In columns 1, 4 and 5, each observation is a household-year. In columns 2 and 3, each observation is a household-crop-year. The dependent variable is the IHS function of the column label. The quartiles refer to the village-level distribution of pre-reform marginal productivity. The regressions include household fixed effects, year fixed effects, the agricultural tax rate, the interaction between each marginal productivity quartile and $ReformYear$. Columns 2 and 3 also include crop fixed effects. Standard errors clustered at the province level are in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 5: Impact of Reform on Components of Aggregate TFP and Aggregate TFP Growth

	Aggregate TFP					Aggregate TFP Growth				
	Aggregate TFP	Average TFP	OP Cov	Within Crop	Between Crops	Aggregate TFP Growth	Between Farm	Within Farm	Entry	Exit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post Reform Year	0.0755**	0.00888	0.0666**	0.0360*	0.0306	0.0567**	0.0441*	0.0118	0.00175	0.000967
	(0.0277)	(0.0203)	(0.0250)	(0.0202)	(0.0199)	(0.0263)	(0.0229)	(0.0197)	(0.00698)	(0.0170)
Reform Year	0.0297	0.00105	0.0286	0.0175	0.0111	0.0245	0.0172	0.00133	0.00548	-0.000494
	(0.0208)	(0.0156)	(0.0180)	(0.0125)	(0.0149)	(0.0245)	(0.0178)	(0.0213)	(0.00764)	(0.0126)
Observations	2232	2232	2232	2232	2232	1964	1964	1964	1964	1964

Notes: The dependent variables in Columns 1-5 are the components of the Olley-Pakes decomposition of aggregate TFP. The dependent variables in Columns 6-10 are the components of the Baily-Hulten-Campbell decomposition of aggregate TFP growth. The unit of observation is a village-year. All regressions include village fixed effects, year fixed effects, and the agricultural tax rate. Standard errors clustered at province level are reported in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 6: Relationship between Chinese Provincial and U.S. Crop Prices

	Cash Crops	Staple Crops
	(1)	(2)
US Crop Price	0.334***	0.0908**
	(0.0561)	(0.0359)
Observations	665	948

Notes: Each observation is a province-crop-year. The dependent variable is the crop price in a Chinese province. The regressions include fixed effects for crop, year and province, and the agricultural tax rate. Standard errors clustered at the province level are in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

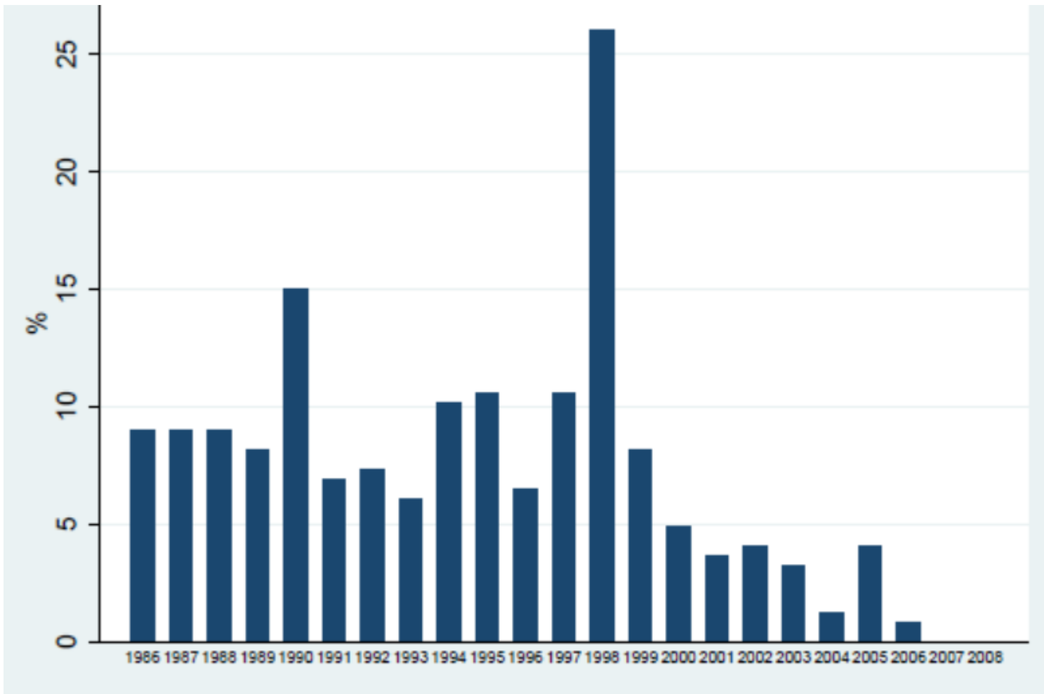
Table 7: Impact of Price and Property Rights Reform on Land Allocation

	IHS Area		I(Any Area)	
	Reduced Form	IV	Reduced Form	IV
	(1)	(2)	(3)	(4)
Lagged Price X Post Reform Year	0.0602*	0.109*	0.0301**	0.0535**
	(0.0321)	(0.0550)	(0.0115)	(0.0200)
Lagged Price X Reform Year	0.0750**	0.230*	0.0267	0.0789
	(0.0294)	(0.113)	(0.0174)	(0.0600)
Post Reform Year	-0.00184	-0.0104	0.00589	0.00173
	(0.0129)	(0.0116)	(0.00892)	(0.00848)
Reform Year	0.00633	0.0234	0.00452	0.00987
	(0.00814)	(0.0139)	(0.00582)	(0.00797)
Observations	444545	444545	444545	444545

Notes: Each observation is a household-crop-year. The dependent variables are IHS area in Columns 1 and 2, and a dummy variable for any area planted in Columns 3 and 4. In Columns 1 and 3, lagged price is measured as U.S. crop price from the previous year. In Column 2 and 4, lagged price refers to the Chinese crop price from the previous year, instrumented with U.S. prices. The sample is restricted to cash crops only. The regressions include controls for agricultural taxes, household-crop fixed effects and crop-year fixed effects. Standard errors clustered at the province level are in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

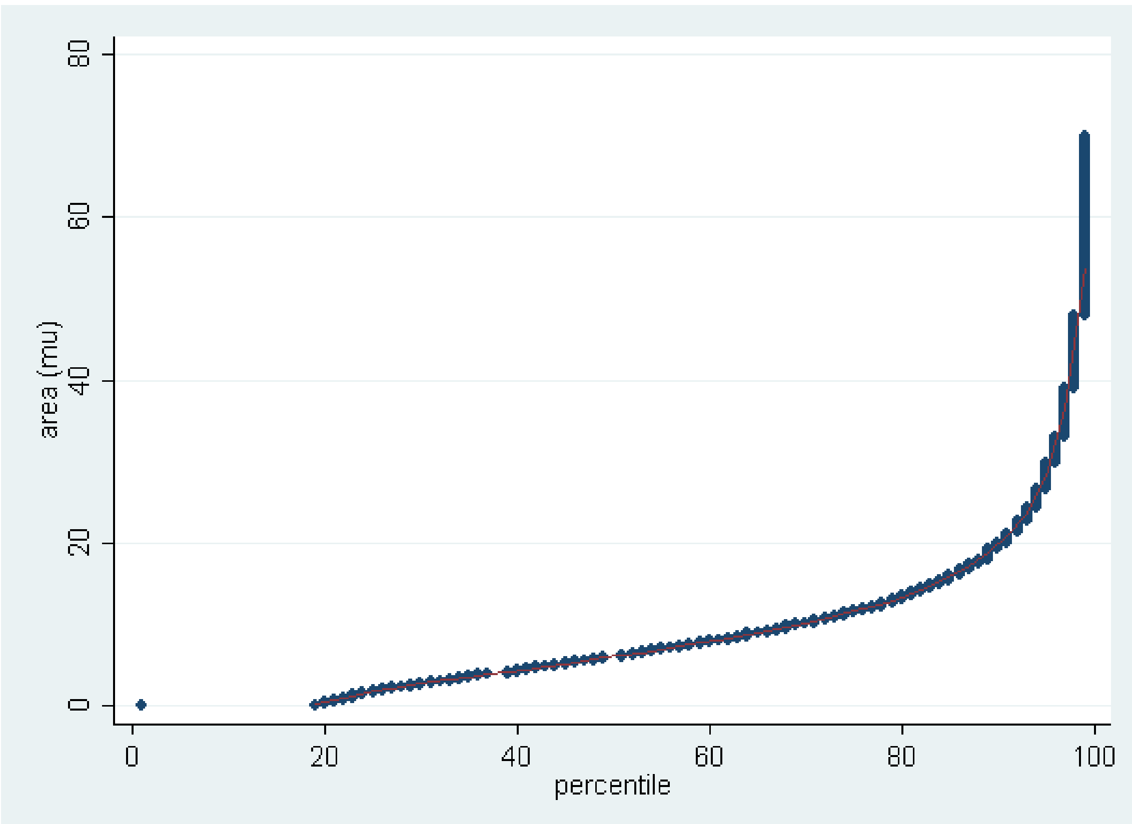
7 Appendix for Online Publication

Figure A.1: Major Government Land Reallocations over Time



Note: Calculated using data in the Village Democracy Survey (2006)

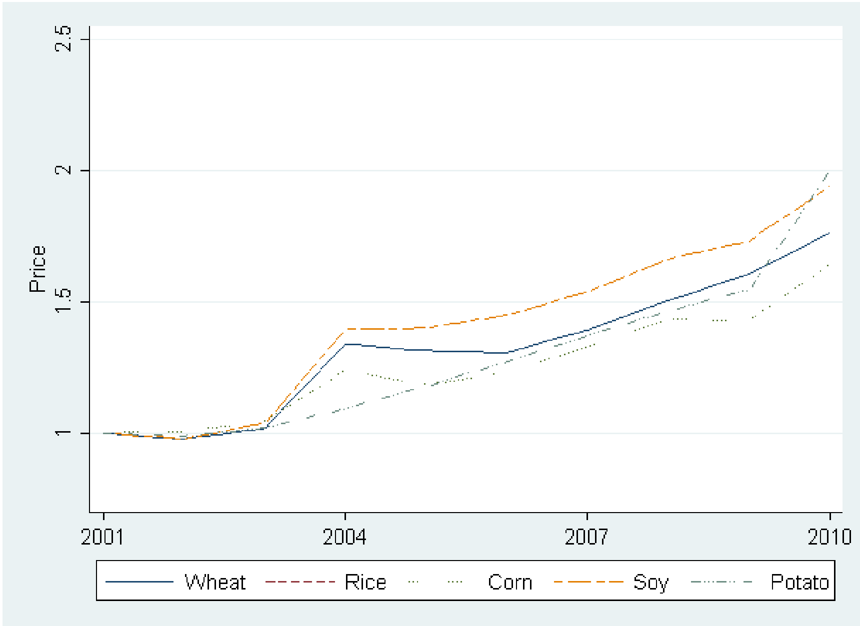
Figure A.2: Distribution of Household Land Area



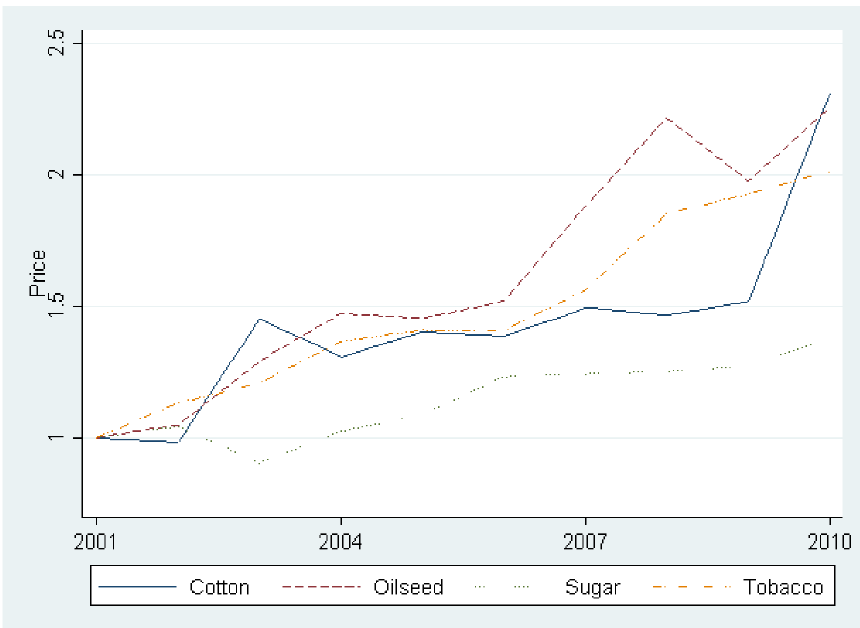
Note: Kernel density function using epanechnikov.

Figure A.3: Average Price Indices of Crops

(a) Staple Crops

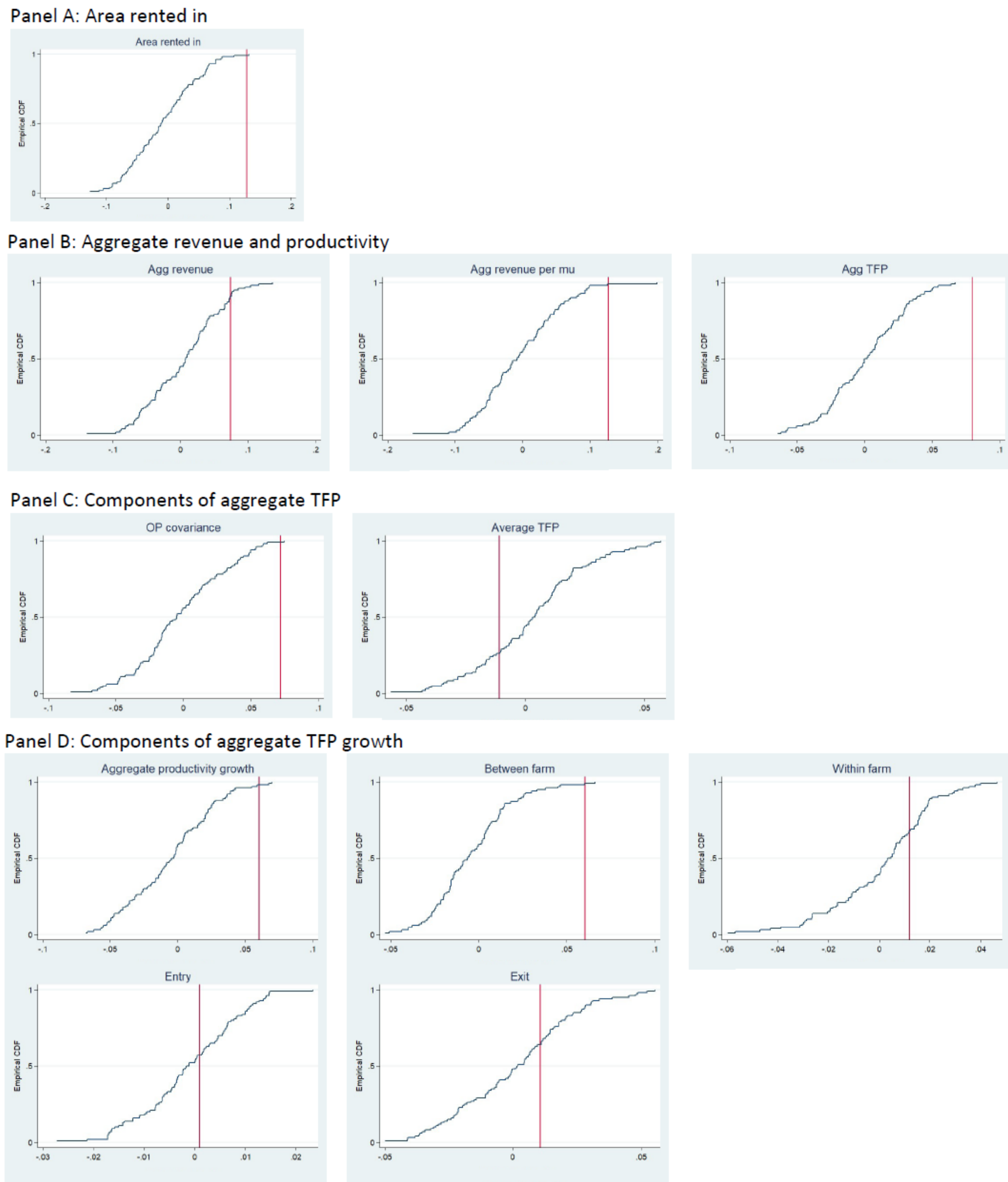


(b) Cash Crops



Note: The figure presents the average prices across provinces by year and by crop.

Figure A.4: Random Inference Distribution of Placebo Treatment Effects



Note: The panels plot the cumulative distribution function (CDF) of the placebo treatment effects (corresponding to the randomization inference exercise) for each of the main outcome variables. The red line in each figure indicates the actual treatment effect.

Table A.1: Rural Land Contracting Law Announcement by Province

Province	Document Name	Issue Date	Effective Date
Shanghai	Hu Fu Fa (2003) No. 29	04/25/2003	04/25/2003
Hunan	Hunan Province People's Congress Standing Committee (2004) No. 35	07/30/2004	10/01/2004
Shandong	Shandong Province People's Congress Standing Committee (2004) No. 37	07/30/2004	10/01/2004
Anhui	Anhui Province People's Congress Standing Committee (2005) No. 57	06/17/2005	10/01/2005
Fujian	Fujian Province People's Congress Standing Committee (Min Chang (2005) No. 18)	09/30/2005	11/01/2005
Jiangsu	Jiangsu Province Government Order (2003) No. 21	12/18/2003	02/01/2004
Jilin	Jilin Province People's Congress Standing Committee (2005) No. 29	01/20/2005	03/01/2005
Liaoning	Liaoning Province People's Congress Standing Committee (2005) No. 28	01/28/2005	04/01/2005
Shanxi	Shanxi Province People's Congress Standing Committee (2004) No. 117	09/25/2004	01/01/2005
Tianjin	Jin Zheng Fa (2005) No. 009	02/05/2005	02/05/2005
Xinjiang	Xinjiang People's Congress Standing Committee (2005) No.24	07/29/2005	10/01/2005
Gansu	Gansu Province People's Congress Standing Committee (Ganzheng Ban Fa (2006) No. 92)	08/03/2006	08/03/2006
Guangxi	Gui Zheng Ban Fa (2006) No. 141	11/14/2006	11/14/2006
Hainan	Hainan Province People's Congress Standing Committee (2006) No.44	07/28/2006	10/01/2006
Sichuan	Sichuan Province People's Congress Standing Committee (2007) No.110	11/29/2007	03/01/2008
Yunnan	Yunnan Province People's Congress Standing Committee (2006) No. 41	07/28/2006	09/01/2006
Chongqing	Chongqing Municipality People's Congress Standing Committee (2007) No. 6	04/02/2007	07/01/2007
Jiangxi	Jiangxi Province People's Congress Standing Committee (2007 No. 102	07/27/2007	10/01/2007
Shaanxi	Shaanxi Province People's Congress Standing Committee (2006) No. 59	09/28/2006	01/01/2007
Zhejiang	Zhejiang Province People's Congress Standing Committee (2006) No. 59	09/30/2006	01/01/2007
Inner Mongolia	Inner Mongolia People's Congress Standing Committee (2009) No. 10	07/30/2009	10/01/2009
Qinghai	Qinghai Province People's Congress Standing Committee (2009) No. 15	11/10/2009	03/01/2010

Notes: Provinces not listed here either made their announcements after our sample period or have not made them at the time we collected these data.

Table A.2: Determinants of Provincial Reform Timing

	(1)	(2)	(3)	(4)
Rural Income	0.145	1.090	0.305	-0.460
	(0.490)	(1.981)	(0.562)	(2.521)
Urban Income	0.529**	-0.860	0.869	-0.436
	(0.226)	(1.229)	(0.582)	(1.991)
Agric. Employment	-0.0378	-0.616	-0.0838	-0.825
	(0.145)	(0.494)	(0.223)	(0.801)
Industr. Employment	-0.319	-0.723	-0.475	-0.775
	(0.323)	(0.448)	(0.364)	(0.580)
Service Employment	0.223	-0.194	0.404	-0.145
	(0.460)	(0.526)	(0.354)	(0.734)
Rural Exp. Food	0.0363	-0.414	-0.0145	0.302
	(0.380)	(1.079)	(0.543)	(1.338)
Rural Exp. Clothing	0.0376	-0.281	-0.0854	-1.244
	(0.279)	(0.788)	(0.349)	(0.820)
Rural Exp. Housing	-0.285	0.0742	-0.357	-0.0909
	(0.176)	(0.309)	(0.302)	(0.396)
Manufacturing GDP	0.444	0.267	0.549	-0.452
	(0.278)	(0.530)	(0.439)	(0.739)
Construction GDP	-0.0528	-0.351	-0.0639	-0.103
	(0.225)	(0.429)	(0.271)	(0.565)
Transportation GDP	-0.162	-0.182	-0.111	-0.0723
	(0.210)	(0.338)	(0.272)	(0.352)
Wholes.+Retail GDP	-0.0966	0.401	-0.190	0.367
	(0.225)	(0.336)	(0.294)	(0.418)
Land Dispute	0.00673		-0.0339	
	(0.0582)		(0.0887)	
Distance from Beijing	0.0678		0.0442	
	(0.101)		(0.136)	
Reallocation Share			-0.114	0.170
			(0.671)	(0.614)
Observations	149	149	99	99
Adj. R-squared	0.170	0.389	0.138	0.418
Joint F-test	8.876	2.356	2.728	1.138
Year + Prov FE	No	Yes	No	Yes

Notes: Each observation is a province-year. We employ linear probability model. The dependent variable equals one in the reform year. The sample is limited to pre-reform years and the year of reform implementation. Standard errors clustered at province level are reported in parentheses. Rural and urban income are natural log of income per capita for rural and urban areas in the given province, respectively. Agricultural, industrial and service employment are natural log of provincial-level employment in those sectors. Rural Exp. Food, Clothing and Housing are natural log of expenditure per capita in rural areas for each of the respective categories. Manufacturing, Construction, Transportation, Wholesale and Retail GDP are natural log of provincial-level GDPfor the respective sectors. Aforementioned variables are collected from the provincial-level statistical yearbooks. Reallocation Share is the share of households that experienced a land reallocation by the village with a given province based on data collected by RCRE in the Village Democracy Survey. Padro i Miquel, Qian and Yao (2012) describe that data in more detail. Land disputes reflects the news articles with on land disputes for each given province between 1998 and 2003 on Wisenews, which is the largest database of newspapers in China. Distance from Beijing is natural log of the straight-line distance between each provincial capital and Beijing using Google maps. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.3: The Impact of Land and Productivity Dispersion on Reform Timing

	(1)	(2)
Standard deviation of Marginal Productivities	0.00569	0.00804
	(0.0263)	(0.0131)
Standard deviation of Farm Area	-0.310	0.0209
	(0.210)	(0.168)
Observations	960	960
Year + Prov Fe	No	Yes

Notes: Each observation is a village-year. The dependent variable equals one in the reform year. The sample is limited to pre-reform years and the year of reform implementation. Standard errors clustered at province level are reported in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.4: Reform Impact Estimates with Leads and Lags

	IHS Rent In	Agg. Output	Land Prod	Agg. TFP	OP Cov	Aver. TFP	Within Cov.	Betw. Cov	Agg Prod Growth	Betw Farm	Within Farm	Entry	Exit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Reform_{t-3}	-0.02	-0.00	0.03	-0.01	0.02	-0.02	-0.00	0.02	-0.02	-0.01	0.01	-0.02	-0.00
	(0.03)	(0.06)	(0.04)	(0.03)	(0.03)	(0.03)	(0.01)	(0.02)	(0.04)	(0.01)	(0.02)	(0.02)	(0.02)
Reform_{t-2}	-0.01	-0.03	0.01	-0.04	0.02	-0.05*	0.01	0.01	-0.06	0.01	-0.03	-0.00	-0.01
	(0.03)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)	(0.01)	(0.02)	(0.05)	(0.01)	(0.02)	(0.01)	(0.02)
Reform_{t}	0.04**	-0.02	0.05	0.01	0.04**	-0.02	0.01	0.03**	0.01	0.01	-0.01	0.00	-0.00
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.01)	(0.02)	(0.01)	(0.01)
Reform_{t+1}	0.04*	0.07	0.08**	0.07**	0.08***	-0.01	0.03	0.06***	0.06*	0.04*	0.01	-0.01	-0.01
	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)
Reform_{t+2}	0.04	0.08	0.08	0.06	0.07*	-0.01	0.02	0.06**	0.02	0.02	-0.01	-0.01	-0.01
	(0.03)	(0.06)	(0.05)	(0.04)	(0.04)	(0.02)	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.01)	(0.02)
N	60328	1518	1518	1518	1518	1518	1518	1518	1336	1481	1481	1481	1336

Notes: In column 1, each observation is a household-year, and the regressions include household fixed effects, year fixed effects, year fixed effects, and the agricultural tax rate. In columns 2-13, each observation is a village-year, and the regressions include village fixed effects, year fixed effects, and the agricultural tax rate. The sample is restricted to the six years around the reform. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.5: Estimates with the Sample Restricted to Reforming Provinces

	Post Reform Year		Reform Year		Observations
	Coefficient	P-values(RI)	Coefficient	P-values(RI)	
	(1)	(2)	(3)	(4)	(5)
Renting In Area	0.128***	(0.01)	0.0779*	(0.07)	39227
Aggregate Revenue	0.0744	(0.16)	-0.0132	(0.77)	1791
Aggregate Revenue per Mu	0.127**	(0.02)	0.0643*	(0.13)	1791
Aggregate TFP	0.0800***	(0.00)	0.0307	(0.24)	1791
OP Covariance	0.0719**	(0.02)	0.0323	(0.20)	1791
Average TFP	-0.0110	(0.62)	-0.0192	(0.31)	1619
Within Crops	0.0560***	(0.00)	0.0299*	(0.06)	1791
Between Crops	0.0160	(0.60)	0.00245	(0.92)	1791
TFP Growth	0.0604**	(0.05)	0.0210	(0.57)	1583
Between Farm	0.0602***	(0.01)	0.0298*	(0.10)	1583
Within Farm	0.0121	(0.54)	-0.000323	(1.00)	1583
Entry	0.00101	(0.95)	0.00248	(0.72)	1583
Exit	0.0108	(0.65)	0.00806	(0.65)	1583

Note: The table shows the estimated reform effects (and associated standard errors) for each of the set of dependent variables indicated in the rows, while restricting the sample to provinces that adopted the reform. All regressions include year fixed effects, and the agricultural tax rate. P-values and statistical significance are calculated by randomization inference based on 100 permutations of treatment assignment. The household-year level regressions (in row 1) also include household fixed effects. The other rows include village fixed effects. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.6: Impact of Property Rights Reform on Machine Costs and Labor Outcomes

	Agricultural Capital (1)	Machine Cost (2)	Hired Labor Days (3)	Migrant Days (4)	Off-Farm Days (5)
Post Reform Year	-0.0130	-0.00973	0.0205	-0.0986	-0.0687
	(0.0833)	(0.0781)	(0.0124)	(0.0701)	(0.110)
Reform Year	-0.0103	0.0342	0.00166	-0.0740	-0.0344
	(0.0621)	(0.0527)	(0.00956)	(0.0586)	(0.0772)
Observations	120767	275540	275540	120250	120250

Notes: In columns 1, 3 and 4, each observation is a household-year. In columns 2 and 3, each observation is a household-crop-year. The dependent variable is the IHS function of the column label. The regressions include household fixed effects, year fixed effects, and the agricultural tax rate. Columns 2 and 3 also include crop fixed effects. Standard errors clustered at the province level are in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.7: Production Function Estimates by Crop

	Area	Labor	Machinery	Other	Obs
Wheat	0.631***	0.122***	0.020***	0.097***	46,010
	(0.021)	(0.010)	(0.003)	(0.009)	
Rice	0.644***	0.119***	0.011***	0.086***	62,161
	(0.016)	(0.009)	(0.002)	(0.009)	
Corn	0.657***	0.148***	0.012***	0.079***	75,709
	(0.017)	(0.010)	(0.002)	(0.006)	
Soybean	0.569***	0.226***	0.014***	0.046***	31,823
	(0.013)	(0.011)	(0.004)	(0.004)	
Potato	0.520***	0.184***	0.027***	0.048***	30,804
	(0.018)	(0.011)	(0.006)	(0.006)	
Other grains	0.597***	0.198***	0.026***	0.094***	40,804
	(0.023)	(0.019)	(0.007)	(0.011)	
Cotton	0.835***	0.083***	-0.006	0.034***	10,363
	(0.024)	(0.019)	(0.005)	(0.013)	
Oilseed	0.598***	0.186***	0.012***	0.051***	40,897
	(0.014)	(0.010)	(0.003)	(0.004)	
Sugar	0.743***	0.141***	0.020**	0.128***	2,897
	(0.074)	(0.059)	(0.009)	(0.037)	
Hemp	0.578***	0.141	-0.244	-0.075*	468
	(0.118)	(0.122)	(0.153)	(0.045)	
Tobacco Leaf	0.516***	0.146***	0.022*	0.295***	1906
	(0.092)	(0.046)	(0.012)	(0.057)	
Other cash	0.254***	0.414***	-0.026	0.054**	5248
	(0.045)	(0.036)	(0.026)	(0.018)	
Vegetables	0.243***	0.281***	0.052***	0.076***	72,984
	(0.021)	(0.010)	(0.005)	(0.004)	
Other farm	0.385***	0.276***	0.042***	0.054***	11,865
	(0.023)	(0.021)	(0.012)	(0.009)	
Fruit	0.241***	0.370***	0.013**	0.135	18,259
	(0.025)	(0.018)	(0.006)	(0.009)	
Other orchard	0.368***	0.436***	0.037	0.107***	5517
	(0.068)	(0.044)	(0.072)	(0.017)	
Total	0.475***	0.239***	0.019***	0.075***	457,715
	(0.009)	(0.005)	(0.001)	(0.002)	

Notes: Each row presents the production function coefficient estimates for a particular crop. Standard errors are in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.8: Production Function Estimates by Crop (Balanced Panel)

	Area	Labor	Machinery	Other Inputs	Observations
Wheat	0.582***	0.150***	0.020***	0.068***	10,991
	(0.037)	(0.017)	(0.004)	(0.020)	
Rice	0.641***	0.118***	0.015***	0.083***	12,759
	(0.030)	(0.016)	(0.003)	(0.016)	
Corn	0.594***	0.175***	0.008**	0.084***	15,781
	(0.048)	(0.023)	(0.004)	(0.014)	
Soybean	0.581***	0.224***	0.009	0.034***	4,262
	(0.029)	(0.026)	(0.008)	(0.010)	
Potato	0.410***	0.217***	0.003	0.054***	3,759
	(0.064)	(0.032)	(0.015)	(0.017)	
Other grains	0.607***	0.178**	0.034**	0.064*	529
	(0.088)	(0.078)	(0.016)	(0.037)	
Cotton	0.922***	0.076	-0.012	-0.040	1,454
	(0.060)	(0.048)	(0.011)	(0.029)	
Oilseed	0.613***	0.147***	0.015***	0.068***	7,905
	(0.023)	(0.020)	(0.005)	(0.009)	
Sugar	0.817***	-0.071	0.024*	0.207**	405
	(0.136)	(0.098)	(0.014)	(0.098)	
Tobacco leaf	0.848***	0.010	0.031	0.161	169
	(0.233)	(0.149)	(0.024)	(0.195)	
Other cash	0.194*	0.420***	-0.028	0.050*	499
	(0.102)	(0.072)	(0.038)	(0.026)	
Vegetables	0.204***	0.292***	0.072***	0.066***	13,622
	(0.043)	(0.020)	(0.014)	(0.006)	
Other Farm	0.309***	0.059	0.071	0.031	795
	(0.067)	(0.051)	(0.056)	(0.027)	
Fruit	0.153***	0.371***	0.008	0.168***	1,743
	(0.059)	(0.044)	(0.012)	(0.025)	
Other orchard	0.233	0.383***	-0.050	0.075***	1,033
	(0.155)	(0.075)	(0.087)	(0.021)	
Total	0.417***	0.243***	0.018***	0.070***	75,706
	(0.025)	(0.013)	(0.002)	(0.005)	

Notes: Each row presents the production function coefficient estimates for a particular crop. The sample is limited to observations in which the household farms the crop in every year. Standard errors are in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.9: Production Function Estimates by Crop Instrumented with Lagged Values

	Area	Labor	Machinery	Other Inputs	Observations
Wheat	0.587***	0.113***	0.017**	0.129***	46,010
	(0.033)	(0.022)	(0.007)	(0.018)	
Rice	0.641***	0.095***	0.008	0.116***	62,161
	(0.027)	(0.021)	(0.005)	(0.018)	
Corn	0.670***	0.117***	-0.002	0.081***	75,709
	(0.022)	(0.018)	(0.005)	(0.010)	
Soybean	0.604***	0.270***	0.015	0.059***	31,823
	(0.028)	(0.027)	(0.010)	(0.011)	
Potato	0.459***	0.205***	0.014	0.039***	30,804
	(0.026)	(0.024)	(0.015)	(0.012)	
Other grains	0.536***	0.128**	0.051***	0.167***	40,804
	(0.068)	(0.055)	(0.018)	(0.034)	
Cotton	0.772***	0.001	0.016	-0.006	10,363
	(0.060)	(0.047)	(0.013)	(0.026)	
Oilseed	0.655***	0.180***	0.014*	0.055***	40,897
	(0.025)	(0.022)	(0.008)	(0.010)	
Sugar	1.034***	-0.315**	-0.009	0.611***	2,897
	(0.184)	(0.129)	(0.019)	(0.086)	
Hemp	2.726	0.845*	-0.033	-1.084	468
	(1.707)	(0.502)	(0.028)	(0.942)	
Tobacco Leaf	0.493***	0.030	0.179***	0.470*	1906
	(0.110)	(0.171)	(0.068)	(0.252)	
Other cash	-0.041	0.499***	0.009	-0.020	5248
	(0.145)	(0.082)	(0.009)	(0.036)	
Vegetables	0.192***	0.249***	0.070***	0.064***	72,984
	(0.015)	(0.017)	(0.022)	(0.008)	
Other farm	0.447***	0.253***	0.020*	0.044*	11,865
	(0.052)	(0.042)	(0.012)	(0.025)	
Fruit	0.229***	0.309***	-0.005	0.173***	18,259
	(0.063)	(0.036)	(0.046)	(0.018)	
Other orchard	0.452***	0.569***		0.005	5517
	(0.088)	(0.062)		(0.023)	
Total	0.477***	0.207***	0.012***	0.077***	185989
	(0.007)	(0.008)	(0.003)	(0.004)	

Notes: Each row presents the production function coefficient estimates for a particular crop. Each input is instrumented with its lagged value. Standard errors are in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.10: Impact of Property Rights Reform on Farm Area by Quartile

Quartile Measure:	Marginal Productivity		Fixed TFP Component		Profits
	(1)	(2)	(3)	(4)	(5)
Post Reform X Quartile 1	-0.0393*	-0.0543*	-0.114***	-0.0432*	-0.0958***
	(0.0230)	(0.0291)	(0.0240)	(0.0252)	(0.0230)
Post Reform X Quartile 2	-0.00640	-0.0494	-0.0395	-0.0185	-0.0329
	(0.0193)	(0.0365)	(0.0232)	(0.0230)	(0.0241)
Post Reform X Quartile 3	0.0341	-0.00280	0.0406	0.0247	0.0383
	(0.0219)	(0.0336)	(0.0263)	(0.0270)	(0.0268)
Post Reform X Quartile 4	0.0701**	0.0851*	0.120***	0.0690**	0.107***
	(0.0257)	(0.0456)	(0.0275)	(0.0270)	(0.0274)
Post Reform X Soil Quality		0.00536			
		(0.0330)			
Reform Year X Soil Quality		0.00531			
		(0.0134)			
Observations	275540	31284	275540	275540	275540

Notes: The dependent variable is log farm area. Each observation is a household-crop-year. The productivity quartile is measured in terms of marginal productivity of land in columns 1 and 2, in terms of the fixed component of TFP in columns 3 and 4 and in terms of agricultural profits in column 5. Column 2 includes additional soil quality measures interacted with the reform indicators. In Column 3, the fixed TFP component is measured by applying a Bayesian shrinkage procedure to the TFP fixed effect from the production function regression. All regressions include the productivity quartiles, a Reform Year indicator interacted with the productivity quartiles, household fixed effects, year fixed effects, crop fixed effects, and the agricultural tax rate. Standard errors clustered at province level are reported in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.11: Impact of Reform on Farm Area with Continuous Productivity

	(1)
Reform Year	-0.414***
	(0.144)
Post Reform Year	-0.374***
	(0.132)
Productivity	-0.0842**
	(0.0408)
Productivity X Reform Year	0.0799***
	(0.0263)
Productivity X Post Reform Year	0.0702***
	(0.0250)
Observations	275120

Notes: The dependent variable is log farm area. Each observation is a household-crop-year. Productivity refers to marginal productivity of land. The regression includes household fixed effects, year fixed effects, crop fixed effects, and the agricultural tax rate. Standard errors clustered at province level are reported in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.

Table A.12: Impact of Price Changes and Property Rights Reform on Land Allocation with Leads and Lags

	IHS Area		I(Any Area)	
	Reduced Form	IV	Reduced Form	IV
	(1)	(2)	(3)	(4)
Lagged Price X Reform_{t-3}	0.0316	0.0436	-0.00303	-0.00165
	(0.0397)	(0.0458)	(0.0186)	(0.0222)
Lagged Price X Reform_{t-2}	-0.0130	-0.0146	-0.00870	-0.00951
	(0.0301)	(0.0354)	(0.0199)	(0.0234)
Lagged Price X Reform_{t}	0.0543**	0.156*	0.0193	0.0479
	(0.0257)	(0.0765)	(0.0133)	(0.0415)
Lagged Price X Reform_{t+1}	0.0697**	0.140**	0.0332**	0.0616**
	(0.0326)	(0.0642)	(0.0142)	(0.0267)
Lagged Price X Reform_{t+2}	0.0828**	0.161*	0.0380**	0.0705*
	(0.0400)	(0.0844)	(0.0184)	(0.0361)
Observations	329496	329496	329496	329496

Notes: Each observation is a household-crop-year. The dependent variables are IHS area in Columns 1 and 2, and a dummy variable for any area planted in Columns 3 and 4. In Columns 1 and 3, lagged price is measured as U.S. crop price from the previous year. In Column 2 and 4, lagged price refers to the Chinese crop price from the previous year, instrumented with U.S. prices. The sample is restricted to cash crops only. The regressions include controls for agricultural taxes, household-crop fixed effects and crop-year fixed effects. Standard errors clustered at the province level are in parentheses. *, **, *** denote significance at the 10%, 5% and 1% levels, respectively.