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Land Rights and the Allocation of Talent

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Abstract

Reducing barriers to rural-urban mobility can promote structural transformation, but the gains may be limited by workers' existing skills. We study how human capital enables additional reallocation of talent when stronger land rights reduce barriers to leaving agriculture, exploiting the staggered provincial rollout of China's Rural Land Contracting Law. Guided by a Roy model with endogenous schooling, we use a cohort difference-in-differences design that contrasts individuals exposed by age 15, when schooling could still adjust, with older cohorts whose education was already fixed. For boys, exposure increased high school completion and college enrollment by around 3 percentage points, and raised entry into high-skill urban occupations by 1.3 percentage points. The schooling response is about three times as large among boys from households with lower agricultural productivity, consistent with comparative-advantage sorting rather than income effects. By contrast, schooling and occupational responses among girls are small and statistically insignificant. For cohorts young enough to adjust their schooling, our quantitative analysis attributes nearly half of the occupational reallocation and about half of the income gains to additional education.

Keywords: Land Rights, Misallocation, Human Capital, Roy model, China.

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1 Introduction

Labor reallocation from agriculture to non-agriculture lies at the heart of structural transformation and economic development (Herrendorf et al., 2014). Yet in many developing countries this process remains far from complete. Agriculture still employs more than a quarter of the world's workers (FAO, 2025), even though value added per worker outside agriculture is roughly three times as high as in agriculture (Gollin et al., 2014). One important barrier to this reallocation is insecure land rights, which can discourage households from leaving agriculture even when their labor could be more productive elsewhere (De Janvry et al., 2015; Adamopoulos et al., 2022).

Existing evidence on the gains from securing land rights focuses largely on the short-run responses of the existing workforce following improvements in land security (Chari et al., 2021; Acampora et al., 2025). However, these workers made their schooling decisions under insecure land rights. Because non-agricultural work is often more human-capital intensive than farming, they may leave agriculture but remain concentrated in low-skill non-agricultural occupations, even when their comparative advantage lies in more skill-intensive work.[1] This raises an important question: if individuals could acquire new skills, would improving land security lead to greater reallocation of talent into more skilled occupations?

We study this question in the context of China's Rural Land Contracting Law (RLCL), which strengthened rural households' land rights and reduced the cost of leaving agriculture. Specifically, we examine how the allocation of talent evolves after land rights are strengthened: not only how many individuals leave agriculture, but also which occupations they enter. Doing so involves one conceptual challenge and two empirical challenges. The conceptual challenge is to determine whether these dynamics reflect reallocation made possible by acquiring new skills. Occupational sorting can change over time for many reasons, so the dynamics alone do not reveal the role of skills. To address this challenge, we develop a Roy model of occupational choice with endogenous education. The model separates the reallocation of workers with a given level of human capital from the reallocation that occurs when individuals can also adjust their human capital through education, and thus offers a framework to identify the role of education in the allocation of talent across occupations.

The first empirical challenge is to isolate talent reallocation driven by education from other

[1] Gollin et al. (2014) show that workers in non-agriculture have substantially more schooling than those in agriculture, and Herrendorf & Schoellman (2018) show that the returns to skill are higher outside agriculture.

factors that are affected by changes in land security. Addressing it requires comparing individuals who are similar in their underlying abilities, face the same economic conditions, and experience the same improvement in land security, but differ only in whether they can still adjust their education, as implied by our Roy model. We construct such a comparison using a cohort difference-in-differences design. Combining the staggered provincial rollout of the RLCL with the 2015 population mini-census, we compare individuals exposed to the reform by age 15, when schooling could still adjust, with slightly older cohorts whose education was largely fixed, relative to corresponding cohort differences in provinces that did not implement the reform. The key identifying assumption is that, absent the reform, cohort trends in underlying abilities and economic conditions would have been parallel across provinces. Under this assumption, differences between the two groups in educational attainment and subsequent occupational sorting identify the additional reallocation enabled by human capital adjustment.[2]

The second empirical challenge is to establish whether the schooling response reflects changes in the relative returns to different occupations, rather than other effects of the reform, such as increased household income. Mobility reforms often change parental income, employment, and migration, which can raise children's education without correcting a distortion in their occupational choices (Dinkelman & Mariotti, 2016). To distinguish between these explanations, we derive a testable prediction from our Roy model: improved land security should induce larger educational responses among individuals with lower agricultural productivity, for whom leaving farming offers greater potential gains. We test this prediction using pre-reform household agricultural productivity and compare it with the patterns implied by alternative explanations.

Building on this framework, our analysis yields four sets of findings. First, we establish a short-run benchmark by estimating the reform's effects on workers already in the labor market when the RLCL was implemented. We use the National Fixed Point Survey (NFP), a nationally representative household panel with detailed information on land rental, labor allocation, and income, and exploit the staggered provincial rollout of the RLCL using extended two-way fixed effects (ETWFE) (Wooldridge, 2025). Consistent with Chari et al. (2021), the reform increases land rental activity.[3] It also raises household income by about 5% and shifts labor toward non-

[2]This approach is similar in spirit to Hsieh et al. (2019), who emphasize that because human capital is acquired early in life, changes across cohorts are informative about the role of human capital in the allocation of talent. Like them, our identification rests on the assumption that differences in underlying abilities across groups remain stable over time, so that changes in occupational sorting reflect changes in barriers rather than in talent.

[3]Chari et al. (2021) focus on the reform's effects on land rental and agricultural efficiency. We extend the NFP panel through 2013 and additionally estimate effects on household income and non-agricultural

agricultural work. These responses are consistent with workers reallocating toward sectors in which they have a comparative advantage (Roy, 1951; Lagakos & Waugh, 2013; Adamopoulos et al., 2022), and provide a benchmark for the long-run reallocation we study next.

Second, we turn to the estimation of cohort effects using the 2015 mini-census. We use age 15 as the cutoff because it marks the transition from compulsory schooling to high school, the first major educational decision in China and the gateway to college and skilled employment. We estimate a cohort-based version of ETWFE, in which "time" is defined by the year an individual turns 15 and treatment groups are defined by provincial RLCL adoption years. We find that the reform substantially increased educational attainment among younger cohorts relative to older ones. For boys, exposure to the RLCL by age 15 increased high school completion by 3.4 percentage points (9% of the mean) and college enrollment by 2.9 percentage points (17% of the mean). The effects are small and statistically insignificant for girls. This gender difference is consistent with a household-based channel of land security under patrilocal norms. For men who expect to head a contracting household, leaving agriculture may directly affect the security of the household's land claim. By contrast, women's effective access to land often depends on their status within their natal or marital households, making land rights less directly linked to their own educational and occupational choices (Hare et al., 2007; Judd, 2007).[4]

Among men, the educational gains translate into a shift toward higher-skill occupations. Relative to older cohorts, those exposed to the RLCL by age 15 are 1.9 percentage points more likely to live outside of their *hukou* village, 2.3 percentage points (6.8 percent of the mean) less likely to work in rural low-skill occupations, and 1.3 percentage points (10.4 percent of the mean) more likely to work in urban high-skill employment. In contrast, employment in low-skill urban occupations, for which a high school education is less important, does not increase. This indicates that the reform enables younger cohorts to reallocate into high-skill occupations by acquiring additional education, rather than simply moving them out of agriculture. The implied income gain, imputed from education and occupation using the 2018 wave of the China Family Panel Studies (CFPS) survey, is about 1%.

Third, we test the model's prediction that individuals with lower agricultural productivity respond more strongly in their schooling decisions. We use pre-reform household agricultural

employment. Beyond that, we examine the reform's longer-run effects on younger cohorts' education and subsequent occupational sorting, which is our main focus.

[4]This gender asymmetry is also visible directly in rural land disputes: women's cases disproportionately dispute whether they hold a valid claim, given their status in a natal or marital household, while men's cases more often concern enforcing a claim already recognized as theirs (see Section 2 and Appendix A for more details).

productivity from the NFP data as a proxy for individual productivity. This measure also appears informative about comparative advantage across sectors: lower-agricultural-productivity households engage more in non-agricultural work despite having similar overall income, suggesting a stronger comparative advantage in non-agricultural work (Alvarez-Cuadrado et al., 2026).[5] Consistent with the model's prediction, the effect of the reform on high school completion among boys from low-productivity households is about three times as large as among boys from high-productivity households.[6] Girls show no significant response, regardless of pre-reform productivity. This heterogeneity is unlikely to reflect relaxed budget constraints: the reform's effect on household income is similar across productivity groups, and the schooling response is, if anything, stronger among households with higher pre-reform income and more educated parents.

Finally, we use the model to quantify the contribution of education to the reallocation of talent. In the model, the response of the existing workforce captures reallocation with fixed schooling, while the additional response of younger cohorts reflects reallocation enabled by educational adjustment. Comparing the two, we find that education-enabled reallocation accounts for approximately 47% of total occupational reallocation among cohorts young enough to adjust their schooling. To quantify the associated income gains, we calibrate the model to match our reduced-form estimates, using parameter values consistent with the literature (Adamopoulos et al., 2022, 2024). In the calibrated model, additional schooling accounts for 50.6% of the reform-induced income gain among cohorts young enough to adjust their schooling. Together, these findings imply that analyses limited to the existing workforce at the time of reform, or that hold education fixed, miss an important part of the reform's effects on income and occupational sorting.

This study builds on the extensive literature on land institutions and their economic implications. A large body of evidence across developing countries shows that securing property rights promotes household investment and labor reallocation (Besley, 1995; Field, 2007; Galiani & Schargrodsky, 2010; De Janvry et al., 2015; Adamopoulos & Restuccia, 2014; C. Chen, 2017; Gottlieb & Grobovšek, 2019; Acampora et al., 2025), with recent work focusing specifically on the Chinese context (Ngai et al., 2018; Chari et al., 2021; Adamopoulos et al.,

[5] We draw on the inferential logic in Alvarez-Cuadrado et al. (2026) rather than their empirical conclusion. In their setting, more productive farming households are also more likely to supply non-farm labor, implying that comparative and absolute advantage are misaligned. In our setting, the correlation runs in the opposite direction, suggesting that comparative and absolute advantage are aligned in the way a standard Roy model would predict.

[6] Because the last available NFP wave with a sufficient sample is 2013, we focus this heterogeneity analysis on high school completion, as the relevant cohorts are too young for reliable college or occupational outcomes.

2022; Liu et al., 2023; Adamopoulos et al., 2024; T. Chen et al., 2025). While this work establishes land security as an important mobility barrier, it largely takes education as fixed and focuses on the responses of the existing workforce. We show that land insecurity also distorts the human capital investments of younger generations, so that a substantial part of the gains from securing land rights materializes only as new cohorts enter the labor market. In this way, we connect the land-rights literature to work on talent allocation and human-capital-driven structural transformation (Hsieh et al., 2019; Porzio et al., 2022).

We also contribute to the literature on resource misallocation and aggregate productivity (Restuccia & Rogerson, 2017). Early work in this literature emphasized the misallocation of capital and labor (Hsieh & Klenow, 2009; Banerjee & Moll, 2010; Brandt et al., 2013; Tombe & Zhu, 2019), while more recent studies show how barriers related to gender, caste, networks, location, and institutions distort both human capital investment and the allocation of talent (Balboni et al., 2022; Munshi & Rosenzweig, 2016; Hsieh et al., 2019; Nakamura et al., 2022; Gai et al., 2025; Ager et al., 2025; Cassan et al., in press). We identify insecure land rights as another such barrier. Unlike barriers that directly restrict access to education, land insecurity distorts human capital investment indirectly, by lowering the expected returns to skills that are useful outside farming. Because these distortions are embedded in the schooling choices of each cohort, the resulting misallocation of talent can persist long after the barrier is removed.

Finally, we bring together two strands of research that have studied mobility and human capital from two different angles. The first studies migration decisions induced by lower migration costs or higher migrant earnings and the effects on education in origin communities (Dinkelman & Mariotti, 2016; Khanna et al., 2026); these studies point to relaxed household budget constraints as the operative channel. The second uses shocks that force people to move across locations to study the effects of mobility on subsequent education and labor market outcomes (Nakamura et al., 2022; Becker et al., 2020; Chiovelli et al., 2026; Sarvimäki et al., 2022). Because individuals in these settings move as a result of the shock, it is difficult to tell whether their subsequent change in education and labor market outcomes reflects an investment to take advantage of better opportunities after the move, or simply better access to education and labor market at their destination. We study a policy that lowers the cost of leaving agriculture while allowing individuals to choose whether to exit, and schooling decisions are made before any move takes place. This setting allows us to test directly whether schooling responds to new opportunities, and we find that it does: schooling rises most where

the potential gains from leaving agriculture are largest.

The rest of the paper is organized as follows. The next section provides the institutional background and describes the data and key variables. Section 3 presents a model of occupational choice that guides the empirical analysis, which is described in Section 4. Section 5 uses the calibrated model to quantify the role of the education channel. Section 6 concludes and discusses policy implications.

2 Institutional Background and Data

2.1 Land Rights in China

To understand why land insecurity could shape schooling and occupational choices, we begin with three features of the pre-reform rural land system. First, land was allocated on egalitarian principles among eligible village households. Second, individual land claims differed systematically by gender. Third, protecting a claim often required continued presence and cultivation, creating a *de facto* "use-it-or-lose-it" mobility friction. We discuss each feature before describing the Rural Land Contracting Law (RLCL) and its provincial implementation.

Under the Household Responsibility System (HRS), introduced across rural China between 1978 and 1984, village collectives retained ownership of farmland and contracted plots to individual households. Households chose how to cultivate their plots and kept the income remaining after they paid taxes and met state procurement quotas (Almond et al., 2019; Xu, 2021). Village authorities allocated land according to egalitarian principles, with distribution tied to formal village membership through the *hukou*, or household registration, system.[7] This system generated relatively equal per capita landholdings among households within each village. In practice, village authorities often allocated each household multiple plots to equalize both land endowments and productive opportunities across households within the same village (Rozelle & Li, 1998; Brandt et al., 2004; Ho, 2005).

The effort to preserve egalitarian access could, however, undermine tenure security over time. Rural households initially received 15-year land-use contracts. The 1998 Land Management Law extended these contracts to 30 years, but village officials continued to retain substantial discretion over reallocations, often justified as preserving equal per capita land access after

[7]The *hukou* system assigns each individual agricultural, or rural, status or non-agricultural, or urban, status in a specific locality, shaping legal residence, employment opportunities, and access to local public services. Although migration restrictions gradually relaxed after the early 1980s, land rights remained closely linked to agricultural *hukou* status.

demographic changes such as births, deaths, marriage, and migration (Benjamin & Brandt, 2002). Land claims therefore depended not only on formal contracts, but also on continued village membership, presence in the village, and visible engagement in agricultural production.

Before the RLCL, this discretion created a "use-it-or-lose-it" mobility friction. Households whose members left the village for work, schooling, or other reasons could fear losing contracted land, future reallocations, or compensation claims in the event of land expropriation for infrastructure, urban expansion, or industrial development. Households could partially offset this risk in the short run by reallocating labor within the household: one member—often a woman, or an older member—could remain to farm and maintain the household's presence in the village as "guard labor" protecting its land claims, while others, typically working-age men, migrated for non-agricultural work (Adamopoulos et al., 2024; T. Chen et al., 2025).[8] This strategy did not eliminate the friction, however. One piece of evidence for this persistent friction is Giles & Mu (2018), showing that between 1995 and 2003, reallocations were tied to village-election incentives and that rural residents reduced migration in the year before expected reallocations. Presence in the village and active cultivation therefore helped protect land claims, raising the opportunity cost of migration and non-agricultural employment (Giles & Mu, 2018; Adamopoulos et al., 2024).

This friction, however, did not weigh equally on all members of a household. Under Chinese patrilineal norms, land was allocated and registered at the household level in the name of a household head, a position women rarely held themselves. A woman's access to land typically depended on her status as a daughter or wife in a household headed by her father or husband, making it less directly tied to her own education and occupational choices. Women who left their natal villages could lose claims there without securing equally secure claims in their husbands' villages (Sargeson, 2012). For women, land insecurity often concerned whether they held a recognized claim at all. Men, by contrast, were more likely to hold recognized claims but risked losing them when migrating or leaving agriculture—the "use-it-or-lose-it" mobility friction. Consistent with this distinction, women's rural-land disputes disproportionately concern the recognition of land claims, whereas men's more often involve the enforcement or exercise of existing claims.[9] For this reason, we conduct our

[8] T. Chen et al. (2025) study married women's labor allocation, finding that reducing land insecurity disproportionately frees wives from this guard-labor role. However, their mechanism becomes directly relevant only after marriage, when a woman obtains land rights through the husband.

[9] In a sample of 9,004 first-instance rural land decisions in 2014, 68.9% of cases brought by women dispute the existence or allocation of a land claim, compared with 22.9% of cases brought by men; by contrast, 64.6% of men's cases concern exercising or enforcing an already-recognized land-use right, compared with 26.5% of

analysis separately by gender and discuss what these gender differences imply about the underlying mechanisms.

The RLCL, enacted nationally in 2003, strengthened household land rights without privatizing farmland or abandoning egalitarian allocation (Chari et al., 2021). The law protected rights to contracted land during the 30-year contract period, prohibited arbitrary village-wide reallocations, and granted farmers the legal right to lease out contracted agricultural land. Only when the entire household moves to a city and obtains urban *hukou* can the village reclaim the contracted land. These provisions addressed different sources of land insecurity due to demographic changes, marriage, and migration. Most relevant to us, the provisions weakened the de facto "use-it-or-lose-it" mobility friction by allowing rural households to lease out land and move into non-agricultural work while retaining their land claims. Appendix A.1 provides examples of court cases in which migrants and students invoked the RLCL to challenge village decisions that denied compensation or reclaimed contracted land—illustrating that the law gave rural households not just formal rights but a legal basis for contesting practices that had previously raised the cost of leaving farming.

Although the RLCL was enacted nationally in 2003, provinces implemented it on different schedules, issuing local guidelines that clarified how its protections applied to common sources of land-rights insecurity, including temporary migration, education, marriage, divorce, widowhood, and land leasing. These guidelines addressed broadly similar concerns across provinces, and we do not exploit differences in their content. Appendix A.2 provides examples from Yunnan Province.

As Chari et al. (2021) document, provincial governments determined the timing of local implementation. Following their approach, we use the effective dates of provincial implementation rules as our treatment timing, capturing formal local implementation beyond the national statute. By the end of 2014, 24 provincial governments had officially announced their implementation rules (Table A1).

2.2 Data

We link the timing of policy rollout in each province to two key data sources: (1) a 1 percent random subsample of the 2015 Population Survey (the "mini-census") and (2) the National Fixed Point Survey (NFP), a household panel initiated by the Chinese Ministry of Agriculture

women's cases. See Appendix A for further detail.

in 1986.

2.2.1 2015 Population Survey

The 1 percent sample of the 2015 mini-census, collected by the National Bureau of Statistics, contains 2,000,563 individuals. Similar to the earlier population survey conducted in 2005, it covers the entire population at current residence, regardless of local *hukou* registration. The survey records year and month of birth, gender, education, employment status, occupation, current residence area code, and *hukou* place area code. These area codes identify province, prefecture, county, and whether the community is a village or an urban community. Because the mini-census does not report urban or rural *hukou* status directly, we drop county-level administrative units designated as urban districts (*qu*) and retain individuals whose *hukou* communities are classified by the National Bureau of Statistics as villages, rural centers, town-village junctions, or urban-rural junctions.[10]

We use the mini-census to construct education, migration, and labor market outcomes. Education outcomes are indicators for ever completing high school and ever attending college. The migration indicator equals one if an individual did not live in the *hukou* village in 2015. For labor market outcomes, we construct three variables. The first is whether the individual is currently employed. The second is whether the individual holds a rural low-skill job, including farming and low-skill construction.[11] The third variable is whether an individual is employed in a high-skill occupation, defined to include government and party officials, professional and technical personnel (e.g., researchers, engineers, healthcare workers, finance specialists, lawyers, teachers), and service workers in information technology, finance, real estate, technical support, utilities, and healthcare.[12]

Because the mini-census does not report labor income, we estimate gender-specific earnings models using the 2018 China Family Panel Studies (CFPS). For each mini-census respondent, we

[10]We drop Beijing and Shanghai municipalities as they are fully urbanized.

[11]The mini-census records an individual's occupation based on work performed in the past week, not their usual occupation. Restricting the rural low-skill category to farming alone would therefore miss workers who are simultaneously engaged in agriculture and other off-farm work. We classify construction specifically as rural low-skill employment because the data show it is the off-farm occupation most likely to be held by such workers: 58.4% of construction jobs are in rural areas and 87.3% are held by agricultural *hukou* holders (2010 China Family Panel Studies; see Table B2). Construction is also the least skill-intensive occupation available to temporary migrants, and its project-based nature—unlike the rigid schedules of manufacturing or the continuous attendance required in service jobs—allows workers to take extended time off between projects for agricultural activities. These features make construction institutionally and economically closer to agricultural production than to other urban employment sectors.

[12]To classify high-skill occupations, we use a separate sample of individuals born between 1971 and 1985 to calculate the share of college graduates by occupation. We define occupations with at least 25% college graduates as high-skill. This data-driven approach aligns closely with the conventional set of white-collar, professional jobs.

predict log annual earnings using the characteristics recorded in 2015 and the age the respondent would reach in 2020. This imputed measure is not intended to capture an individual's actual earnings; rather, it reflects the income level typically associated with an individual's occupation, which in turn proxies for the occupation's education and skill requirements and productivity, following a similar logic to approaches that use the wage level associated with an occupation as a proxy for its skill intensity (Autor & Dorn, 2013). Our main specification predicts log income using occupation indicators, years of schooling and its squared term, age and age squared, and province fixed effects; Appendix B.1.3 reports the full specification and alternative prediction models.

Summary statistics for the main variables are presented in Table B1. The sample includes individuals who turned age 15 between 2000 and 2010, covering 98,581 males and 92,303 females. The window spans the RLCL's 2003 national enactment and the bulk of its provincial rollout: 23 of the 24 provinces that eventually implemented the law locally had done so by 2010. Individuals in China typically make high school enrollment decisions at age 15 and college enrollment decisions at age 18. The youngest cohort in our sample was already 20 years old in 2015, allowing us to observe high school and college enrollment decisions for all sample cohorts. For labor market outcomes, we exclude individuals younger than 23 in 2015 to avoid conflating delayed labor market entry after college with employment effects.

2.2.2 National Fixed Point Survey

We supplement the main analysis with the National Fixed Point Survey (NFP), a nationally representative panel of villages and randomly sampled households with information on agricultural activities, income, family background, and demographics (Chari et al., 2021). Household and village analyses use annual waves from 2000 to 2013, covering over 20,000 households across 407 villages in 29 province-level regions. For individual-level analysis, we cannot track the same individuals across waves because of attrition. We instead treat each wave as a repeated cross-section and use the most recent one, 2013, for our cohort analysis, since sample sizes drop substantially in subsequent waves. We exclude households that did not sow any land before the reform, as they were unlikely to be affected by changes in agricultural land rights. Table B3 presents summary statistics for the main NFP variables.

3 A Model of Occupational Choice

To study how land security affects educational investments and talent allocation, we build a partial equilibrium Roy (1951) model with endogenous education. The model serves two purposes: it guides our reduced-form empirical analysis in Section 4 and provides the foundation for the quantitative exercise in Section 5. The key mechanism is that land insecurity distorts two linked decisions. A young person who expects to lose land rights by leaving farming has less incentive to acquire education, the main input for skilled non-agricultural work. Once he enters the labor market, forgone schooling limits his ability to reallocate out of agriculture even if land security later improves.

3.1 Setup

The model has two occupations: farming in the rural area and manufacturing in the urban area. Individual i draws a pair of occupation-specific productivities, $(z_F(i), z_M(i))$, where $z_F(i)$ governs farm productivity and $z_M(i)$ governs manufacturing productivity. Together, ($z_F(i)$ and $z_M(i)$) determine individual i's comparative advantage across the two occupations, as well as how strongly schooling and occupational choices respond to land insecurity.

We model two cohorts that differ in the timing of their schooling decision relative to the reform. The young cohort has yet to choose schooling when the reform arrives, whereas the old cohort has already made this choice. During the education stage, individuals observe their productivities and choose whether to invest in education, $h \in \{0, 1\}$, at cost c. The choice $h = 1$ corresponds to continuing beyond compulsory schooling toward high school and possibly college, the gateway to skilled urban employment.[13] In the subsequent labor-market stage, individuals choose an occupation and decide how much land to rent in or out. Schooling is irreversible, so occupational choices are constrained by earlier education decisions.

In principle, land rentals are not prohibited. However, families that rent out land risk losing access to this land . If individual i chooses to be a farmer, he decides how much land to rent in or out, $\ell^{rent}(i) > -\bar{\ell}(i)$, where $\bar{\ell}(i)$ is his endowment. Due to land rights insecurity, individuals who rent out land face an exogenous probability $\eta \in [0, 1]$ of losing the rented-out land, in which case there is an individual-specific income loss $\varphi(i) > 0$ per unit of rented-out land. Heterogeneity in $\varphi(i)$ across individuals reflects differences in the value attached to land,

[13] In our sample, more than 90% of individuals complete compulsory education while less than 40% complete high school; thus, the key margin of educational choice is high school completion.

arising from observable and unobservable characteristics of the land and the farming household (Adamopoulos et al., 2022).[14] Importantly, members of the young and old cohorts participate in the same post-reform land-rental market, so their rental decisions jointly determine the equilibrium rental price q, which is pinned down by the land-market-clearing condition.

Farmer i's optimal income, after choosing how much land to rent in or out, is

$$I_F(i) = \max_{\ell^{rent}} \left\{ p_a A_a z_F(i) [\bar{\ell}(i) + \ell^{rent}]^{\theta} - q\ell^{rent} + \eta\varphi(i)\ell^{rent}\mathbf{I}(\ell^{rent} < 0) \right\}$$

where $p_a > 0$ is the price of agricultural products, A_a is an agricultural technology parameter, $\theta \in (0, 1]$ captures the importance of land relative to labor in agricultural production, and $\mathbf{I}(\ell^{rent} < 0)$ is an indicator equal to one when the farmer rents out land. The last term captures the expected income loss from land insecurity: a farmer who rents out land faces an expected loss of $\eta\varphi(i)|\ell^{rent}|$.

A non-farmer rents out his entire land endowment and works in manufacturing. His total income combines manufacturing earnings, land rental income, the expected loss from land insecurity, and education cost:

$$I_M(i) = w_M(1 - \kappa_i)z_M(i)(1 + \tau h(i)) + q\bar{\ell}(i) - \eta\varphi(i)\bar{\ell}(i) - ch(i),$$

where w_M is the manufacturing wage, c is the education cost, and $\kappa_i \in (0, 1)$ is an individual-specific mobility friction that proportionally reduces manufacturing earnings.[15] $\tau > 0$ captures the complementarity between education and non-agricultural production.[16]

This formulation allows us to compare the young and old cohorts, who differ in the timing of their schooling decision relative to the reform. Suppose that the two cohorts have the same distribution of individual characteristics, including $(z_F, z_M, \kappa, \bar{\ell})$, and participate in the same

[14] To highlight the main intuition, the analytical results initially impose a common $\varphi(i) = \varphi$ for all individuals. This assumption allows us to abstract from heterogeneity in land insecurity exposure and focus on how improved land security affects educational and occupational choices through the equilibrium rental market. In the quantitative analysis in Section 5, we show that the analytical results hold under a reasonable set of parameters even when we allow $\varphi(i)$ to vary across individuals.

[15] The friction κ_i captures all barriers to rural-urban mobility other than land insecurity—most importantly, *hukou* restrictions that limit migrants' access to urban public services and labor markets. We model κ_i as a logistic function: $\kappa_i = \frac{\exp(\mu_\kappa + \varepsilon_i^\kappa)}{1+\exp(\mu_\kappa + \varepsilon_i^\kappa)}$, where $\varepsilon_i^\kappa \sim \mathcal{N}(0, \sigma_\kappa^2)$. This functional form ensures $\kappa_i \in (0, 1)$ and captures heterogeneity in mobility barriers across individuals.

[16] Reimers & Klasen (2013) find that primary and secondary education significantly raise agricultural productivity in developing countries, but that tertiary education does not. In China specifically, high school education is not compulsory and mainly serves as a gateway to tertiary education rather than as a human capital investment with its own return to earnings (Li et al., 2012); consistent with this, census data show that only 7% of farmers in China have completed high school, the lowest rate across occupations. Accordingly, we treat education beyond compulsory schooling as augmenting productivity exclusively in non-agricultural work.

local land-rental market. Land-rental choices of both the young and old cohorts jointly determine the equilibrium rental price $q(\eta)$. When land security improves (η falls), renting out land becomes less risky, increasing land supply and lowering the equilibrium rental price. Although this price adjustment partially offsets the direct gain from lower land-loss risk, the net return to renting out land still increases (See Appendix C).

The two cohorts differ only in whether education can still adjust. The older cohort, denoted by O, had already completed its education when land security improved, so education is fixed at its pre-reform level, $h_i^O = \bar{h}_i$, and only occupational and land-rental choices can respond. The younger cohort, denoted by Y, was still of schooling age and can therefore adjust education as well as subsequent occupational and land-rental choices. Thus, both cohorts face the same improvement in land security and the same equilibrium rental-price response, but the younger cohort has an additional education margin of adjustment.

Let $k(i) \in \{F, M\}$ denote the occupational choice. The optimization problem for individual i from cohort O is

$$I^O(i) = \max_{k(i)\in\{F,M\}} \left\{ I_F(i; \bar{h}_i, \eta), I_M(i; \bar{h}_i, \eta) \right\}.$$

The optimization problem for individual i from cohort Y is

$$I^Y(i) = \max_{h_i\in\{0,1\}, k(i)\in\{F,M\}} \left\{ I_F(i; h_i, \eta), I_M(i; h_i, \eta) \right\}.$$

3.2 Results

When land security improves (η falls), individuals face a lower risk of losing land that they rent out after leaving farming, which increases the relative attractiveness of urban manufacturing jobs. For the older cohort, whose education is fixed, this operates solely through a *security channel*—the direct effect of reduced land-loss risk on the relative payoff to manufacturing—which raises the older cohort's share employed in manufacturing.[17] The younger cohort can respond along both margins: it can switch occupations directly through this security channel, and it can also acquire education, opening an additional *education channel* for reallocation from farming to manufacturing. Result 1 formalizes this cohort difference.

[17] See Appendix C for the formal derivation.

Result 1 (Cross-cohort responses).
Let $\eta_H > \eta_L$ denote the land-insecurity levels before and after the reform. For cohort $c \in \{O, Y\}$, let $H^c(\eta) \equiv \Pr(h_i = 1 \mid \eta)$ denote its share choosing education, and let $M^c(\eta) \equiv \Pr(k_i = M \mid \eta)$ denote its share employed in manufacturing.

(i) $H^O(\eta_L) = H^O(\eta_H)$, while $H^Y(\eta_L) \geq H^Y(\eta_H)$.

(ii) $M^Y(\eta_L) - M^Y(\eta_H) \;\geq\; M^O(\eta_L) - M^O(\eta_H)$.

Figure 1 illustrates how the security and education channels jointly shape occupational switching. The horizontal axis ranks individuals by their comparative advantage in manufacturing, while the vertical axis measures income. Before the reform, only those in the dark blue segment find manufacturing more attractive. Improved land security shifts the manufacturing income curve upward relative to farming income, from the dashed red line $I(M, \eta > 0)$ to the solid blue lines, expanding the set of individuals who find manufacturing preferable to farming.[18]

For individuals who are initially employed in farming, the pre-reform optimal education level is $h = 0$. For the older cohort, whose schooling is fixed at this pre-reform level, education therefore remains at $h = 0$ regardless of the reform. As land security improves, their manufacturing income increases to $I(M, h = 0)$, and individuals in Domain A find it worthwhile to switch occupations even without acquiring education: these are the *security-only switchers*. Occupational switching in Domain A is identical across cohorts, though some individuals in the younger cohort may additionally acquire schooling after the reform if the return to education in manufacturing is sufficiently high. Individuals in Domain B, by contrast, find manufacturing attractive only once they also invest in education, which raises their income to $I(M, h = 1)$: these are *education-enabled switchers*, observed only in the younger cohort.[19] This distinction motivates the empirical design in Section 4.2: by comparing younger cohorts of schooling age with older cohorts with fixed education, we identify the reform's effect on education (Domain A and B) and the reform's additional impact on occupational reallocation (Domain B), since individuals in Domain A switch to manufacturing regardless of education.

The figure also indicates which farmers are closest to the switching margin. Switchers lie in

[18] While farming income can also change with land security η, for simplicity, we hold it fixed to focus on changes in manufacturing income relative to farming income.

[19] Appendix C provides a formal characterization of these two types of switchers.

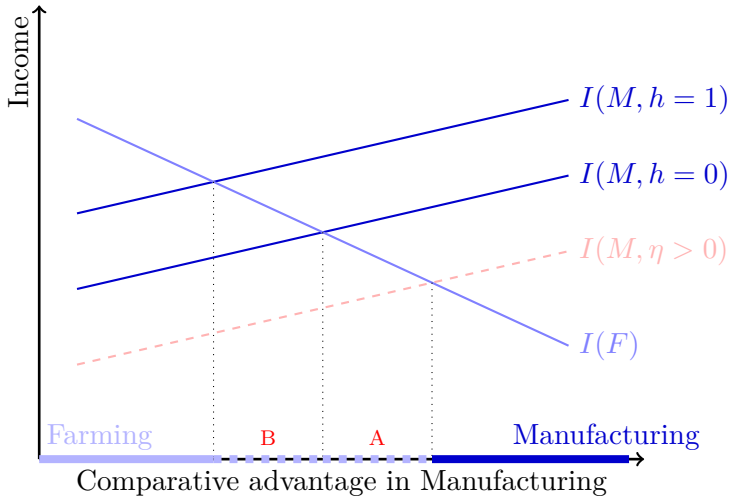


Figure 1: Two Types of Switchers in Response to Improved Land Security

Note: The horizontal axis ranks individuals by comparative advantage in manufacturing. The solid light blue line shows farming income ($I(F)$). The dashed red line shows manufacturing income under high land insecurity ($I(M, \eta > 0)$). The solid darker blue lines show manufacturing income after land security improves, without education ($I(M, h = 0)$) and with education ($I(M, h = 1)$). Domain A identifies *security-only switchers*, who move to manufacturing solely because land security improves, regardless of education. Domain B identifies *education-enabled switchers*, who move to manufacturing only after investing in schooling.

Domains A and B, to the right of the farming segment, because they have a higher comparative advantage in manufacturing than those who remain in agriculture. Among individuals initially working in agriculture, those with lower agricultural productivity z_F are closer to the switching margin and therefore respond more strongly to a given improvement in land security.[20] Result 2 formalizes this relationship.

Result 2 (Heterogeneity by z_F).
For $z_F \in \mathbb{R}_+$, let $H^Y(\eta \mid z_F) \equiv \Pr(h_i = 1 \mid \eta, z_F(i) = z_F)$ denote the share of the younger cohort with agricultural productivity z_F who would farm under the pre-reform regime and who choose education at land-insecurity level η. Then for $z_F^L < z_F^H$,

$$H^Y(\eta_L \mid z_F^L) - H^Y(\eta_H \mid z_F^L) \;\geq\; H^Y(\eta_L \mid z_F^H) - H^Y(\eta_H \mid z_F^H).$$

Intuitively, among individuals who farm before the reform, individuals with lower agricultural productivity have less to gain from remaining in farming conditional on other individual characteristics. A given improvement in land security is therefore more likely to make education and subsequent entry into manufacturing to become attractive. This

[20] Here, comparative advantage in farming is measured by the difference between an individual's potential income in farming and manufacturing, $I_F(i, \eta) - I_M(i, h, \eta)$, holding land security and education fixed. In both the empirical analysis and the quantitative model, we find that absolute advantage in farming, measured by z_F, is positively correlated with this income-based measure of comparative advantage. We therefore use z_F as a proxy for both absolute and comparative advantage when characterizing heterogeneity.

heterogeneity operates through Domain B. For individuals in Domain A, who would enter manufacturing without education once land is secure, the decision to additionally acquire education is governed mainly by their manufacturing ability z_M rather than z_F.

Results 1 and 2 together generate two testable predictions. First, improvements in land security generate larger educational and occupational responses among younger cohorts whose education is not yet fixed. Second, within the younger cohort, the educational response is stronger among individuals with lower agricultural productivity and, in our setting, lower comparative advantage in farming. Section 4 tests both predictions.

4 The Impact of the Land Reform

4.1 Short-run effects on current workers

We first examine the causal impact of the RLCL reform on outcomes of the existing workforce at the time of the reform by exploiting its staggered rollout across provinces. Because these workers had already completed their schooling, their responses correspond to the security channel in our model: reallocation with a given level of human capital. We build on the framework of Chari et al. (2021), who focused on land-renting activity, but replace their two-way fixed-effects estimator with recent advances in difference-in-differences methodology (ETWFE, Wooldridge, 2025). This estimator avoids contaminated comparisons with already-treated units by saturating the model with fixed effects for all possible combinations of treatment groups (as defined by the timing of treatment) and year, yielding separate average treatment effects on the treated (ATTs) for each group-year cell. Figure 2 plots ATTs aggregated by event time (calendar year relative to RLCL adoption). We observe a shift in both the magnitude and significance of the coefficients after the implementation of the reform for all four outcomes. The pattern of increasing effect sizes is consistent with land security strengthening gradually after the RLCL takes effect. Pre-implementation trends are flat and insignificant across all outcomes, supporting the parallel pre-trends assumption. Technical details of the ETWFE specification and the aggregation of group-time ATTs are provided in Appendix D.

Panel (a) replicates Figure 2 of Chari et al. (2021), confirming their original results. A key question left open by prior work, however, is whether increased land market activity translated into overall household income gains. We address this directly using household-level income data

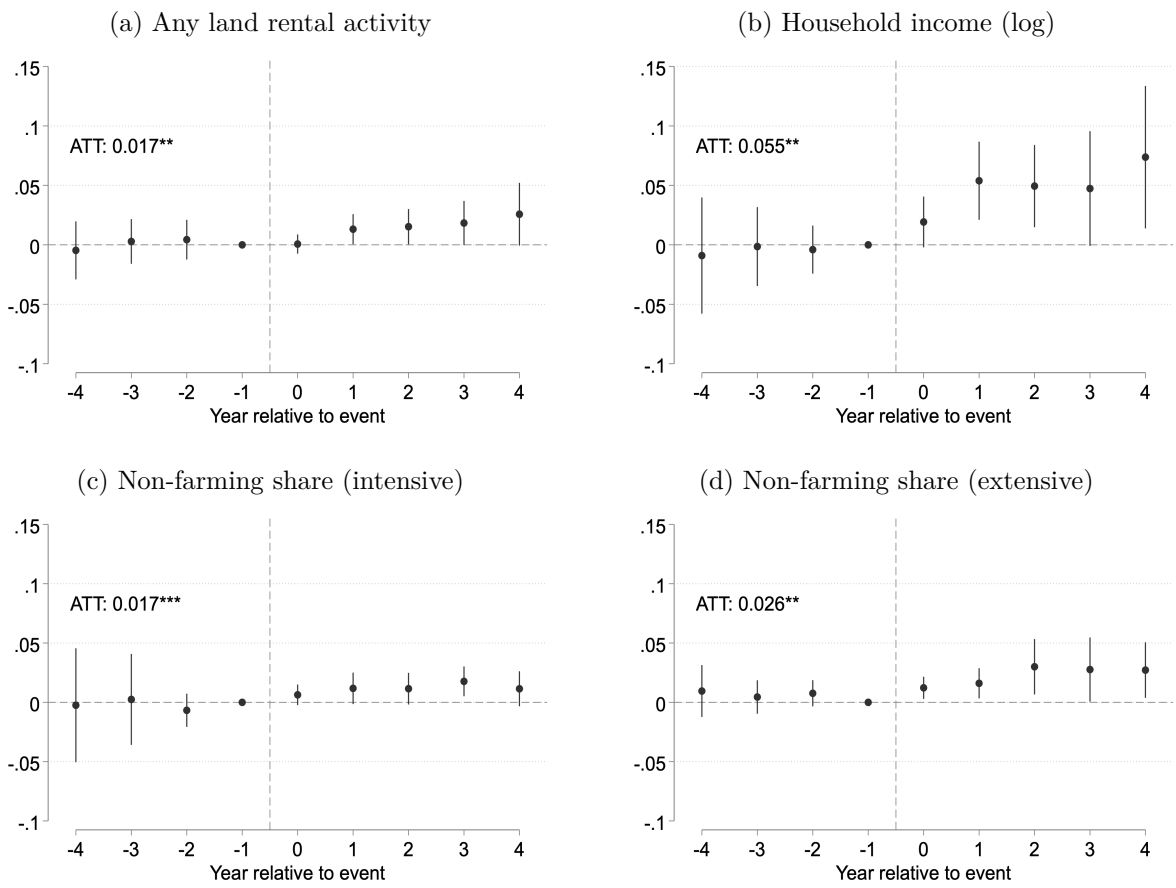


Figure 2: Effects of the RLCL on the existing workforce

Note: The figure illustrates the short-run effects of the RLCL on household and village outcomes of the existing workforce between 2000 and 2013, estimated using equation (D2). Event time is the calendar year relative to provincial RLCL adoption, with the event time of -1 omitted as the reference period. Panels (a) and (b) present the estimated effects on household-level rental activity and income: an indicator for whether a household engaged in any rental activity in a given year, and the log of annual household income, respectively. Panel (c) reports the estimated effects on the share of household members engaged in non-farming activities, estimated at the household level. Panel (d) reports the estimated effects on the share of non-farming households in the village, estimated at the village level. Standard errors are clustered at the province level.

from the NFP. Panel (b) presents an event study of total income, showing a sustained increase of just over 5 percent beginning in the second year post-reform.[21] Notably, the estimated average treatment effect on income closely mirrors that on rental activity.

The model in Section 3 predicts that improved land security should shift the current workforce out of farming. The ideal test would examine individual occupational choices directly, but the NFP data lack the necessary granularity. We turn instead to two measures at

[21]Because the way income by source is recorded changes across NFP survey waves, we cannot construct a consistent panel measure of income composition and therefore do not disaggregate the event-study effects in Panel (b) by source; we leave this valuable exercise for future work. Conceptually, gains in total income can arise from both agricultural and non-agricultural activities. High-agricultural-productivity households tend to rent in land to expand cultivated acreage, raising farm earnings. By contrast, lower-agricultural-productivity households often rent out land and shift into non-agricultural employment; for them, higher rental income together with increased non-agricultural earnings drives the observed rise in total household income.

the household and village levels. The first, drawn from the household survey, is the share of household members engaged in non-farming activities, capturing the intensive margin of labor reallocation. The second, from the village survey, is the share of households fully engaged in non-farming, capturing the extensive margin. Panels (c) and (d) of Figure 2 report the corresponding event-study estimates. Both measures show a pronounced reallocation of labor out of farming and into non-farm employment, with average increases of 1.7 and 2.6 percentage points, respectively.

4.2 Cohort effects on education and talent allocation

We now turn to our primary objective: identifying how the RLCL reshaped educational investments among younger cohorts who could still adjust their schooling, relative to older cohorts whose education was already fixed at the time of the reform. This comparison isolates the effects of improved land security operating through the human capital channel.

4.2.1 Identification

Our identification strategy builds directly on the model's key prediction in Section 3. Because the RLCL strengthens land tenure security, it raises the returns to non-agricultural work and induces labor reallocation out of farming. As discussed in Result 1, older cohorts, whose education was predetermined, respond only through the security channel (security-only switchers), while younger cohorts can respond additionally through the education channel (education-enabled switchers). Comparing the younger and older cohorts therefore allows us to identify the reform's effect on education for both types of switchers and its occupational effect specifically for education-enabled switchers.[22]

We use age 15 at the time of RLCL implementation to separate younger and older cohorts.[23] China mandates nine years of schooling (six years of primary and three years of middle school), and compliance is high. Children typically start primary school at age six, placing the high school enrollment decision at around age 15. Progression to high school generally requires passing an entrance exam and incurring tuition and forgone earnings. Importantly, the high school decision serves as the first gateway to both college and skilled employment: the standalone

[22] Although our theoretical model abstracts to binary educational and occupational choices, the same identification logic applies in richer settings with multiple non-agricultural occupations that vary in their complementarity with education.

[23] This same cutoff is used in Lu et al. (2023), which examines how industrial policies shape human capital investments in China.

return to high school is modest, but its value lies in preserving the option to attend college, to which returns are substantially higher (Li et al., 2012). Consequently, individuals who have already passed age 15 have largely fixed their educational trajectories: they have either chosen to continue toward college or exited the academic track, with limited opportunity to reverse that choice later in life. The age-15 cutoff therefore represents a salient, forward-looking margin through which the RLCL could shift incentives by raising expected returns to continued schooling.

Formally, we estimate a birth-cohort-based ETWFE model that exploits the age-15 threshold. Instead of defining time by calendar year, we define "time" by birth cohort (the year turning age 15) and assign provinces to treatment groups by their RLCL adoption year.[24] The specification saturates the model with group-by-birth-cohort indicators and includes province and birth-cohort fixed effects to accommodate staggered rollout. Identification comes from comparing adjacent birth-cohorts—those younger than 15 at adoption versus those just older—within provinces that adopted RLCL, benchmarked against the corresponding cohort differences in never-treated provinces. This recovers long-run effects on educational attainment and occupational choices operating through the human capital channel.

The model estimates separate average treatment effects on the treated (ATTs) for each group-cohort combination and is written as follows:

$$Y_{icp} = \sum_{g \in G} \sum_{s=s_0}^{g-1} \theta_{gs}^{pre} D_{gs} + \sum_{g \in G} \sum_{s=g}^{C} \theta_{gs}^{post} D_{gs} + \xi_p + \phi_c + \varepsilon_{icp}, \tag{1}$$

where Y_{icp} is the long-term outcome of individual i who turned 15 in year t and registered in province p. D_{gs} is a dummy that takes the value of 1 if the observation is in the treatment group g and turned 15 in year s and 0 if otherwise. G is a set that indicates at what time the reform started in the province. ξ_p and ϕ_c are province and cohort fixed effects, respectively. θ_{gs}^{post} represents average treatment effect that individuals in treatment group g experience if turning 15 in year s after the RLCL was implemented (ATT(gs)). We use provinces that have never implemented the RLCL by 2015 (never-treated group) as the control group, which allows us to estimate θ_{gs}^{pre}, that is, the pre-treatment effects for individuals in the treatment group g

[24] In China, the school year begins in September, and only children who turn seven before September of that year may enroll in primary school. Accordingly, we define a birth cohort as individuals born between September of year t and August of year t+1, who reach age 15 during school year t. The implementation timing of the RLCL is adjusted on the same basis to ensure that treatment status aligns with the schooling calendar relevant for the high school enrollment decision.

who turn 15 before the RLCL was implemented.[25]

In this cohort-based ETWFE framework, identification of θ_{gs}^{post} hinges on no anticipation and common cohort trends assumptions. The no anticipation assumption requires that individuals who turned 15 before the RLCL did not adjust their education or occupational choices in anticipation of the reform. These older cohorts may still be affected by the RLCL after its implementation, for example through occupational adjustments that do not operate through education. The conditional common trends assumption requires that, in the absence of the RLCL, cohort trends in education and occupational choices would have evolved similarly across treated and never-treated provinces, after conditioning on province- and cohort-invariant covariates. The key condition underlying this assumption is that the distribution of innate ability evolves similarly across provinces over time: if the composition of talent within each cohort follows parallel trends, then education and occupational choices should also follow parallel trends absent the reform. Our event study estimates for pre-treatment cohorts can provide evidence supporting these assumptions. At the end of this section, we conduct several robustness checks to further support our identification strategy.

The coefficient θ_{gs}^{post} is the group-cohort average treatment effect on the treated (ATT). It captures the causal change in outcome Y_{ipc} (e.g., educational and occupational choices) for individuals who turn 15 in cohort s in provinces that implemented RLCL in year g, relative to the counterfactual without exposure to the reform by age 15. For example, $\theta_{04,04}^{post}$ is the effect for individuals who turned 15 in 2004 in a province adopting RLCL in 2004, whereas $\theta_{04,05}^{post}$ is the effect for individuals in the same province who turned 15 in 2005. Identification comes from comparing cohort-to-cohort changes in outcomes within provinces with adoption year g to the corresponding cohort changes in never-treated provinces over the same years. The interpretation of θ_{gs}^{post} differs by outcome. Because older cohorts had largely fixed their educational trajectory before the reform, θ_{gs}^{post} identifies the causal effect of the RLCL on education. For occupational choices, older cohorts may also adjust after the reform, so θ_{gs}^{post} captures the additional effect of exposure to the RLCL by age 15, on top of the effect of exposure after age 15. We then aggregate θ_{gs}^{pre} and θ_{gs}^{post} by event time, yielding an event study that tests for pre-existing cohort trends and average treatment effects for post-reform cohorts.

[25] Using not-yet-treated provinces as an alternative control group yields similar estimates (Appendix Table E2).

4.2.2 Impact on education and occupational choices

In Figure 3, we present the event study estimates calculated using equation (1) for all six main outcomes. For each outcome, the model is estimated for males and females separately because we expect the land reform to operate through different mechanisms by gender, given gender differences in land-rights allocation in China.[26] The first two outcomes focus on education choices. The third outcome focuses on migration. The last three outcomes focus on their occupational choices, where we further restrict the sample to individuals who turn 15 in or before 2007 to observe their post-education outcomes (at least 23 in 2015). Due to this restriction, the sample size is reduced to 76,610 (71,704) from 98,581 (92,303) for males (females), among which 66,851 (49,555) were working at the time of the survey. Due to the same reason, we are unable to report the post-treatment effects for event times larger than three for labor market outcomes.

Event time is defined as the year a cohort turns 15 relative to the province's RLCL adoption year. Non-negative event times correspond to cohorts exposed to the RLCL by age 15, while negative event times correspond to older cohorts whose education was largely fixed when the reform took effect. Three points are worth noting. First, for children who were already passed 15 when the RLCL was implemented, the cohort trends in all six outcomes are similar in the treated and never-treated provinces. Such parallel pre-treatment trends provide support for the identification assumption of the ETWFE estimator. Second, for males, effect sizes generally rise with event time — except for the outcome variable "currently working" — which is consistent with education and occupational choices adjusting to improved land security after the RLCL. Cohorts who turned 15 in the implementation year show smaller effects, likely because they had less time to respond.[27]

Third, we find no effects for females. This, however, does not mean that the RLCL had no effect on women's land entitlements. Our design only captures effects that operate through schooling decisions at age 15, and women's entitlements depend mainly on marriage and recognition of collective membership (Section 2.1). What the gender difference suggests is that the mechanism at work at age 15 is specific to males. This is consistent with the RLCL alleviating the use-it-or-lose-it mobility friction for sons, whose land insecurity primarily stemmed from the risk of losing existing household land claims upon leaving agriculture. This

[26] More details can be found in Section 2 and Appendix A.

[27] Estimates from alternative difference-in-differences estimators are similar; see Figure E3 and Appendix E.2.

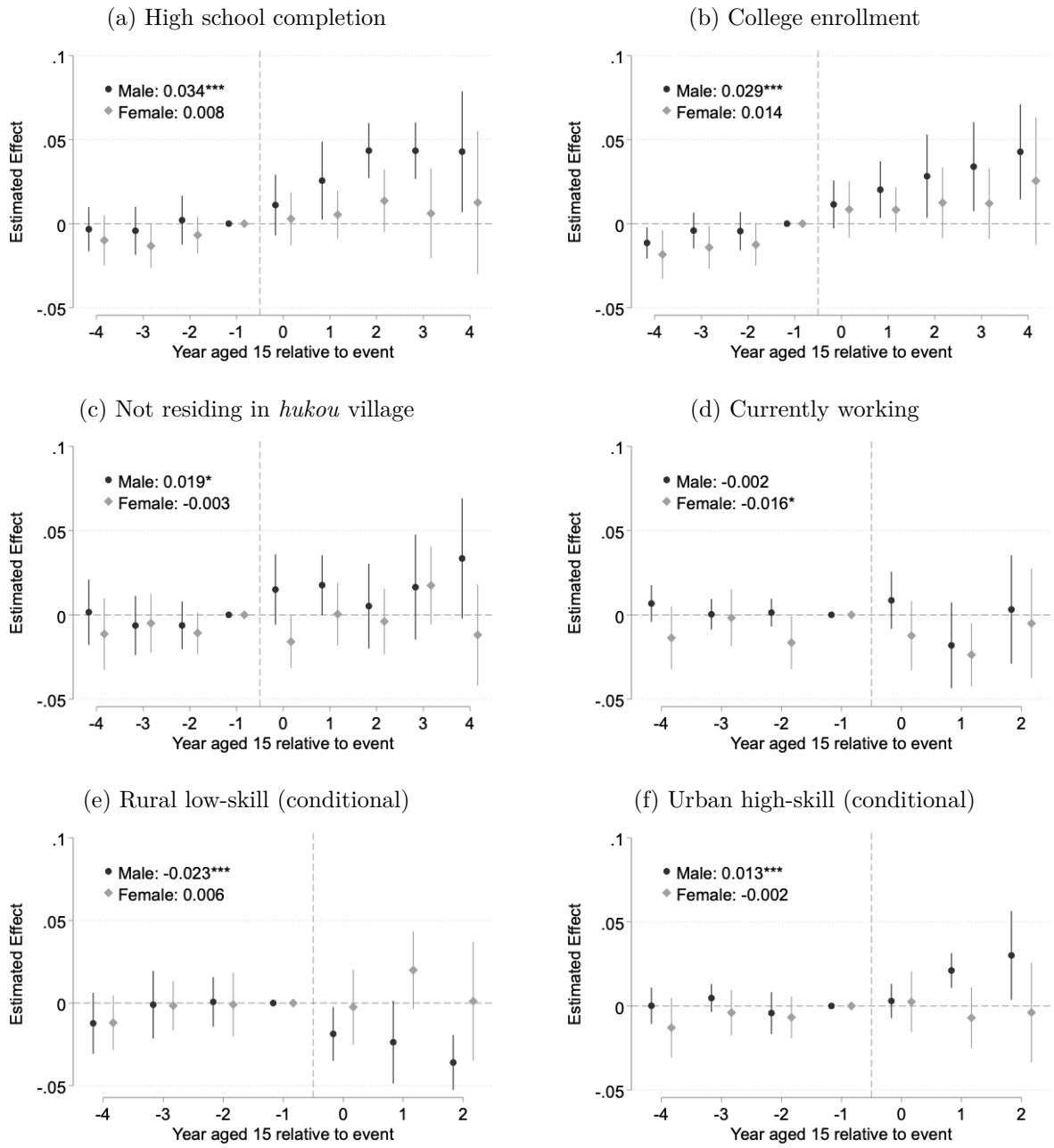


Figure 3: Effects of the RLCL on the younger generation

Note: Figure 3 reports the effects of the RLCL on the outcomes of the younger generation, based on the equation (1) for both genders. The sample includes individuals who turned age 15 between 2000 and 2010 and whose *hukou* is registered in a rural village. Panels (a) and (b) report the effects on high school completion and college enrollment, respectively. Panel (c) reports the effects on migration. Migration is defined as an indicator equal to one if the individual has ever left the place of hukou registration, and zero otherwise. Panels (d)–(f) present the effects on labor market outcomes. Currently working is defined as whether an individual was employed in 2015. Rural low-skilled is defined as whether an individual is employed in farming or construction, while urban high-skilled refers to being employed in high-skilled occupations as defined in Section 2.2.1. Standard errors are clustered at the province level.

gender difference also argues against channels that are not gender-specific, such as changes in household resources at the time of the schooling decision. We return to this and other alternative explanations in Section 4.4.

The aggregated ATT estimates are reported in the event-study plots and Table 1. With only 29 province-level clusters, conventional cluster-robust inference may over-reject the null hypothesis (Cameron & Miller, 2015). We therefore also report p-values from the wild-cluster bootstrap using six-point Webb weights (Webb, 2023). As the effects on the female sample are all small and insignificant, we focus on the effects on the male sample. Exposure to the RLCL at or before age 15 raises the probability of high school completion by 3.4 percentage points and college enrollment by 2.9 percentage points, representing 9% and 17% of the respective means. The similar magnitude of effects across the two margins is particularly informative. College admission in China is highly competitive and constrained by quotas, so mainly high-ability students can successfully enter college, and the returns to college substantially exceed those to high school only (Li et al., 2012). Similar effects on high school completion and college enrollment therefore suggest that the RLCL did not merely induce marginal students to stay in school longer. Rather, it enabled high-ability rural children who were constrained by land insecurity to invest in education and pursue their academic potential. Absent the RLCL, these students, with high returns to education, would have been held back from non-agricultural careers, representing a misallocation of talent. Consistent with the effect on education, we find that the RLCL increases the probability of not residing in the *hukou* village by 1.9 percentage points (4% of the mean), although this effect is only marginally significant.

Columns (4)–(6) of Table 1 show the effects of exposure to the RLCL at age 15 on occupational outcomes.[28] As discussed in Section 3, the comparison between younger and older cohorts identifies occupational changes only for education-enabled switchers, those who move to non-agricultural work only after acquiring more education. Security-only switchers, who would have changed occupation regardless of education, contribute to the occupational response of both younger and older cohorts equally and therefore do not appear in the estimated cohort difference. This is why the occupation effect is smaller than the education effect: the former captures only a subset of those who change education and occupation among the younger cohorts. Consistent with this interpretation, among male cohorts exposed to the RLCL by age 15 or earlier, the probability of entering a rural low-skill occupation, which includes farming and construction jobs, falls by 2.3 percentage points (7% of the mean), roughly two-thirds the size of the education effect. Among employed men, the 1.3 percentage point increase in high-skill occupations is economically meaningful—approximately 10 percent

[28] The education effects are similar when we restrict the sample to the employed individuals used in the occupation analysis, as shown in Table E3.

Table 1: Effects of the RLCL on education and labor market outcomes

	Education		Migration	Labor Market		
	(1) High school completion	(2) College enrollment	(3) Not residing in *hukou* village	(4) Currently working	(5) Rural low-skill	(6) Urban high-skill
Panel A. Males						
RLCL at age 15	0.034***	0.029***	0.019*	-0.002	-0.023***	0.013***
	(0.009)	(0.009)	(0.010)	(0.010)	(0.008)	(0.005)
Wild-bootstrap p-value	[0.006]	[0.009]	[0.098]	[0.883]	[0.018]	[0.063]
Mean dep var	0.377	0.170	0.533	0.873	0.340	0.125
Number of clusters	29	29	29	29	29	29
Observations	98581	98581	98515	76610	66851	66851
Panel B. Females						
RLCL at age 15	0.008	0.014	-0.003	-0.016*	0.006	-0.002
	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)
Wild-bootstrap p-value	[0.424]	[0.188]	[0.708]	[0.113]	[0.552]	[0.849]
Mean dep var	0.360	0.178	0.525	0.691	0.316	0.152
Number of clusters	29	29	29	29	29	29
Observations	92303	92303	92118	71704	49555	49555
Gender difference, asymptotic p-value	0.000	0.043	0.005	0.146	0.024	0.075
Gender difference, wild-bootstrap p-value	0.008	0.078	0.044	0.273	0.068	0.147

Note: Table 1 reports the effects of the RLCL on education and labor market outcomes corresponding to Figure 3, based on equation (1). The sample includes individuals who turned age 15 between 2000 and 2010 and whose *hukou* is registered in a rural village. Columns (1) and (2) report effects on high school completion and college enrollment, respectively. Column (3) reports effects on migration, defined as an indicator for not residing in the *hukou* village. Columns (4)–(6) report labor-market outcomes. Currently working indicates employment in 2015. Rural low-skill indicates employment in farming or construction, while urban high-skill refers to employment in high-skill occupations as defined in Section 2.2.1. Panel A reports results for males and Panel B reports results for females. Standard errors in parentheses are clustered at the province level. Significance stars are based on conventional province-clustered inference. Bracketed p-values are from symmetric wild-cluster bootstrap tests with 9,999 Webb-weight replications and province-level clustering. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

of the sample mean, although it is only marginally significant under wild-cluster bootstrap inference. Combined with the decline in rural low-skill work, the null effect on current employment, and the absence of effects on low-skill urban occupations documented below, the pattern indicates occupational upgrading rather than merely migration or labor-force entry. In the language of the talent-allocation literature, stronger land rights allow some rural youth to acquire the human capital required to work in occupations in which their non-agricultural skills are more productive (Hsieh et al., 2019).

4.2.3 Are the occupational effects driven by education?

Our model has a sharp prediction for occupational choices: the cohort comparison should capture only education-enabled switchers, because security-only switchers respond equally in younger and older cohorts. An alternative explanation is that younger cohorts respond more strongly to improved land security itself, owing to longer career horizons or lower occupational

switching costs upon entering the labor market. To distinguish between these interpretations, we need to show that the cohort effects on occupation would not exist without the change in education. This condition is akin to an exclusion restriction: exposure to the RLCL by age 15 affects occupational choices only through education. We provide three pieces of evidence in support of this condition.

First, exposure to the RLCL by age 15 increases entry into urban high-skill occupations (column (6) of Table 1), where high school education is almost a prerequisite. Second, the effects on urban low-skill occupations, which require less formal education, are small and insignificant (Figure E7), suggesting that younger cohorts did not simply move into non-agricultural work without upgrading their education. Third, education and occupational switches occur jointly. Table E6 reports effects on joint education-occupation categories. The RLCL reduces the probability of working in rural low-skill jobs without a high school degree (column (1)) and increases the probability of working in urban jobs with a high school degree (column (4)), but has no effect on urban employment without a high school degree (column (3) Together, these three patterns indicate that the occupational effects are driven by the reform's effect on education rather than by a stronger security-channel response among younger cohorts.

This schooling-occupation complementarity is consistent with a broader literature showing that the skill content of labor-market opportunities shapes educational investment. Low-skill export-manufacturing opportunities can induce earlier school exit (Atkin, 2016), whereas technology-oriented zones in China and access to skilled service-sector jobs can increase schooling or post-school training (Jensen, 2012; Lu et al., 2023). Our setting identifies a complementary institutional mechanism: stronger land rights make education-intensive occupations more attainable.

4.2.4 Impact on income

Another implication of our theoretical model concerns cohort differences in income gains. Younger cohorts benefit more from improved land security because they can adjust their education, and therefore experience larger income gains than older cohorts whose schooling is already fixed. These gains differ across individuals depending on their post-reform adjustment. For those who would remain in agriculture whether young or old, or switch occupations without acquiring additional education, the income gains are the same across cohorts and do

not appear in the cohort difference. For those who would acquire additional education if young, income gains arise either from the productivity increase associated with additional education or from switching into higher-skill non-agricultural occupations, which is itself enabled by education. As a result, the cohort comparison identifies an effect on income that is driven by education, as with the cohort effect on occupation.

To provide suggestive evidence on the cohort effect in income, we estimate the effects of exposure to the RLCL by age 15 on four measures of imputed log annual earnings. We predict earnings based on age profiles, education, occupation, and location, using prediction models estimated on CFPS 2018 and applied to the mini-census sample (see details in Appendix B.1.3). Across four different measures, male cohorts exposed to the RLCL by age 15 have predicted log annual earnings that are 0.008 to 0.010 log points higher (Table E1). The corresponding effects for females are smaller and statistically insignificant. Because earnings are imputed, these estimates capture the earnings value of the reform-induced changes in schooling and occupation under the 2018 earnings structure, rather than an effect on actual earnings.

4.2.5 Robustness checks

Wild-cluster bootstrap tests confirm significant gender differences in high school completion (1% level), migration (5% level), and college enrollment and rural low-skill employment (10% level; Table 1). Our results are not sensitive to the choice of estimator: event-study estimates from conventional TWFE and from the estimator of Callaway & Sant'Anna (2021) closely track the ETWFE estimates for all main outcomes (Figure E3). Using not-yet-treated provinces as additional controls in ETWFE also yields similar estimates (Table E2). We validate age 15 as the exposure cutoff by re-estimating Equation (1) at exposure ages 12–18: for boys, effects are similar for exposure up to age 15 and fall sharply after age 15, consistent with the high school enrollment decision being made at that age (Figure E6). Agricultural-tax controls leave the main estimates unchanged (Panel A of Table E4), and the urban placebo yields null effects (Table E5). We also control for cohort trends in pre-reform economic development and the results hold, as shown in Panel B of Table E4. Exposure to the RLCL by age 15 does not affect the probability of holding an urban *hukou*, so selective exit from our rural-*hukou* sample is unlikely to drive the results (Figure E4). Finally, replicating the analysis on the NFP sample yields larger but qualitatively similar education effects (Figure E1). The NFP sample consists of households that remained active in agriculture before the reform and are therefore more likely

to be compliers to the reform; the broader census sample also includes households that had already left agriculture before the RLCL, diluting the estimated effects. Appendix E.2 provides details on each robustness check.

4.3 Heterogeneity by agricultural productivity

We now turn to the second empirical challenge: establishing whether the education response reflects changes in the relative returns to different occupations, rather than other effects of the reform such as higher household income. Result 2 provides a testable prediction. Individuals with lower productivity in farming have a weaker agricultural outside option and are therefore more likely to adjust their schooling and subsequently reallocate toward non-agricultural work when land insecurity is reduced. An income channel does not generate this pattern unless the reform raises income more for low-productivity households, which we show is not the case. We test this prediction using pre-reform household agricultural productivity measured in the NFP. Section 4.3.1 constructs this measure and shows that it is also informative about comparative advantage across sectors, and Section 4.3.2 presents descriptive and regression evidence.

4.3.1 Measuring agricultural productivity

We construct each household's pre-reform agricultural productivity from the NFP, measured by the marginal product of land (MPL) in 2003.[29] Throughout, we exploit only within-village variation in MPL by normalizing each household's productivity relative to its village mean.[30] We use this measure as the empirical counterpart of farming productivity, z_F, in the model.

Although MPL directly measures productivity in farming, the data suggest that it is also informative about households' relative opportunities across sectors. Table E7 examines the relationship between pre-reform agricultural productivity and households' allocation of labor to non-agricultural activities. Conditional on household labor supply, skill composition, landholdings, household size, and village fixed effects, lower-MPL households are more likely to engage in non-agricultural activities, devote more labor days to non-agricultural work, and

[29]The detailed procedure for calculating MPL follows Chari et al. (2021) and is summarized in Appendix F.

[30]A potential concern is that within-village MPL differences reflect natural land quality rather than household agricultural productivity. Two features of the setting make this unlikely to be the primary source of the variation we exploit. First, Adamopoulos et al. (2022) report an across-village standard deviation in potential yield of 0.096. Even if within-village land-quality dispersion were as large as this across-village benchmark, our calculations imply that it would account for at most 27% of the within-village productivity dispersion in the NFP. Second, village administrations historically allocated households multiple plots in part to equalize land endowments and productive opportunities (Rozelle & Li, 1998; Brandt et al., 2004; Ho, 2005). Together with our use of only within-village variation, these features make it unlikely that differences in natural land quality are the main driver of the MPL heterogeneity we exploit.

allocate a larger share of labor outside agriculture. While MPL should therefore be interpreted first as a measure of absolute productivity in farming, these sectoral-allocation patterns suggest that it also contains information about comparative advantage across sectors (Alvarez-Cuadrado et al., 2026).

A separate concern is that MPL may proxy for other determinants of children's educational investment. In particular, low-MPL households could simply be poorer or differ systematically in pre-existing human capital. Figure E8 provides little support for these interpretations. Panels (a) and (b) show that household income is approximately flat in MPL both before and after the RLCL, suggesting that agricultural productivity is not simply a proxy for household resources. Panels (c) and (d) plot the high-school share among the household's rural labor force against MPL. Before the RLCL, this relationship is also essentially flat (Panel (c)), indicating that low-MPL households did not already have a more educated rural workforce. After the reform, a positive gradient emerges (Panel (d)), with a more educated rural workforce remaining in higher-MPL households. This pattern is consistent with differential compositional adjustment of the rural workforce across the MPL distribution, as more educated members of lower-MPL households leave for non-agricultural work.

4.3.2 Time and cohort effects by agricultural productivity

Before turning to the formal estimates, Figure 4 provides a descriptive counterpart to the cohort comparison underlying our empirical design. We plot educational attainment against pre-reform MPL separately for two birth cohorts: individuals born in 1985–1987, who turned 15 before the reform, and individuals born in 1992–1994, who turned 15 after it. Both groups are relatively young within the NFP sample—distinct from the existing workforce examined in Section 4.1—so this comparison focuses on the human-capital margin. Among the older cohorts, whose schooling was largely predetermined when the reform arrived, educational attainment is approximately flat in MPL for both genders. Among younger male cohorts who could still adjust their schooling, however, a pronounced negative gradient emerges: boys from lower-MPL households are substantially more likely to complete high school. No comparable pattern appears for girls.

These descriptive patterns are consistent with the mechanism in Result 2: the schooling response appears largest among boys from households with lower agricultural productivity, for whom farming provides a weaker outside option. Combined with the greater pre-reform

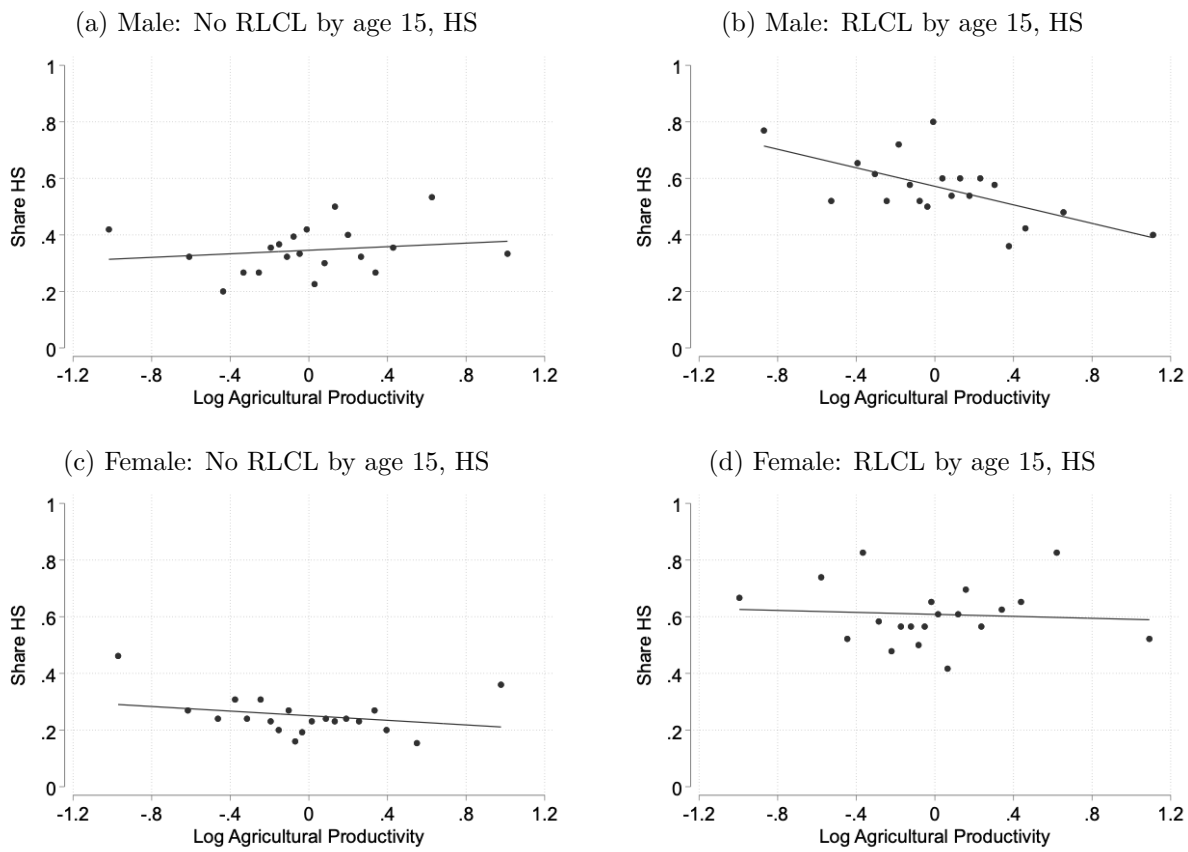


Figure 4: Agricultural productivity and education for the younger and older cohorts

Note: This figure presents the relationship between agricultural productivity and high school completion for males and females in two different cohorts. We focus on individuals living in provinces that implemented RLCL between 2003 and 2006 and use those born between 1985 and 1987 to represent the first generation (Panels a and c) and those born between 1992 and 1994 as the second generation (Panels b and d). The first generation was at least 16 when the RLCL took effect, and the second generation turned 15 after the RLCL. The upper panels (a) and (b) show males, and the lower panels (c) and (d) show females. The y-axis is the high school completion rate in all four panels. The x-axis is the deviation in log agricultural productivity before 2003 relative to the village mean. The solid line is the estimated linear relationship between log agricultural productivity and high school completion. Data are based on the NFP.

non-agricultural orientation of these households, the evidence is also consistent with the reform allowing young individuals to sort more strongly toward sectors in which they have a comparative advantage.

We now test these patterns formally using both equations (D2) and (1). Our findings are presented in Table 2, where we split the NFP sample into low- and high-MPL groups within village based on whether pre-reform agricultural productivity is above or below the village mean.

Columns (1)–(3) report estimates for the existing workforce from equation (D2). Column (1) shows that the land reform raises household income by about 4% in both productivity groups, with no difference between them (p-value = 0.923). The point estimates suggest that the composition of this increase differs: low-MPL households shift labor toward non-agricultural work, particularly in urban areas, whereas high-MPL households expand their labor supply

Table 2: Heterogeneous impacts by household productivity

	First generation RLCL implementation			**Second generation** Exposure by age 15	
		Labor days per capita			
	(1) Household Income (log)	(2) Agriculture	(3) Non-agri	(4) High School Male	(5) High School Female
Panel A: Low-MPL households					
RLCL	0.042*	-0.594	3.525	0.242***	0.065
	(0.021)	(2.358)	(2.960)	(0.057)	(0.069)
Dep. Var. Mean	9.430	79.921	35.154	0.433	0.397
Clusters	28	28	28	28	28
Observations	99599	83321	83321	1878	1739
Panel B: High-MPL households					
RLCL	0.043	1.519	1.162	0.082*	0.064
	(0.031)	(2.022)	(3.581)	(0.049)	(0.080)
Dep. Var. Mean	9.476	80.741	31.661	0.437	0.415
Clusters	28	28	28	28	28
Observations	93105	77978	77978	1803	1565
Diff p-value	0.923	0.062	0.269	0.042	0.798

Notes: This table presents the effects of the land reform on the outcomes, conditional on pre-reform household productivity. RLCL denotes province-year implementation in columns (1)–(3) and exposure by age 15 in columns (4)–(5). Column (1) reports the effect of the land reform on household income, estimated using equation (D2), with standard errors clustered at the province level. Columns (2) and (3) present the effects of the land reform on labor supply, also estimated using equation (D2). The outcome variables are measured as the number of days worked per capita among household members aged 25 and above. Standard errors are clustered at the province level. Means of the outcome variables are calculated based on the values of the sample in 2003. Household productivity is proxied by the marginal product of land (MPL) in 2003. Columns (4) and (5) report the effects on high school completion for males and females, respectively, estimated using equation (1). Standard errors are clustered at the province level. Panel A presents results for households with low MPL, while Panel B shows results for households with high MPL.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

within agriculture. These estimates are imprecise, and only the difference in agricultural labor days is marginally significant (p-value = 0.062), but the pattern is consistent with Chari et al. (2021), who show that low-MPL households tend to rent land out to high-MPL households.

In Columns (4)–(5), we shift to the younger cohorts not yet in the labor market using the cohort-based specification in Equation (1). Consistent with Result 2, exposure to the RLCL by age 15 increases high school completion by 24.2 percentage points among boys from low-MPL households, about three times the 8.2 percentage point increase among boys from high-MPL households, and the difference is statistically significant (p-value = 0.050). Both effects are larger than the census estimates in Section 4.2, because the NFP sample consists of households that were actively farming before the reform and were therefore more directly exposed to land insecurity. Due to data limitations, we are unable to explore corresponding heterogeneity in their occupational choices. Girls from low- and high-MPL families experience a similar, though insignificant, increase in educational attainment following the reform,

consistent with the absence of main effects for girls. Together with the similar income gains across productivity groups in Column (1), this pattern is difficult to reconcile with a pure income channel. Section 4.4 provides further evidence against this explanation.

4.4 Alternative explanations

The heterogeneity results above are consistent with the model's prediction that improved land security raises schooling most strongly for children from households with weaker comparative advantage in agriculture. In this subsection, we provide further evidence against two alternative explanations for the education response. First, the schooling effects could reflect a household income channel: the reform may have raised household resources and thereby relaxed constraints on investments in education. Second, the reform may have changed parents' migration or occupational choices, which in turn affected children's educational outcomes. We argue that neither explanation can fully account for the empirical patterns.

4.4.1 Income effects

A natural alternative explanation is that the RLCL increased household income, thereby raising children's schooling through an income effect rather than a change in the expected returns to non-agricultural work. However, as shown in Section 4.3.2, household income increases by similar magnitudes among low- and high-MPL households, whereas the schooling response for boys is concentrated among low-MPL households.

We further examine this explanation by splitting households according to pre-reform income, savings, and parental education, each measured relative to the village mean. If relaxed financial constraints were the primary mechanism, we would expect stronger schooling responses among poorer households, those with lower savings, and those with less educated parents. Table E8 provides little support for this prediction. Income gains are similar across these groups, while boys' schooling responses are not systematically larger among more constrained households. If anything, the effects are stronger among higher-income households and those with more educated parents.[31] For girls, the point estimate is larger among low-savings households, potentially consistent with relaxed financial constraints benefiting daughters when households prioritize sons' education. However, the effect is small and

[31] One possible interpretation is that parental education partly reflects children's non-agricultural ability, which our model predicts also affects the education decision. Because these differences are imprecisely estimated, we do not pursue this interpretation further.

statistically insignificant. Overall, these patterns suggest that income effects alone are unlikely to explain the schooling response and are more consistent with our comparative-advantage mechanism.

4.4.2 Intergenerational spillovers from parental occupational adjustment

A second alternative interpretation is that the RLCL affected children's schooling indirectly through parents' migration and occupational decisions, rather than through children's own educational choices. This concern is particularly salient in our setting because 2000–2010 was also the period in which Chinese cities began systematically allowing migrant workers to bring their children along and enroll them in local compulsory schooling.[32] Two distinct channels could generate a spurious cohort pattern under this story. First, if the RLCL induced more parents to migrate for non-agricultural work, and migrating parents disproportionately brought along their younger children, those children could benefit from urban compulsory schooling even absent any change in their own educational decisions. Second, parents with children young enough to benefit from urban schooling—that is, those still under 15—might have been differentially induced to migrate by the RLCL, again generating a cohort pattern unrelated to children's own choices.

We find little support for either channel. Because national policy during this period targeted access to *compulsory* education rather than the transition to high school, the first channel should primarily show up as an effect concentrated on completing compulsory schooling around age 15, with limited scope to affect high school enrollment. On the contrary, we find no effect of RLCL exposure at age 15 on completing compulsory education, for either boys or girls (Appendix Figure E5). Moreover, the estimated effect of the RLCL on existing workforce's non-agricultural employment—the channel through which parents would migrate—is modest (Section 4.1), making it unlikely that migration-driven access to urban schooling alone can account for the large effect on high school completion we document. For the second channel, we estimate whether the RLCL differentially affected parental non-farm employment depending on the child's age at reform exposure using the NFP data; Appendix Figure E2 shows no evidence of such age-specific effects.

Together, these patterns point to a direct reoptimization of schooling among younger cohorts rather than intergenerational spillovers from parental migration or occupational change.

[32] See the Ministry of Education's 2003 notice on strengthening compulsory education for children of migrant workers in cities, http://www.moe.gov.cn/jyb_xxgk/gk_gbgg/moe_0/moe_9/moe_40/tnull_147.html.

5 Quantitative Analysis

Section 4 provides reduced-form evidence that stronger land rights increased schooling and shifted younger male cohorts toward skill-intensive, non-agricultural employment. These estimates, however, do not reveal the quantitative importance of the human-capital channel. In this section, we use the model to address three related questions. First, how much of the occupational reallocation is driven by endogenous schooling choices? Second, how much of the resulting income gains can be attributed to human-capital accumulation? Finally, what are the long-run implications for schooling, employment, and income once the entire working-age population has made its schooling decisions under secure land rights?

5.1 Education-enabled reallocation

We first quantify how much of the occupational reallocation induced by stronger land rights is enabled by additional schooling. The key is to compare workers whose schooling was already fixed at the time of the reform with younger cohorts who could still adjust their education. For the former, reallocation occurs with existing human capital; for the latter, it can also be facilitated by additional schooling. We therefore use the occupational response of the existing workforce (Section 4.1) as the baseline and interpret the additional response among younger cohorts (Section 4.2) as education-enabled reallocation. Section 4.2.3 provides further support for this interpretation by showing that the additional occupational response operates through education.

Combining estimates from the NFP and the mini-census, we quantify the contribution of education to occupational reallocation.[33] Eliminating land insecurity raises non-agricultural employment among the existing workforce by 2.6 percentage points, and by a further 2.3 percentage points among cohorts exposed by age 15.[34] Among cohorts who could still adjust their schooling, the education channel therefore accounts for $2.3/(2.6 + 2.3) = 47$ percent of

[33] Although the two estimates come from different data sources, both are defined over the rural-*hukou* population. Following Result 1, the accounting attributes the additional response of younger cohorts to their ability to adjust schooling, assuming that the security channel operates similarly across cohorts. Consistent with this interpretation, Figure 3 shows no differential pre-trends in occupational outcomes among older cohorts, whose schooling was already fixed at the time of the reform.

[34] The household-level effect on the share of non-farming members is smaller, at 1.7 percentage points (Section 4.1). We use the village-level effect on the share of non-farming households instead, for two reasons. First, the model treats each household as a single worker who chooses between farming and non-farming, which corresponds to the extensive margin of a household leaving agriculture rather than to the reallocation of labor among household members. Second, the village survey covers all households registered in the village, matching the rural-*hukou* population in the census, whereas the household sample excludes households that did not farm before the reform.

the total occupational response. At the time of the reform, however, cohorts able to adjust their schooling accounted for about 25 percent of the rural population. Weighting the two responses accordingly, the education channel accounts for about 18 percent of economy-wide occupational reallocation. This share rises as reform-exposed cohorts replace older generations in the labor force, a process we examine in Section 5.3.

5.2 Calibration

The accounting exercise in Section 5.1 quantifies the role of education in occupational reallocation, but it cannot tell us how the reform's income gains are divided across different margins of adjustment. The main purpose of the calibration is to decompose these gains into three components: the reallocation of land within agriculture, the reallocation of workers across occupations with schooling held fixed, and the additional reallocation enabled by education. The calibrated model also allows us to examine the reform's longer-run consequences once all cohorts have made their schooling choices under secure land rights.

Our Roy model with endogenous education contains 15 parameters governing agricultural and non-agricultural productivity, schooling choices, mobility frictions, and land insecurity.[35] We do not model the urban sector in general equilibrium because, by 2015, the additional high-skilled workers entering from rural areas as a result of the reform still constitute only a small share of the urban labor market.[36] A full general-equilibrium treatment of the urban sector would become more relevant over a longer horizon, as additional cohorts who were younger than 15 at the time of the reform enter the labor market. We therefore take non-agricultural wages as given and allow only the land rental price to adjust endogenously, clearing the common land market faced by both young and old cohorts before and after land reform.

Of the 15 parameters, six are jointly calibrated to empirical moments. Each of these parameters generally affects several model outcomes, so identification does not rely on a one-to-one mapping between parameters and moments. Instead, the parameter vector is disciplined by differences in how the six targeted moments respond to changes in the parameters. The economic intuition is nevertheless transparent. Pre-reform schooling and non-agricultural employment shares discipline the balance between education returns and

[35]The only departure from the theoretical framework in Section 3 is that we relax the assumption on φ and allow it to vary across individuals according to a log-normal distribution, $\log \varphi_i \sim N(\mu_\varphi, \sigma^2_\varphi)$.

[36]To assess this, we estimate that the reform induced approximately 500,000 additional individuals to attend college by 2015. This represents only about 0.5% of the college-educated working population, suggesting that the reform was unlikely to have materially affected urban wages or other labor-market conditions by that point.

costs, (τ, c), together with the non-agricultural wage, w_M. The occupational response to the reform when schooling is held fixed identifies the strength of the land-insecurity distortion, governed by μ_ϕ. Comparing this response with the additional schooling and employment adjustments that arise when education is allowed to respond helps separate the return to schooling from its cost. Finally, heterogeneity in schooling responses across low- and high-productivity farmers is informative about how non-agricultural opportunities covary with agricultural ability, thereby disciplining the joint distribution of sectoral abilities, (ρ, σ_M).

The remaining nine parameters are fixed using normalizations or external estimates, primarily from Adamopoulos et al. (2022, 2024), with the reform modeled as eliminating land-loss risk by setting $\eta = 0$. The calibrated parameter values are reported in Online Appendix Table G1, while Appendix G.2 provides the definitions of the six target moments and further calibration details. As a formal check on local identification, Appendix Table G2 reports the Jacobian of the six model moments with respect to the six calibrated parameters, evaluated at the calibrated parameter values. The Jacobian is numerically full rank and exhibits the comparative-statics patterns described above, supporting local identification of the calibrated parameter vector.

Table 3: Calibration moments: data and model

	Data	Model
Panel A: Targeted moments		
High school completion share, *Old cohorts*	0.386	0.386
Non-agricultural employment share, *Old cohorts*	0.469	0.469
Non-agricultural employment response, *Old cohorts*	0.026	0.026
High school completion response, $Young - Old$	0.034	0.034
Non-agricultural employment response, $Young - Old$	0.023	0.023
Schooling-response ratio (low-z_F/high-z_F), $Young - Old$	2.951	2.951
Panel B: Non-targeted income comparison		
Log non-land income response, $Young - Old$	0.008	0.007

Notes: Panel A reports the six targeted moments defined in Appendix G.2. Panel B compares the model's young-minus-old response in mean log non-land income with the cohort effect on imputed log annual earnings in Appendix Table E1. Model income deducts individual-specific mobility costs from non-agricultural earnings and excludes education costs; non-land income excludes the return on owned land.

Table 3 evaluates the fit of the calibrated model. Panel A reports the six targeted empirical moments alongside their model counterparts. Overall, the model reproduces the targeted moments closely. Because the targets include both the response of the existing workforce and the cohort difference in non-agricultural employment, the calibrated model yields the same decomposition of occupational reallocation as the accounting exercise in Section 5.1.

Panel B compares the model's implied income effect with its empirical counterpart, which is not targeted in the calibration. Because younger and older cohorts face the same post-reform land market, the cohort difference in income in the model arises entirely from the education margin: higher earnings with occupation held fixed and education-enabled occupational reallocation. The model implies that younger cohorts experience a 0.65 percent increase in non-land income relative to older cohorts, close to the 0.8 percent cohort effect on imputed log annual earnings (Appendix Table E1), which likewise reflects only changes in education and occupation. The model thus generates income gains from the education margin of a plausible magnitude.

5.3 Income gains and longer-run implications

To assess the importance of each margin of adjustment, we decompose the income gains from eliminating land insecurity by sequentially allowing land, occupations, and schooling to adjust. Income here includes labor income, with non-agricultural earnings net of individual-specific mobility costs, plus rental income on owned land. It excludes education costs and the perceived φ-related loss from land insecurity. We compute the decomposition for the average individual who can still adjust their schooling, evaluated at the post-reform equilibrium rental price. Figure 5 reports each component as a share of the total gain.

With occupations and schooling held fixed, improved land security allows land to be reallocated among existing farmers and makes it safer for non-farmers to rent out their land. This land reallocation accounts for 42.3 percent of the total income gain. Allowing occupations to adjust adds a further 7.1 percent, as individuals can now leave agriculture for non-agricultural work in addition to renting out their land. Allowing schooling to respond adds the remaining 50.6 percent: 15.3 percent through higher earnings with occupation held fixed and 35.3 percent through education-enabled occupational reallocation.

These results have direct implications for the longer run. At the time of the reform, only about 25 percent of the population could still adjust their schooling. As reform-exposed cohorts replace older generations, the decomposition above increasingly describes the economy as a whole. This cohort-replacement process is an important driver of structural transformation (Porzio et al., 2022), and our results identify land security as one institutional determinant of its human-capital margin. To quantify these consequences, we compute a new equilibrium in which all working-age cohorts have made their schooling choices under secure land rights,

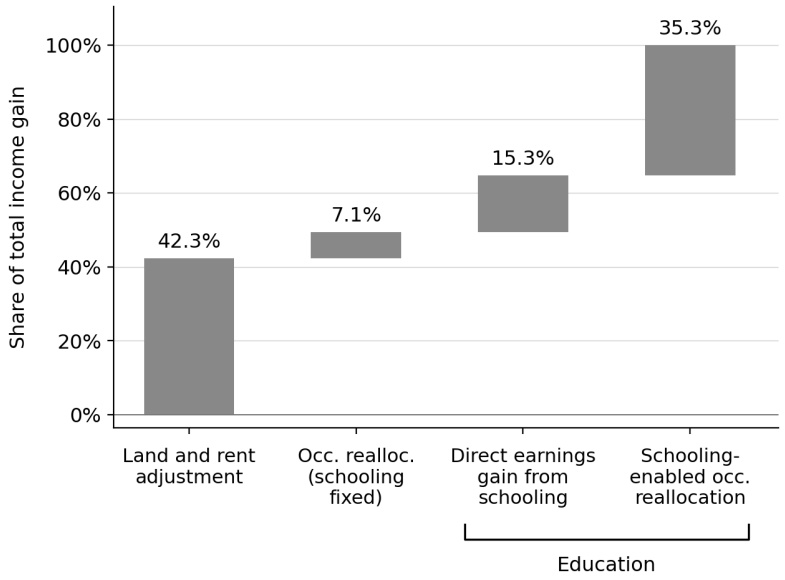


Figure 5: Decomposition of income gains from land security

Notes: The figure decomposes the income gain from eliminating land insecurity for an individual who can still adjust schooling, averaged over all types and evaluated at the post-reform rent q^*. Income is labor income, net of individual-specific mobility costs for non-agricultural workers, plus rental income on owned land; it excludes education costs and perceived land-loss costs. The components are added sequentially: (i) land holdings are reoptimized at q^*, holding schooling and occupations at their pre-reform levels; (ii) occupations adjust with schooling fixed; (iii) schooling adjusts with occupations held as in (ii); and (iv) occupations adjust given flexible schooling. The bracket groups the two schooling components, (iii) and (iv). Appendix G.3 defines the income measures and the decomposition.

re-solving schooling, occupational choice, and the land rental price. With non-agricultural wages unchanged, high school completion rises by 3.19 percentage points and non-agricultural employment by 4.62 percentage points relative to the pre-reform equilibrium, close to the 3.4 and 4.9 percentage points implied by a simple extrapolation of the reduced-form estimates. The further decline in the rental price slightly lowers the education share of the income gain, to 48.3 percent.

Because we do not model the urban labor market, these results hold non-agricultural wages fixed. To assess their sensitivity, we consider an assumed 3 percent reduction in the non-agricultural efficiency wage. In this case, the schooling gain falls to 1.96 percentage points and the non-agricultural employment gain to 3.56 percentage points. This scenario should be viewed as a pessimistic benchmark. Educational attainment remains low among China's rural population, which will make up a large share of the future labor force (Rozelle & Hell, 2020). Preparing these workers for high-skill urban employment need not depress wages: a larger supply of educated workers may encourage the adoption of skill-complementary technologies (Caselli & Coleman, 2006), as documented for China's higher education expansion (Che & Zhang, 2018). Such effects, which our partial-equilibrium framework does not capture, would sustain wages

and reinforce the gains from education.

6 Conclusion

This paper studies how improved land security affects talent allocation through two margins: the immediate reallocation of workers across occupations and the longer-run adjustment of human capital. We extend the classical Roy model of occupational choice to allow education to respond endogenously to mobility frictions. When schooling is more complementary to non-agricultural work than to farming, improved land security generates a stronger response among younger cohorts who can still adjust their education than among otherwise similar older cohorts whose schooling is already fixed. Using China's Rural Land Contracting Law as a natural experiment, we find precisely this pattern. Among individuals younger than 15 when the reform was adopted, high school completion and college enrollment each increased by approximately 3 percentage points, followed by a shift away from farming and construction and toward high-skill urban occupations.

These findings highlight a form of *intertemporal misallocation*. Mobility frictions can distort talent allocation before individuals enter the labor market by lowering the return to education and inducing young people to underinvest in human capital. Because such investments are costly to reverse, the effects of mobility barriers can persist even after the underlying institutional constraint is removed. Evaluating mobility-enhancing reforms only through their immediate effects on occupational choice can therefore substantially understate their longer-run impact.

More broadly, our results emphasize the complementarity between mobility and human capital in the process of structural transformation. Moving workers out of agriculture requires not only greater freedom to move, but also the skills needed to enter more productive non-agricultural activities. This mechanism extends beyond rural land institutions. Whenever livelihoods are tied to location- or occupation-specific assets, mobility barriers can shape both current occupational choices and earlier investments in skills. Accounting for these dynamic responses is therefore important for understanding the full effects of institutional reform on talent allocation and structural transformation.

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Appendix A Institutional Background and Legal Evidence

A.1 Two Illustrative Court Cases

The RLCL gave rural residents a legal basis for challenging village decisions. In one case, a graduate student from Jiangxi sued his village committee after being denied land compensation because he had transferred his *hukou* to his university.[37] The court ruled in his favor on appeal, affirming that leaving the village for education did not eliminate his collective land rights. In another case, a rural-to-urban migrant in Shandong sued a village committee for reclaiming and reallocating his land after he transferred his *hukou* to work in the city in 2003.[38] The court ordered the village to return the land and pay 60,000 yuan in compensation. These two cases illustrate the mechanism emphasized in the paper: the RLCL lowered the expected land-rights cost of schooling and migration by making land claims more legally enforceable.

A.2 Provincial Implementation of the Rural Land Contracting Law

Table A1 documents the provincial rollout of the Rural Land Contracting Law (RLCL). The national law was enacted in 2003, but local implementation depended on provincial regulations that specified when and how the central protections applied. The table reports the official provincial document, its announcement date, and its effective date. These local rules are important because they translate the national law into enforceable protections over land retention, leasing, and dispute resolution.

Yunnan's 2006 implementation illustrates the content of these provincial rules. Article 15 made the "use-it-or-lose-it" restriction explicit by listing circumstances under which village authorities could not reclaim contracted land during the contract period. These included cases in which a household member left the village for college, outside employment, or military service; married into another household but had not received contracted land at the new residence; or became divorced or widowed and either remained in the original village or had not received land elsewhere. Articles 18 to 29 further clarified rules for leasing, transferring leases, and resolving land-leasing disputes. Together, these provisions reduced the risk that education, migration, or non-agricultural work would weaken a household's land claim.

[37] https://zqb.cyol.com/content/2007-01/13/content_1641621.htm
[38] https://thepaper.cn/newsDetail_forward_25862181

Table A1: Province-level implementation of the Rural Land Contracting Law

Province	**Document Name**	**Announcement 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)	05/29/2006	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
Hubei	E Zheng Ban Han (2009) No. 051	06/18/2009	06/18/2009
Hebei	Hebei Province Rural Land Contracting Regulation	07/25/2013	11/01/2013

Note: This table illustrates the implementation of the RLCL in China across years and provinces. Some provinces have not announced any regulation before 2014: Beijing, Heilongjiang, Henan, Guangdong, Guizhou, Tibet, and Ningxia.

A.3 Land Disputes in the 2014 Judicial Record

A.3.1 Corpus Construction and Dispute Coding

To better understand how rural land institutions contribute to gender differences in land-rights insecurity, we classify 28,995 published judicial decisions concerning rural land disputes rendered in 2014, eleven years after the reform began to be rolled out in 2003.[39] Following J. Zhu (2026), we assign each decision to one of twelve mutually exclusive primary dispute categories based on the stated cause of action, the parties involved, their claims, and the factual description of the case. We then consolidate these categories into three broader groups: entitlement and distribution disputes, exercise and enforcement disputes, and other rural land disputes. Entitlement and distribution disputes include claims concerning collective membership arising from marriage or married-out status, the distribution of expropriation compensation, and other collective benefits. Exercise and enforcement disputes include the confirmation, control, or infringement of contracted land rights; the validity of land-transfer contracts; boundaries and registration; village-led land reallocation or reclamation; and rent or other contract-performance disputes.

Our preferred descriptive sample restricts attention to first-instance decisions in the 29 provinces covered by the paper's estimation sample and to cases in which all explicitly identified individual claimants have the same sex. We exclude cases with mixed-sex claimants or no explicitly sex-coded claimant. These restrictions yield 9,004 cases: 5,704 with male claimants and 3,300 with female claimants.

A.3.2 Dispute Composition by Gender

Table A2 reveals a pronounced gender difference in the legal margin under dispute. Entitlement-and-distribution disputes account for 68.9 percent of cases involving women, compared with only 22.9 percent of those involving men. This pattern suggests that women face substantially greater barriers in establishing land rights: they more often turn to the courts to obtain recognition as collective members and to establish their eligibility for land allocations, expropriation compensation, or other collective benefits. The converse pattern appears in exercise-and-enforcement disputes, which account for 64.6 percent of cases involving men but only 26.5 percent of those involving women. Men are therefore more likely

[39] 2014 is the first year for which judicial records were made systematically public. Focusing on cases from that year also minimizes overlap with the subsequent land-titling reform, which began in 2014.

to litigate over the possession, transfer, boundaries, reallocation, or enforcement of rights that have already been recognized. Taken together, these patterns indicate that women more often contest whether they hold a land claim at all, whereas men more often contest how an established claim may be exercised.

Table A2: Gender and the Legal Margin of Rural-Land Disputes, 2014

	Male claimants		Female claimants	
Case group	Count	Share (%)	Count	Share (%)
Entitlement and distribution	1,307	22.9	2,275	68.9
Exercise and enforcement	3,686	64.6	875	26.5
Other rural-land disputes	711	12.5	150	4.5
Total	5,704	100.0	3,300	100.0

Notes: The sample contains first-instance rural-land decisions from 2014 in the 29 provinces covered by the paper's estimation sample. Claimants must be explicitly male-only or female-only. Entitlement and distribution combines marriage or married-out-woman membership, expropriation-compensation distribution, and other collective-membership cases. Exercise and enforcement combines contracted-right confirmation, control, and infringement; transfer-contract validity; boundaries and registration; village reallocation or reclamation; and rent or contract performance. Shares are calculated within claimant sex.

Appendix B Data and Variable Construction

B.1 2015 Population Survey (Mini-Census)

B.1.1 Sample Construction and Summary Statistics

Table B1: Summary Statistics for Census Variables

Variables	Count	Mean	Std
Panel A: Boys Sample			
Birth	98581	1989.39	3.04
High School completion	98581	0.38	0.48
College enrollment	98581	0.17	0.38
Currently Working	76610	0.87	0.33
Rural low-skill	66851	0.34	0.47
Urban high-skill	66851	0.12	0.33
	Count	Mean	Std
Panel B: Girls Sample			
Birth	92303	1989.41	3.02
High School completion	92303	0.36	0.48
College enrollment	92303	0.18	0.38
Currently Working	71704	0.69	0.46
Rural low-skill	49555	0.32	0.47
Urban high-skill	49555	0.15	0.36

Note: This table reports the summary statistics for the Census sample. A low-skill job is defined as farming or low-skill construction. A high-skill job is defined based on the share of college graduates in an occupation, with occupations having at least 25% college graduates classified as high-skill.Panel A presents the summary statistics for the boys' sample, and Panel B presents those for the girls' sample.

B.1.2 Occupation Categories and *Hukou* Composition

Table B2: Rural and Agricultural *Hukou* Share by Occupation Category

Occupation Category	Rural Rate (%)	Agricultural Hukou Rate (%)
Agricultural Workers	86.38	98.70
Construction Workers	58.41	87.32
Low-skilled Service Workers	23.72	51.32
Low-skilled Manufacturing Workers	41.22	63.59
Other Low-skilled Workers	31.42	59.74
High-skilled Workers	17.77	26.86

Notes: This table reports the rural and agricultural *Hukou* share by occupation category. Sample restricted to individuals aged 25-50. Rural rate represents the percentage of the rural population within each occupation category. Agricultural hukou rate represents the percentage of agricultural household registration within each occupation category. Occupation categories based on 3-digit occupation codes with remapping to align with 2015 census classifications. Data are based on CFPS 2010.

B.1.3 CFPS-Based Earnings Index

Because the mini-census does not report labor income, we construct an imputed income index for each individual by fitting a regression-based prediction model using data from the 2018 wave of the China Family Panel Studies (CFPS), a large nationally representative survey, and applying the estimated model to the mini-census sample. We use the 2018 wave to avoid the labor market disruptions associated with the COVID-19 pandemic. This imputed measure captures the income level typically associated with an individual's occupation and education, rather than that individual's actual earnings, and proxies for the occupation's skill requirements and productivity. We estimate gender-specific regressions of log income on occupation, education, age, and province of residence using 6,622 employed respondents aged 21–46 in the 2018 CFPS. We apply the estimated coefficients to mini-census respondents at their 2020 ages, so the index values their characteristics using the 2018 earnings structure. We report results from four specifications that include different subsets of these predictors: (i) occupation only, (ii) education only, (iii) occupation and education, and (iv) occupation, education, and province-of-residence fixed effects (age is included as a control in all four). Specification (iv) is selected by cross-validation as our preferred model, though the four imputed measures yield very similar results, as shown in Appendix Table E1.

B.2 National Fixed Point Survey (NFP)

Table B3: Summary Statistics for NFP Variables

	(1) Count	(2) Mean	(3) Std
Panel A: Variables by Household-year			
Sown area (mu)	245527	15.98	946.14
Log total income	227725	9.95	0.88
Any land renting	245549	0.14	0.35
Non-farming share	244699	0.60	0.30
Panel B: Variables by Village-year			
Non-farming share(extensive)	4357	0.08	0.15
Panel C: Variables by Individual			
Gender	10218	0.53	0.50
Age	10218	23.53	2.94
High school rate	9967	0.44	0.50

Notes: This table provides summary statistics for the NFP samples. Panel A reports the household data.Log total income is expressed in real 2001 RMB (deflated using the 2001 CPI) and reported in logarithmic form. Any land renting refers to whether a household rented any land in that year. The nonfarm share is the nonagricultural labor share within a household. Panel B reports data at the village-year level. The nonfarm share is defined as the ratio of nonfarm households to farming households within a village. Panel C reports the individual data.

Appendix C Conceptual Framework: Proofs

This appendix derives the aggregate and cohort results from the model. The equilibrium rental price $q(\eta)$ is endogenous and determined by the land-market clearing condition. Throughout the analytical proof, land-loss exposure is homogeneous:

$$\varphi(i) = \varphi > 0 \qquad \text{for all } i. \tag{C1}$$

This benchmark isolates the main mechanism. The quantitative analysis relaxes this assumption and allows $\varphi(i)$ to vary across individuals.

C.1 Equilibrium rental price

Let $d_i(q)$ be land demand by a renter-in household, with $d_i'(q) < 0$. Let $s_j(r_j)$ be land supplied by a renter-out household. Under homogeneous exposure, the net rental return received by a renter-out household is

$$r_j = q - \eta\varphi, \qquad s_j'(r_j) > 0. \tag{C2}$$

Market clearing is

$$\sum_{i \in R} d_i(q) = \sum_{j \in S} s_j(q - \eta\varphi) + S_0, \tag{C3}$$

where S_0 is land supplied by non-farmer households. Equivalently, define excess demand by

$$E(q, \eta) = \sum_{i \in R} d_i(q) - \sum_{j \in S} s_j(q - \eta\varphi) - S_0 = 0. \tag{C4}$$

Lemma 1 (Equilibrium price response)**.** *Under $d_i'(q) < 0$ and $s_j'(r_j) > 0$, the equilibrium rental price satisfies*

$$q'(\eta) = \varphi \frac{\sum_{j \in S} s_j'(r_j)}{\sum_{j \in S} s_j'(r_j) - \sum_{i \in R} d_i'(q)} > 0, \tag{C5}$$

and

$$0 < q'(\eta) < \varphi. \tag{C6}$$

Proof. Differentiate (C4) with respect to η:

$$E_q(q, \eta) q'(\eta) + E_\eta(q, \eta) = 0. \tag{C7}$$

Because $d_i'(q) < 0$ and $s_j'(r_j) > 0$,

$$E_q = \sum_{i \in R} d_i'(q) - \sum_{j \in S} s_j'(r_j) < 0. \tag{C8}$$

Moreover, since $r_j = q - \eta\varphi$,

$$E_\eta = \varphi \sum_{j \in S} s_j'(r_j) > 0. \tag{C9}$$

Substituting these expressions into (C7) gives (C5). Finally, $-\sum_i d_i'(q) > 0$, so the denominator in (C5) is strictly larger than $\sum_j s_j'(r_j)$. Therefore the fraction multiplying φ is strictly between zero and one, which proves (C6). □

C.2 The occupational response of cohort O

We now specialize the occupational-choice analysis to the older cohort O. Education for this cohort was determined before the reform and is therefore fixed at

$$h_i^O = \bar{h}_i. \tag{C10}$$

The older cohort can consequently respond to land-security improvements only through the occupational margin.

C.2.1 Aggregate income

Define the net land-rental position of an individual in cohort O as

$$\ell_i^O(\eta) = \begin{cases} \ell_i^{\text{rent},O}(\eta), & \text{if } i \text{ is a farmer in cohort } O, \\ -\bar{\ell}_i, & \text{if } i \text{ works in manufacturing in cohort } O. \end{cases} \tag{C11}$$

Land-market clearing implies

$$\int \ell_i^O(\eta)\, dF^O(i) = 0. \tag{C12}$$

Let $Y^O(\eta)$ denote aggregate optimized income for cohort O. Rental payments are transfers within the economy:

$$-q(\eta) \int \ell_i^O(\eta)\, dF^O(i) = 0. \tag{C13}$$

Thus, the equilibrium rental price does not directly change aggregate income. Away from a measure-zero set of individuals exactly at an occupational threshold, the envelope theorem

gives

$$\frac{dY^O(\eta)}{d\eta} = -\varphi \int_{\ell_i^O(\eta)<0} [-\ell_i^O(\eta)]\, dF^O(i) < 0. \tag{C14}$$

The inequality holds whenever a positive amount of land is rented out and $\varphi > 0$. Therefore,

$$\eta \downarrow \quad \Longrightarrow \quad Y^O(\eta) \uparrow . \tag{C15}$$

C.2.2 Occupational choice

For the older cohort, define the manufacturing advantage as

$$\Delta_i^O(\eta) \equiv I_M^O(i, \bar{h}_i, \eta) - I_F^O(i, \eta). \tag{C16}$$

The older cohort chooses manufacturing if and only if

$$\Delta_i^O(\eta) > 0.$$

Accordingly, the cohort-O manufacturing share is

$$M^O(\eta) = \int \mathbf{1}\{\Delta_i^O(\eta) > 0\}\, dF^O(i). \tag{C17}$$

Consider an individual at the occupational switching margin who rents out land while retaining a positive amount for farming:

$$-\bar{\ell}_i < \ell_i^{\text{rent},O*}(\eta) < 0. \tag{C18}$$

The envelope theorem applied to the farmer's problem gives

$$\frac{\partial I_F^O(i, \eta)}{\partial \eta} = [\varphi - q'(\eta)]\ell_i^{\text{rent},O*}(\eta). \tag{C19}$$

For an older-cohort individual working in manufacturing,

$$\frac{\partial I_M^O(i, \bar{h}_i, \eta)}{\partial \eta} = [q'(\eta) - \varphi]\bar{\ell}_i. \tag{C20}$$

Consequently,

$$\frac{\partial \Delta_i^O(\eta)}{\partial \eta} = [q'(\eta) - \varphi]\bar{\ell}_i - [\varphi - q'(\eta)]\ell_i^{\text{rent},O*}(\eta) \tag{C21}$$

$$= [q'(\eta) - \varphi]\left[\bar{\ell}_i + \ell_i^{\text{rent},O*}(\eta)\right] < 0, \tag{C22}$$

where the last inequality follows from (C6) and (C18).

Thus, reducing η increases the relative attractiveness of manufacturing for the older cohort. If the distribution of individuals has positive density at the occupational switching margin, then

$$\frac{\partial M^O(\eta)}{\partial \eta} < 0. \tag{C23}$$

Therefore, improved land security increases the manufacturing share of the older cohort through the occupational security channel alone. Since education is fixed for cohort O, this cohort has no education-enabled switching margin.

C.3 Proof of Result 1

Let $\eta_H > \eta_L$ denote the land-insecurity parameters before and after the reform, respectively. The two cohorts face the same rental market and therefore the same equilibrium rental price $q(\eta)$ and price response $q'(\eta)$. They also have the same distribution of individual characteristics.

For each individual i, let $\bar{h}_i$ denote the education level that is optimal under the pre-reform environment η_H. Education for the older cohort is fixed at this pre-reform level:

$$h_i^O = \bar{h}_i. \tag{C24}$$

The younger cohort can reoptimize education after the reform.

For a given education level h, define the younger cohort's manufacturing advantage as

$$\Delta_h^Y(i,\eta) \equiv I_M^Y(i,h,\eta) - I_F^Y(i,\eta). \tag{C25}$$

Thus,

$$\Delta_1^Y(i,\eta) = I_M^Y(i,1,\eta) - I_F^Y(i,\eta) \tag{C26}$$

is the manufacturing advantage of an individual who acquires education. It compares educated manufacturing income with optimized farming income and is distinct from the return to

education within manufacturing, which is denoted by K_i below. For the older cohort, define

$$\Delta_i^O(\eta) \equiv I_M^O(i, \bar{h}_i, \eta) - I_F^O(i, \eta). \tag{C27}$$

For an individual at the relevant occupational margin, the occupational-choice derivation gives

$$\frac{\partial \Delta_h^Y(i,\eta)}{\partial \eta} = [q'(\eta) - \varphi] \left[\bar{\ell}_i + \ell_i^{\text{rent},Y*}(\eta)\right] < 0, \tag{C28}$$

where the inequality follows from $0 < q'(\eta) < \varphi$ and $-\bar{\ell}_i < \ell_i^{\text{rent},Y*}(\eta) < 0$. The same expression applies to the older cohort at its fixed education level. Thus, reducing η increases the relative attractiveness of manufacturing at every fixed education level. In particular, manufacturing employment cannot fall when land security improves.

Definition of the switching domains

Because education does not increase farming productivity and has positive cost $c > 0$, choosing $h = 1$ while remaining in farming is strictly dominated by choosing $h = 0$. Thus, individuals who are initially employed in farming have $\bar{h}_i = 0$.

Define the net return to education within manufacturing as

$$K_i \equiv I_M^Y(i, 1, \eta) - I_M^Y(i, 0, \eta) = w_M(1 - \kappa_i) z_M^Y(i)\tau - c. \tag{C29}$$

This return does not depend on η, because the rental-income and land-insecurity terms are identical under $h = 0$ and $h = 1$.

The security-only switching domain is

$$\mathcal{A} \equiv \left\{ i : \max_{h \in \{0,1\}} \Delta_h^Y(i, \eta_H) \leq 0, \quad \Delta_0^Y(i, \eta_H) \leq 0 < \Delta_0^Y(i, \eta_L) \right\}. \tag{C30}$$

These individuals remain in farming before the reform but enter manufacturing after the reform even when education is held fixed at $h = 0$.

The education-enabled switching domain is

$$\mathcal{B} \equiv \left\{ i : \max_{h \in \{0,1\}} \Delta_h^Y(i, \eta_H) \leq 0, \quad \Delta_0^Y(i, \eta_L) \leq 0 < \Delta_1^Y(i, \eta_L), \quad K_i > 0 \right\}. \tag{C31}$$

These individuals remain in farming after the reform if education is not acquired, but enter

manufacturing after acquiring education. Because the definition of $\mathcal{B}$ imposes $K_i > 0$, individuals in $\mathcal{B}$ strictly prefer $h = 1$ to $h = 0$ once they enter manufacturing. Thus, every individual in $\mathcal{B}$ increases education after the reform.

Some individuals in Domain $\mathcal{A}$ may also acquire education. Define this subset as

$$\mathcal{A}^+ \equiv \{i \in \mathcal{A} : K_i > 0\}. \tag{C32}$$

Educational response

Education is fixed for the older cohort, so its educational attainment does not respond to the reform:

$$\overset{O}{\Pr}(h = 1 \mid \eta_L) = \overset{O}{\Pr}(h = 1 \mid \eta_H). \tag{C33}$$

For the younger cohort, if $K_i \leq 0$, education does not increase manufacturing income enough to cover its cost, so the individual never chooses $h = 1$. If $K_i > 0$, choosing $h = 1$ yields higher manufacturing income than choosing $h = 0$. Since education does not affect farming income, the individual then chooses $h = 1$ if and only if educated manufacturing is preferred to farming, namely if

$$I_M^Y(i, 1, \eta) > I_F^Y(i, \eta), \tag{C34}$$

or equivalently, if $\Delta_1^Y(i, \eta) > 0$. By equation (C28),

$$\frac{\partial \Delta_1^Y(i, \eta)}{\partial \eta} < 0. \tag{C35}$$

Therefore, when land security improves and η falls, $\Delta_1^Y(i, \eta)$ increases. This weakly expands the set of younger-cohort individuals who choose education.

Therefore, the younger cohort's educational attainment weakly increases when land security improves. In particular, a positive-measure Domain $\mathcal{B}$ alone is sufficient for a strict education response, even if no individual in Domain $\mathcal{A}$ acquires education. More generally, the increase is strict whenever

$$\Pr(i \in \mathcal{A}^+ \cup \mathcal{B}) > 0. \tag{C36}$$

Occupational response

Define the older cohort's manufacturing choice by

$$m_i^O(\eta) \equiv \mathbf{1}\left\{\Delta_i^O(\eta) > 0\right\}, \tag{C37}$$

and the younger cohort's manufacturing choice by

$$m_i^Y(\eta) \equiv \mathbf{1}\left\{\max_{h\in\{0,1\}} \Delta_h^Y(i,\eta) > 0\right\}. \tag{C38}$$

Because $\bar{h}_i = 0$ for individuals who are initially employed in farming, the older cohort switches through Domain $\mathcal{A}$ only. Its reform-induced switching set is therefore

$$\mathcal{S}^O = \mathcal{A}. \tag{C39}$$

The younger cohort can replicate every switch in $\mathcal{A}$ by choosing $h = 0$, and can additionally switch through the education-enabled Domain $\mathcal{B}$. Hence,

$$\mathcal{S}^Y = \mathcal{A} \cup \mathcal{B}. \tag{C40}$$

Consequently,

$$\mathcal{S}^Y \setminus \mathcal{S}^O = \mathcal{B}. \tag{C41}$$

The reform-induced increase in the manufacturing share of the older cohort is the measure of $\mathcal{A}$, whereas the corresponding increase for the younger cohort is the measure of $\mathcal{A}\cup\mathcal{B}$. Therefore,

$$\overset{Y}{\Pr}(M \mid \eta_L) - \overset{Y}{\Pr}(M \mid \eta_H) \geq \overset{O}{\Pr}(M \mid \eta_L) - \overset{O}{\Pr}(M \mid \eta_H), \tag{C42}$$

with a strict inequality whenever $\mathcal{B}$ has positive measure.

Thus, the younger cohort has a weakly positive educational response and a weakly larger occupational response to improved land security. The educational response is strict whenever $\mathcal{A}^+ \cup \mathcal{B}$ has positive measure, and the occupational response is strict whenever $\mathcal{B}$ has positive measure.

□

C.4 Proof of Result 2

The proof focuses on the education-enabled switching domain $\mathcal{B}$. For individuals in the security-only domain $\mathcal{A}$, the decision to acquire education conditional on entering manufacturing is governed by the net return to education in manufacturing, which depends on z_M and does not directly depend on z_F. The z_F heterogeneity characterized below therefore concerns primarily the education-enabled margin.

Consider an individual in the younger cohort who can still choose education. Hold manufacturing productivity, mobility frictions, land endowment, and all other individual characteristics fixed. We study how the education-enabled manufacturing decision varies with agricultural productivity z_F.

For an individual who can enter manufacturing with education, define

$$\Delta_1^Y(i,\eta,z_F) \equiv I_M^Y(i,1,\eta) - I_F^Y(i,\eta,z_F). \tag{C43}$$

Manufacturing income does not directly depend on agricultural productivity, so

$$\frac{\partial I_M^Y(i,1,\eta)}{\partial z_F} = 0. \tag{C44}$$

The only direct effect of z_F on the manufacturing advantage therefore comes through optimized farming income. By the envelope theorem, the derivative of optimized farming income can be evaluated holding the individual's optimal land-rental choice fixed at its optimum:

$$\frac{\partial I_F^Y(i,\eta,z_F)}{\partial z_F} = p_a A_a \left[\bar{\ell}_i + \ell_i^{\text{rent},Y*}(\eta,z_F)\right]^{\theta} > 0. \tag{C45}$$

Substituting these two derivatives into the definition of $\Delta_1^Y(i,\eta,z_F)$ gives

$$\frac{\partial \Delta_1^Y(i,\eta,z_F)}{\partial z_F} = \frac{\partial I_M^Y(i,1,\eta)}{\partial z_F} - \frac{\partial I_F^Y(i,\eta,z_F)}{\partial z_F} = -p_a A_a \left[\bar{\ell}_i + \ell_i^{\text{rent},Y*}(\eta,z_F)\right]^{\theta} < 0. \tag{C46}$$

Higher agricultural productivity thus reduces the relative attractiveness of educated manufacturing employment.

The envelope theorem gives the following effects of land insecurity on optimized farming

income and manufacturing income:

$$\frac{\partial I_F^Y(i,\eta,z_F)}{\partial \eta} = [\varphi - q'(\eta)]\ell_i^{\text{rent},Y*}(\eta, z_F), \tag{C47}$$

and

$$\frac{\partial I_M^Y(i,1,\eta)}{\partial \eta} = [q'(\eta) - \varphi]\bar{\ell}_i. \tag{C48}$$

Therefore,

$$\frac{\partial \Delta_1^Y(i,\eta,z_F)}{\partial \eta} = [q'(\eta) - \varphi]\bar{\ell}_i - [\varphi - q'(\eta)]\ell_i^{\text{rent},Y*}(\eta, z_F) \tag{C49}$$

$$= [q'(\eta) - \varphi]\left[\bar{\ell}_i + \ell_i^{\text{rent},Y*}(\eta, z_F)\right] < 0, \tag{C50}$$

where the last inequality follows from $0 < q'(\eta) < \varphi$ and $-\bar{\ell}_i < \ell_i^{\text{rent},Y*}(\eta, z_F) < 0$. Let $\eta^*(z_F)$ denote the land-insecurity threshold at which the education-enabled manufacturing option becomes equally attractive to farming:

$$\Delta_1^Y\left(i, \eta^*(z_F), z_F\right) = 0. \tag{C51}$$

The implicit function theorem gives

$$\frac{d\eta^*(z_F)}{dz_F} = -\frac{\partial \Delta_1^Y/\partial z_F}{\partial \Delta_1^Y/\partial \eta} < 0. \tag{C52}$$

Thus, individuals with higher agricultural productivity require a larger improvement in land security before education and subsequent entry into manufacturing become attractive.

For two individuals with $z_F^L < z_F^H$, we therefore have

$$\eta^*(z_F^L) > \eta^*(z_F^H). \tag{C53}$$

Consequently, over a reform from η_H to η_L, the education-enabled switching condition

$$\eta_L < \eta^*(z_F) \leq \eta_H \tag{C54}$$

is satisfied for a wider range of reform thresholds among lower-productivity individuals. The inequality is strict whenever the reform crosses a positive measure of low-productivity education thresholds but not the corresponding high-productivity thresholds.

Therefore, conditional on other individual characteristics, lower agricultural productivity is associated with a stronger educational response to improved land security. In the empirical and quantitative applications, where agricultural productivity is positively correlated with income-based comparative advantage in farming, this also implies a stronger response among individuals with lower comparative advantage in farming.

□

Appendix D Extended Two-way Fixed Effects Estimator

Both empirical analyses in this paper use the extended two-way fixed effects (ETWFE) estimator of Wooldridge (2025): the short-run analysis of current workers estimated on the NFP household panel, and the cohort analysis of the younger generation estimated on the mini-census. This appendix describes the estimator, its application to each analysis, and the estimation targets we report in the main text.

D.1 Limitations of Standard TWFE

With homogeneous treatment effects, the standard approach is a two-way fixed-effects (TWFE) model:

$$Y_{ipt} = \beta \, PostRLCL_{pt} + \xi_p + \phi_t + \varepsilon_{ipt}, \tag{D1}$$

where $PostRLCL_{pt}$ is an indicator for whether the RLCL had been implemented in province p by period t, and ξ_p, ϕ_t are province and period fixed effects. Under common trends and homogeneous effects, β identifies the average treatment effect on the treated.

With staggered adoption, treatment effects may vary across groups and over time — for instance, effects may be larger in early-adopting provinces or smaller for individuals who were already near age 15 when the reform arrived. In such cases, the TWFE estimate in (D1) may be biased due to "forbidden comparisons," where already-treated units serve as controls for later-treated units (Borusyak et al., 2024). We follow Wooldridge (2025) and saturate the model with group-by-period interaction dummies, restricting the control group to never-treated provinces, to obtain heterogeneity-robust group-time average treatment effects on the treated (ATTs).

D.2 Short-Run Analysis: Panel ETWFE

For the short-run analysis of current workers, we estimate:

$$Y_{htp} = \sum_{g \in G} \sum_{s=s_0}^{g-1} \theta_{gs}^{pre} D_{hgs} + \sum_{g \in G} \sum_{s=g}^{T} \theta_{gs}^{post} D_{hgs} + \psi_h + \phi_t + \varepsilon_{htp}, \tag{D2}$$

where Y_{htp} is the outcome of household h in year t registered in province p; D_{hgs} equals one if household h belongs to treatment group g (provinces that implemented the RLCL in year g) and zero otherwise; G is the set of adoption years; and ψ_h, ϕ_t are household and year fixed

effects.[40] We use provinces that never implemented the RLCL by 2015 as the control group.

The coefficient θ_{gs}^{post} captures the ATT for households in group g in post-reform year $s \geq g$. For example, $\theta_{04,04}^{post}$ is the 2004 effect for Hunan households (where the RLCL took effect in 2004), while $\theta_{04,05}^{post}$ captures their 2005 effect. The never-treated control group also allows estimation of θ_{gs}^{pre}, the pre-treatment placebo effects for group g in years $s < g$.

Identification of θ_{gs}^{post} rests on two assumptions. The *no-anticipation assumption* requires that outcomes in pre-reform years are unaffected by the future reform, so that $\theta_{gs}^{pre} = 0$ for all $s < g$. The *conditional parallel trends assumption* requires that, in the absence of the RLCL, households in treated provinces would have followed the same time path as households in never-treated provinces, conditional on household and year fixed effects. The flat pre-trends visible in Figure 2 support both assumptions.

D.3 Long-Run Analysis: Cohort ETWFE

For the cohort analysis of the younger generation, we re-index "time" by birth cohort — the calendar year an individual turns 15 — and assign provinces to treatment groups by their RLCL adoption year. The resulting cohort-based ETWFE is given by equation (1) in Section 4.2. Effects are identified by comparing adjacent birth cohorts within treated provinces to the corresponding cohort differences in never-treated provinces.

The identification assumptions are analogous: older cohorts who turned 15 before the RLCL should be unaffected by the reform (no anticipation), and cohort trends in outcomes should run parallel across treated and never-treated provinces absent the reform (conditional parallel trends). The flat pre-trends in Figure 3 support both assumptions.

D.4 Estimation Targets

The ETWFE estimator yields granular group-by-period ATTs. We report two aggregated quantities. The first is a single weighted-average ATT across all treated observations:

$$\bar{\hat{\delta}} = \sum_{g=q}^{T} \sum_{s=g}^{T} \frac{N_{gs}}{N_D} \hat{\delta}_{gs},$$

where N_{gs} is the number of observations in group g in period s, and $N_D = \sum_{g=q}^{T} \sum_{s=g}^{T} N_{gs}$ is the total number of post-treatment observations. This is the single aggregate estimate reported

[40] Given the unbalanced panel structure of our data, we explicitly include household fixed effects, which is identical to the "chained difference-in-differences" framework proposed by Bellégo et al. (2025).

in Table 1 and Table E4. The second is an event-time-specific estimand that averages over the group dimension:

$$\overline{\hat{\delta}}_{\cdot s} = \sum_{g=q}^{s} \frac{N_{gs}}{N_{\cdot s}} \hat{\delta}_{gs},$$

where $N_{\cdot s} = \sum_{g=q}^{s} N_{gs}$. These estimates underlie the event-study figures (Figures 2 and 3). Standard errors for both targets are computed as weighted linear combinations of the group-period-specific estimates, accounting for their covariance.

One caveat applies to the event-study estimand: because early-treated groups contribute more observations at long event times, changes in $\overline{\hat{\delta}}_{\cdot s}$ across event times reflect both genuine dynamics and compositional shifts in group weights. We note this where relevant in the discussion of the figures.

Appendix E Empirical Analysis: Additional Results and Robustness

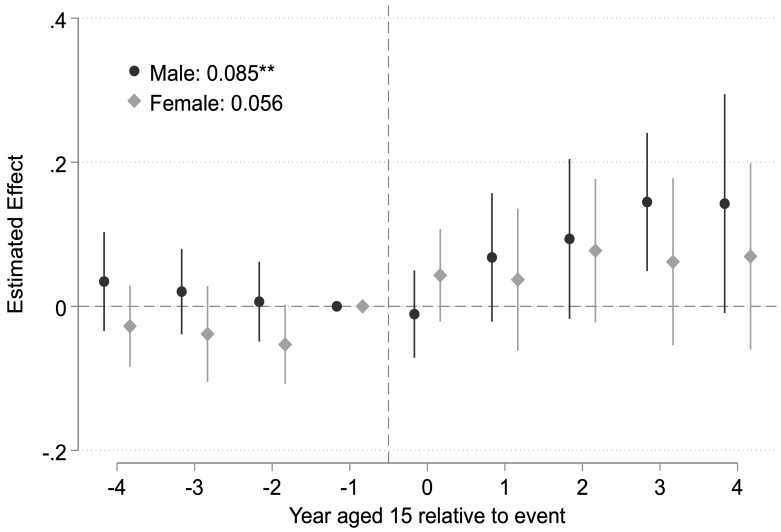


Figure E1: The Effects of the RLCL on high school completion using NFP

Note: This figure reports the results of replicating the main results of the long-run effects on high school completion from the 2015 Census by using 2013 NFP for both genders, based on the equation (1) at the individual level. The sample consists of individuals who turned 15 between 2000 and 2010. Standard errors are clustered at the province level.

(a) Father non-farm employment

(b) Mother non-farm employment

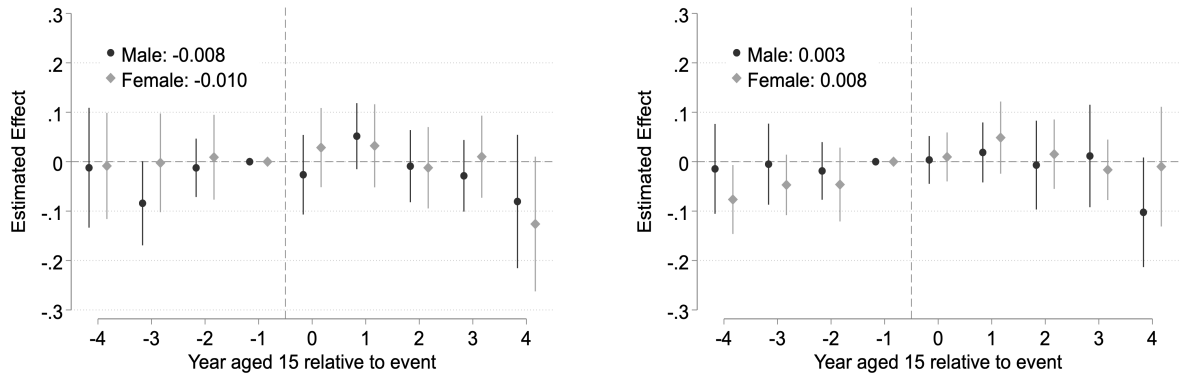


Figure E2: Parental non-farm employment by child exposure age

Note: This figure reports whether the RLCL differentially affected parental non-farm employment by the child's age at reform exposure, using the NFP data. Standard errors are clustered at the province level.

E.1 Effects on predicted income

Table E1 reports the full set of predicted-income results discussed in the main text (Section 4.2).

Table E1: Effects of the RLCL on predicted log annual earnings

	(1) Model (1) Occupation	(2) Model (2) Education	(3) Model (3) Occu. + educ.	(4) Model (4) Occu. + edu. + prov.
Panel A. Males				
RLCL at age 15	0.008*	0.008**	0.010**	0.008**
	(0.005)	(0.004)	(0.005)	(0.004)
Mean dep var	10.568	10.627	10.541	10.585
Number of clusters	29	29	29	29
Observations	66851	66851	66851	66851
Panel B. Females				
RLCL at age 15	0.004	-0.001	0.003	0.001
	(0.008)	(0.009)	(0.005)	(0.008)
Mean dep var	9.928	9.995	10.087	9.924
Number of clusters	29	29	29	29
Observations	49555	49555	49555	49555

Note: Equation (1) estimates the effect of the RLCL on predicted log annual earnings. For each column, we estimate separate models for men and women using the 2018 CFPS and apply them to 2015 mini-census respondents at their 2020 ages. Model (1) predicts gender-specific income based on individuals' occupation. Model (2) predicts income based on years of schooling and its quadratic terms. Model (3) predicts income based on occupation and years of schooling. Model (4) predicts income based on occupation, educational attainment, and province-level location, which is also the optimal model selected by cross validation. All models include age and age squared. Panel A reports the results for males. Panel B reports the results for females. Standard errors in parentheses are clustered at the province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.2 Robustness checks: details

This subsection provides additional detail on the robustness checks summarized in Section 4.2.5.

Alternative estimators—Our main analysis uses the ETWFE estimator of Wooldridge (2025), which avoids comparisons with already-treated units by estimating separate ATTs for each group-cohort cell and using never-treated provinces as controls. To assess whether our results depend on this choice, we re-estimate the event-study specification using two alternative estimators. The first is a conventional TWFE event-study regression, which includes indicators for each event time relative to the RLCL and province and cohort fixed effects. The second is the estimator proposed by Callaway & Sant'Anna (2021) (CS), which estimates group-cohort ATTs by comparing each treated group to never-treated provinces relative to a base cohort, and then aggregates them by event time. In all three cases, event time -1, the cohort aged 16 in the first year of exposure, serves as the reference cohort. Figure E3 presents the results. For all outcomes and both genders, the three estimators yield similar pre-treatment estimates, which are small and insignificant, and similar post-treatment dynamics. For males, the effects on high school completion, college enrollment, and high-skill employment are nearly identical across estimators. The only notable difference arises for rural low-skill employment at event time 2, where the CS estimate is smaller in magnitude and

imprecise. Because this event time is identified only from early-adopting provinces, the estimates are noisier and more sensitive to the choice of comparison. Overall, the similarity across estimators suggests that heterogeneous treatment effects across groups and cohorts do not substantially bias the conventional TWFE estimates in our setting, and that our results are not driven by the choice of estimator.

Not-yet-treated controls—Our main specification uses never-treated provinces as the control group. Table E2 re-estimates Equation (1) using both not-yet-treated and never-treated provinces as controls. The estimates are similar to those in Table 1: exposure by age 15 raises boys' high school completion by 2.7 percentage points and college enrollment by 2.4 percentage points, and shifts employment from rural low-skill to urban high-skill occupations.

Choice of exposure age—We define exposure at age 15 because it is the institutionally relevant age for schooling decisions in China. Compulsory schooling ends at about age 15, when students take the high school entrance exam and decide whether to continue to high school. Individuals exposed to the RLCL by age 15 could therefore still adjust this decision, whereas those exposed later had largely completed it. To assess this choice, we re-estimate Equation (1) defining exposure at ages 12 through 18. Figure E6 reports the estimates for high school completion. For boys, the effects are similar for exposure ages 13 to 15 and fall sharply for exposure after age 15. This pattern is consistent with age 15 as the relevant margin: exposure at any age up to 15 leaves room to adjust the high school decision, whereas exposure after 15 does not. Defining exposure at younger ages classifies some treated cohorts as untreated and reduces the number of post-treatment cohorts observed within our sample window, which makes the estimates less precise, particularly at age 12. For girls, the estimates are small at all exposure ages.

Control for agricultural tax reform—Another concern is that other reforms implemented around the same time may also have affected our outcomes. In particular, provinces began phasing out the agricultural tax in 2004, during the rollout of the RLCL. We therefore control for the provincial agricultural tax rate in the year each cohort turned 15. The results are reported in Table E4, and our main estimates are robust to this control.

Control for pre-reform economic development—Another potential confounder is economic development: more developed provinces may have implemented the RLCL earlier and may also have followed different cohort trends in schooling. We therefore control for county-level log GDP

per capita and urbanization rate, both measured in 2002, before any province in our sample implemented the RLCL, interacted with a linear cohort trend. The results are reported in Panel B of Table E4.

Placebo on the urban sample—We conduct a placebo test using an urban sample, constructed in the same way as our main sample but including only individuals whose *hukou* is registered in cities or towns. Because urban residents hold no contracted farmland, the RLCL should not affect them directly; this test therefore checks whether other province-level policy changes shifted cohort trends in our outcomes. Table E5 reports the results. The estimates are small and statistically insignificant at the 5% level for all outcomes, providing no evidence that other province-level changes drive our results.

Hukou relocation—Our analysis sample is defined by the place of *hukou* registration in 2015. If the RLCL allowed individuals to keep their land rights after moving their *hukou* for study or work, it could also affect *hukou* relocation and thus the composition of our sample. To test this possibility, we use all individuals in the relevant cohorts and estimate the effect of the RLCL on the probability of holding an urban *hukou*. Figure E4 presents the event-study estimates by gender. Exposure to the RLCL by age 15 does not significantly affect *hukou* status, suggesting that selective *hukou* relocation does not drive our results.

Results using the NFP data—We use the NFP data to analyze household-level outcomes and heterogeneity by agricultural productivity in Section 4.3. To assess comparability with the census estimates, we replicate the main analysis of high school completion using the 2013 wave of the NFP. As shown in Figure E1, the NFP estimates follow a similar pattern to the census estimates: no pre-trends, and a rise in high school completion among cohorts exposed by age 15. The NFP estimates are larger in magnitude and less precise. This difference may partly reflect differences in sample composition: the NFP covers households that were farming before the reform and is not designed to be representative of rural-*hukou* cohorts, whereas the census covers the full rural-*hukou* population.

Table E2: Robustness checks: ETWFE with not-yet-treated as control groups

	Education		Migration	Labor Market		
	(1) High school completion	(2) College enrollment	(3) Not residing in hukou village	(4) Currently working	(5) Rural low-skill	(6) Urban high-skill
Panel A. Males						
RLCL at age 15	0.027**	0.024**	0.019**	-0.008	-0.024***	0.011**
	(0.011)	(0.009)	(0.009)	(0.008)	(0.005)	(0.004)
Mean dep var	0.377	0.170	0.533	0.873	0.340	0.125
Number of clusters	29	29	29	29	29	29
Observations	98581	98581	98515	76610	66851	66851
Panel B. Females						
RLCL at age 15	0.012	0.020*	0.003	0.000	0.012*	0.002
	(0.009)	(0.011)	(0.008)	(0.007)	(0.006)	(0.007)
Mean dep var	0.360	0.178	0.525	0.691	0.316	0.152
Number of clusters	29	29	29	29	29	29
Observations	92303	92303	92118	71704	49555	49555
Gender difference, asymptotic p-value	0.068	0.657	0.036	0.556	0.000	0.259

Note: This table re-estimates Equation (1) using both not-yet-treated and never-treated provinces as controls. Panel A reports results for males and Panel B for females. Columns (1) and (2) report the effects on high school completion and college enrollment, respectively. Column (3) reports effects on migration, defined as an indicator for not residing in the *hukou* village. Columns (4)–(6) present the effects on labor market outcomes. Currently working is defined as whether an individual was employed in 2015. Rural low-skill indicates employment in farming or construction, while urban high-skill refers to employment in high-skill occupations as defined in Section 2.2.1. Standard errors in parentheses are clustered at the province level. The sample includes individuals who turned age 15 between 2000 and 2010 and whose *hukou* is registered in a rural village. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E3: Effects of the RLCL on education and occupations, labor-market sample

	Education		Occupation	
	(1) High school completion	(2) College enrollment	(3) Rural low-skill	(4) Urban high-skill
Panel A. Males				
RLCL at age 15	0.027**	0.019***	-0.023***	0.013***
	(0.013)	(0.006)	(0.008)	(0.005)
Mean dep var	0.350	0.129	0.340	0.125
Number of clusters	29	29	29	29
Observations	66851	66851	66851	66851
Panel B. Females				
RLCL at age 15	0.004	0.007	0.006	-0.002
	(0.009)	(0.008)	(0.009)	(0.008)
Mean dep var	0.337	0.146	0.316	0.152
Number of clusters	29	29	29	29
Observations	49555	49555	49555	49555
Gender difference, asymptotic p-value	0.040	0.126	0.024	0.075

Note: This table reports the effects of the RLCL on education and occupational outcomes, estimated from Equation (1), for the sample used in the labor-market analysis: employed individuals who turned 15 between 2000 and 2007 and whose *hukou* is registered in a rural village. Columns (1) and (2) report effects on high school completion and college enrollment. Rural low-skill indicates employment in farming or construction, while urban high-skill refers to employment in high-skill occupations as defined in Section 2.2.1. Standard errors in parentheses are clustered at the province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E4: Effects of the RLCL on education and labor market outcomes, controlling for confounding factors

	Education		Migration	Labor Market		
	(1) High school completion	(2) College enrollment	(3) Not residing in hukou village	(4) Currently working	(5) Rural low-skill	(6) Urban high-skill
Panel A. Controlling for agricultural tax reform, males						
RLCL at age 15	0.035***	0.030***	0.019**	-0.002	-0.022***	0.014***
	(0.009)	(0.010)	(0.009)	(0.010)	(0.007)	(0.005)
Mean dep var	0.377	0.171	0.532	0.873	0.339	0.125
Number of clusters	29	29	29	29	29	29
Observations	97810	97810	97745	75839	66223	66223
Panel B. Controlling for agricultural tax reform, females						
RLCL at age 15	0.006	0.014	-0.004	-0.017**	0.007	0.001
	(0.009)	(0.009)	(0.008)	(0.008)	(0.010)	(0.007)
Mean dep var	0.361	0.179	0.524	0.691	0.317	0.152
Number of clusters	29	29	29	29	29	29
Observations	91619	91619	91438	71020	49108	49108
Gender difference, asymptotic p-value	0.000	0.044	0.005	0.090	0.037	0.128
Panel C. Controlling for economic development, males						
RLCL at age 15	0.035***	0.037***	0.028***	0.001	-0.027***	0.014**
	(0.008)	(0.009)	(0.009)	(0.008)	(0.010)	(0.006)
Mean dep var	0.372	0.167	0.536	0.873	0.343	0.122
Number of clusters	28	28	28	28	28	28
Observations	91378	91378	91324	70997	61948	61948
Panel D. Controlling for economic development, females						
RLCL at age 15	0.006	0.011	-0.003	-0.025***	0.005	-0.006
	(0.009)	(0.007)	(0.007)	(0.007)	(0.010)	(0.009)
Mean dep var	0.354	0.174	0.526	0.690	0.321	0.148
Number of clusters	28	28	28	28	28	28
Observations	85240	85240	85073	66192	45657	45657
Gender difference, asymptotic p-value	0.000	0.000	0.005	0.036	0.016	0.019

Note: This table reports the effects of the RLCL on education and labor market outcomes, estimated from Equation (1), controlling for the agricultural tax rate in Panels A and B and for pre-reform county economic development trends in Panels C and D. The sample includes individuals who turned age 15 between 2000 and 2010 and whose *hukou* is registered in a village. Columns (1) and (2) report the effects on high school completion and college enrollment, respectively. Column (3) reports effects on migration, defined as an indicator for not residing in the *hukou* village. Columns (4)-(6) present the effects on labor market outcomes. Currently working is defined as whether an individual was employed in 2015. Rural low-skill indicates employment in farming or construction, while urban high-skill refers to employment in high-skill occupations as defined in Section 2.2.1. Standard errors in parentheses are clustered at the province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E5: Effects of the RLCL on outcomes for the urban sample

	Education		Migration	Labor Market		
	(1) High school completion	(2) College enrollment	(3) Not residing in hukou community	(4) Currently working	(5) Rural low-skill	(6) Urban high-skill
Panel A. Males						
RLCL at age 15	0.019	0.018	-0.003	-0.013	0.015*	0.003
	(0.021)	(0.018)	(0.012)	(0.009)	(0.008)	(0.010)
Mean dep var	0.710	0.479	0.481	0.793	0.116	0.351
Number of clusters	29	29	29	29	29	29
Observations	45052	45052	44939	34730	27543	27543
Panel B. Females						
RLCL at age 15	0.021*	0.021*	0.010	-0.003	-0.002	0.012
	(0.011)	(0.012)	(0.010)	(0.014)	(0.007)	(0.014)
Mean dep var	0.707	0.502	0.495	0.655	0.073	0.444
Number of clusters	29	29	29	29	29	29
Observations	46316	46316	46178	36505	23910	23910
Gender difference p-value	0.810	0.731	0.403	0.623	0.090	0.595

Notes: This table reports the placebo test—the effects of the RLCL on the main outcomes for the urban sample. The sample includes individuals who turned age 15 between 2000 and 2010 and who are registered in urban areas. Columns (1) and (2) report the effects on high school completion and college enrollment, respectively. Column (3) reports effects on migration, defined as an indicator for not residing in the *hukou* village. Columns (4)-(6) present the effects on labor market outcomes. Currently working is defined as whether an individual was employed in 2015. Rural low-skill indicates employment in farming or construction, while urban high-skill refers to employment in high-skill occupations as defined in Section 2.2.1. Panel A reports the results for males. Panel B reports the results for females. Standard errors in parentheses clustered at the province level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

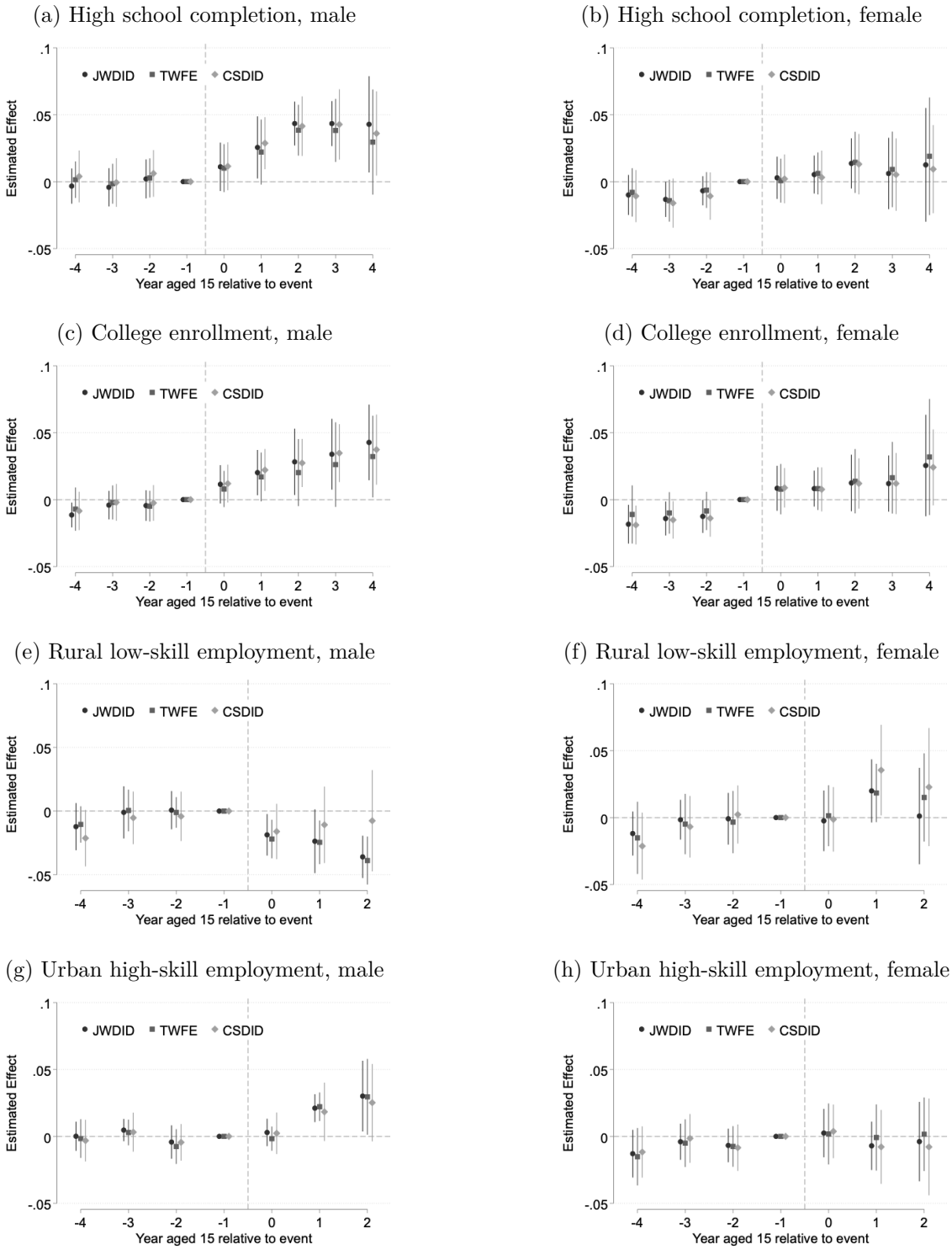


Figure E3: Effects of the RLCL on the younger generation

Note: Event time is the year a cohort turned 15 relative to the year its province implemented the RLCL. The reference cohort (event time −1) was 16 at implementation. Panels (a)–(d) use individuals who turned 15 between 2000 and 2010, while panels (e)–(h) restrict the sample to those who turned 15 by 2007. All individuals hold a *hukou* registered in a rural village. Rural low-skill and urban high-skill employment are measured conditional on employment. JWDID, TWFE, and CSDID denote the extended TWFE estimator (Wooldridge, 2025), the conventional TWFE estimator, and the estimator of Callaway & Sant'Anna (2021), respectively. Error bars show 95% confidence intervals based on standard errors clustered by province.

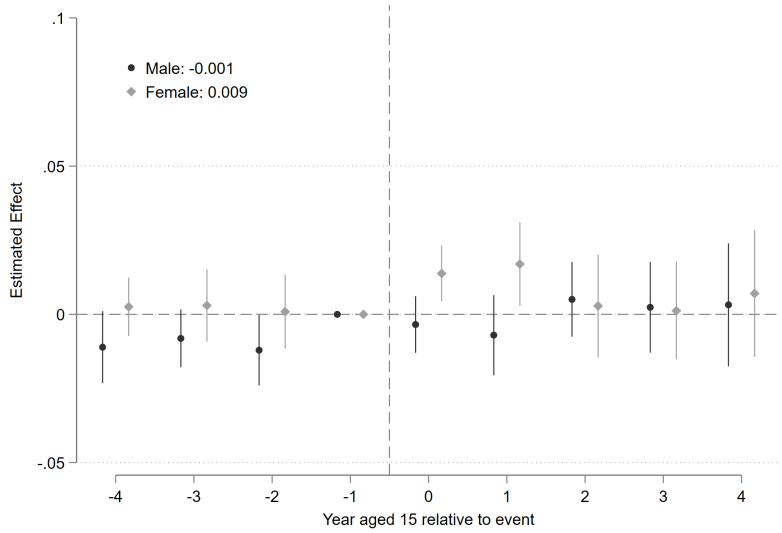


Figure E4: Effects of the RLCL on holding an urban *hukou*

Note: This figure reports estimates for the effects of RLCL on urban *hukou* status in 2015 using equation (1). The outcome equals one for urban *hukou* and zero for village *hukou*. The sample includes all individuals reaching age 15 from 2000 to 2010 for whom *hukou* type is observed, including both village and non-village *hukou* types. Standard errors are clustered by province.

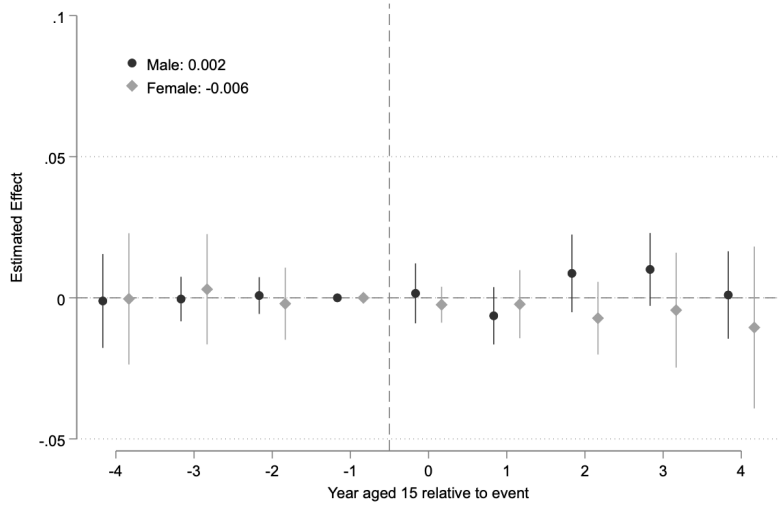
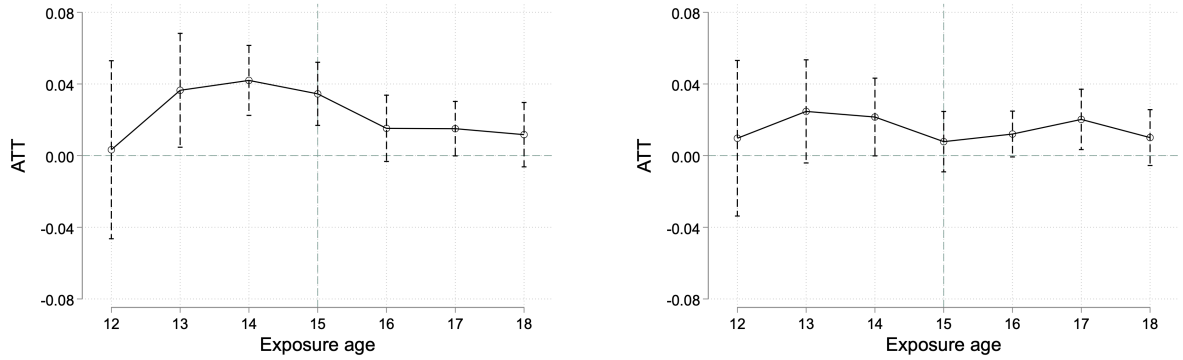


Figure E5: Effects of the RLCL on completing compulsory education

Note: This figure reports estimates of the effects of the RLCL on completing compulsory education by 2015 using equation (1). The outcome indicates whether an individual had completed compulsory education by 2015. The sample includes individuals who reached age 15 between 2000 and 2010. Standard errors are clustered by province.

E.3 Heterogeneity



(a) Male sample (b) Female sample

Figure E6: The Effects of RLCL on High-School Completion at Different Exposure Ages

Notes: This figure reports birth-cohort-specific treatment effects of the RLCL reform on high-school completion at different exposure ages. Panel (a) shows estimates for boys and panel (b) for girls. Each point represents the estimated average treatment effect, and the dashed vertical bars show the corresponding 95% confidence intervals. The vertical dashed line marks age 15, while the horizontal dashed line indicates zero effect. The estimates are obtained using Equation 1 at the corresponding age cutoffs. Standard errors are clustered at the province level.

Table E6: Joint education and occupational switching

	(1) Below HS × Rural employment	(2) Below HS × Urban employment	(3) HS completion × Rural employment	(4) HS completion × Urban employment
RLCL at age 15	-0.022**	-0.005	-0.001	0.028***
	(0.011)	(0.011)	(0.006)	(0.011)
Mean dep. var.	0.274	0.376	0.066	0.284
Observations	66,851	66,851	66,851	66,851

Notes: Each column reports the effect of RLCL exposure at age 15 on the probability of belonging to a joint education–occupation category: (1) no high school and rural low-skill occupation; (2) no high school and an urban occupation (either high-skill or low-skill); (3)high school and rural low-skill occupation; (4) high school and an urban occupation. Standard errors clustered at the province level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

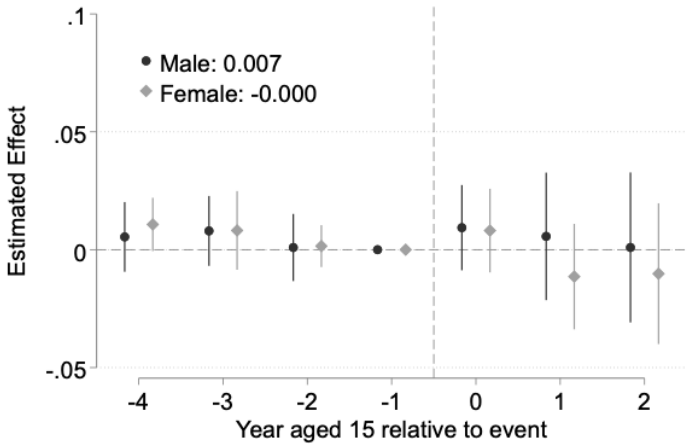
Table E7: Household agricultural productivity and non-agricultural labor allocation in 2003

	(1) Any non-agri activity	(2) Non-agri labor days	(3) Relative non-agri days
Log agricultural productivity	-0.023**	-15.667***	-0.253***
	(0.010)	(4.186)	(0.071)
Log household labor	0.030**	26.758***	0.046
	(0.014)	(9.678)	(0.091)
Male labor share	0.120***	58.186***	0.721***
	(0.022)	(10.562)	(0.145)
High school share	-0.040	-19.564*	-0.125
	(0.029)	(11.102)	(0.189)
Primary school share	0.035**	-4.184	0.158
	(0.018)	(6.624)	(0.120)
Secondary school share	0.054***	0.330	0.386***
	(0.019)	(7.081)	(0.125)
Vocationally trained share	-0.002	5.152	0.076
	(0.022)	(9.289)	(0.146)
Land size	-0.001***	-0.473**	-0.010***
	(0.000)	(0.211)	(0.003)
Household size	0.047***	39.034***	0.280***
	(0.005)	(6.112)	(0.028)
Observations	14463	14463	14463
Mean dependent variable	0.492	143.460	-3.206
Village FE	Yes	Yes	Yes

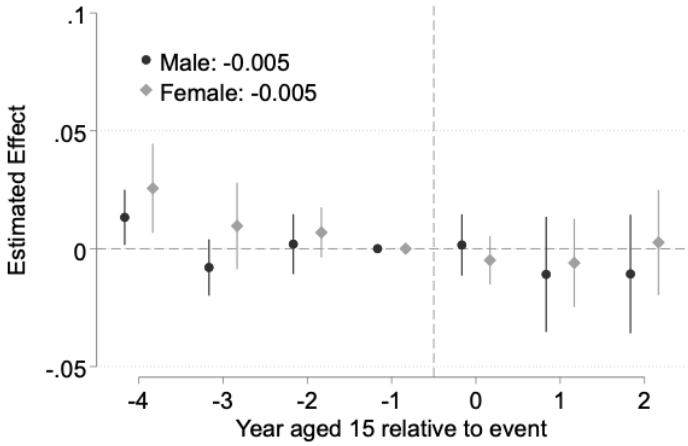
Notes: This table reports OLS estimates for households with positive agricultural labor days in 2003. The dependent variable in column (1) is an indicator equal to one if the household reports any non-agricultural labor activity. The dependent variable in column (2) is the number of non-agricultural labor days. The dependent variable in column (3) is relative non-agricultural labor supply, defined as asinh(non-agricultural labor days) − asinh(agricultural labor days). Higher values of this measure indicate a relatively greater allocation of labor to non-agricultural activities. Agricultural productivity is the location-residualized log of marginal productivity of land. All specifications include location fixed effects and control for log household labor, male labor share, high-school share, primary-school share, secondary-school share, vocationally trained share, land size, and household size. Robust standard errors clustered at the village level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure E7: Effects of the RLCL on other occupations

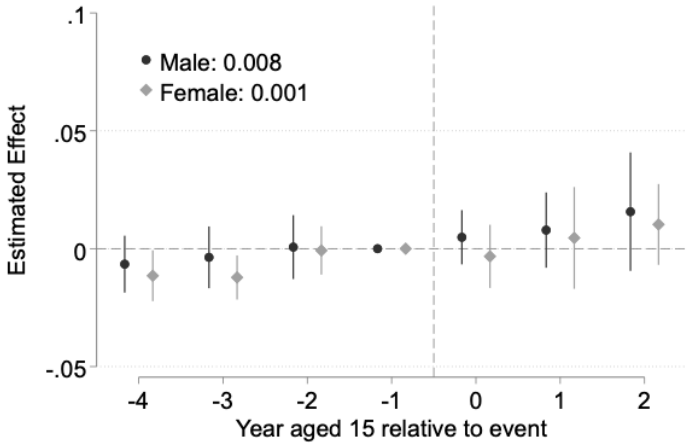
(a) Low-skill manufacturing



(b) Low-skill service



(c) Other urban low skill



Notes: This figure shows the effects of the RLCL on occupational outcomes, estimated using equation (1). Panel (a) reports results for low-skill manufacturing occupations; Panel (b) for low-skill service occupations; Panel (c) for other urban low-skill occupations. The sample consists of individuals who turned 15 between 2000 and 2007 and whose *hukou* is registered in a village. Standard errors are clustered at the province level. Occupation classification is described in Section 2.2.1.

Table E8: Effects by heterogeneity along other dimensions

	By Income			By Savings			By Education		
	(1) HH Inc	(2) HS Boys	(3) HS Girls	(4) HH Inc	(5) HS Boys	(6) HS Girls	(7) HH Inc	(8) HS Boys	(9) HS Girls
Panel A: Low-income/savings/education households									
RLCL (at age 15)	0.037	0.124*	0.061	0.052**	0.133**	0.077	0.047**	0.060	0.069
	(0.027)	(0.066)	(0.081)	(0.025)	(0.056)	(0.076)	(0.023)	(0.074)	(0.087)
Dep. Var. Mean	9.648	0.401	0.421	9.822	0.437	0.429	9.833	0.397	0.343
Clusters	28	28	28	28	28	28	28	28	27
Observations	94280	1732	1446	104078	1983	1775	88708	1337	1215
Panel B: High-income/savings/education households									
RLCL (at age 15)	0.051*	0.203***	0.047	0.093***	0.108	0.025	0.047	0.157***	0.055
	(0.027)	(0.033)	(0.093)	(0.020)	(0.085)	(0.096)	(0.029)	(0.058)	(0.063)
Dep. Var. Mean	10.126	0.465	0.393	10.033	0.454	0.386	9.987	0.482	0.473
Clusters	28	28	28	28	28	28	28	28	28
Observations	98577	1953	1856	52529	928	866	88566	2087	1838
Diff p-value	0.326	0.307	0.721	0.008	0.920	0.610	0.886	0.370	0.978

Notes: This table presents the effects of the land reform on the outcomes, conditional on pre-reform income, savings, and the educational level of the head of the household. Columns (1)–(3) report the effect of the land reform on household income, estimated using equation (D2), with standard errors clustered at the province level, and effects on high school for boys and girls, respectively, estimated using equation (1) conditional on pre-reform household income, with standard errors clustered at the province level. Columns (4) - (6) report the results for savings, while columns (7) - (9) report the results for parental education. Panel A presents results for households with low levels, while Panel B shows results for households with high levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

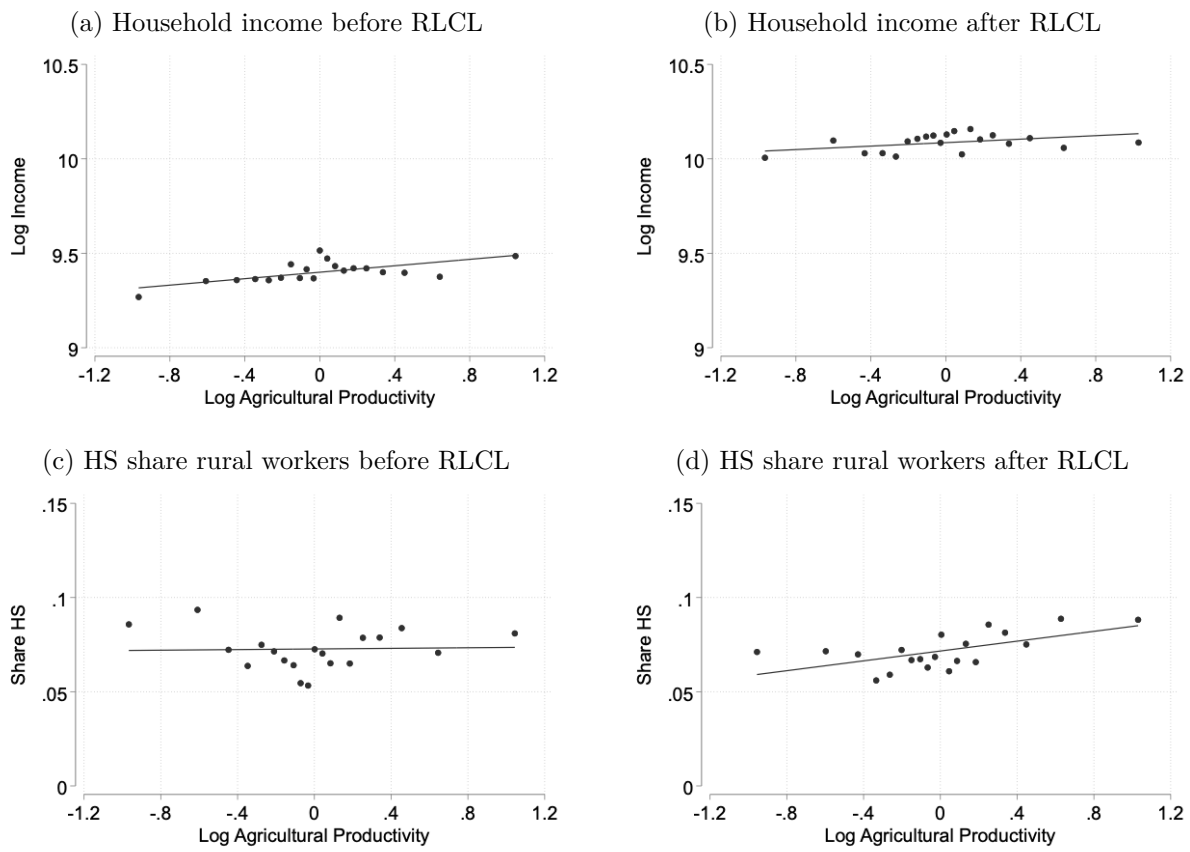


Figure E8: Agricultural productivity and income

Note: This figure presents the relationship between agricultural productivity and household income and education before and after RLCL. We focus on households in provinces that implemented RLCL between 2003 and 2006 and use data from the years 2000–2002 as years before RLCL, and data from the years 2007–2009 as years after RLCL. The y-axis shows average log household income (Panels a and b) and the share of the household's rural laborers who completed high school (Panels c and d). The x-axis is the deviation in log agricultural productivity relative to the village mean. The solid line is the estimated linear relationship between log agricultural productivity and the outcome variable. Data are based on the NFP.

Appendix F Marginal Product of Land (MPL)

F.1 Construction

We use NFP data to construct a household-level measure of the marginal product of land (MPL) before the reform. For household h in year t,

$$MPL_{ht} = \frac{1}{|C_{ht}|} \sum_{c \in C_{ht}} \alpha_c \frac{Y_{cht}}{L_{cht}}, \tag{F1}$$

where C_{ht} is the set of crops household h grows in year t, Y_{cht} and L_{cht} are the output and sown area of crop c, and α_c is the output elasticity of land for crop c. Under Cobb–Douglas technology, $\alpha_c Y_{cht}/L_{cht}$ is the marginal product of land in crop c, and we take the simple mean across the crops a household grows. We use the 2003 value to rank households by pre-reform agricultural productivity.

To estimate α_c, we use household–crop–year data from 1986 to 2003 (1994 is missing). We assume a Cobb–Douglas production function in land, labor, machinery, and other inputs, and estimate it jointly across crops, allowing all elasticities to differ by crop (Chari et al., 2021):

$$y_{hcvt} = \alpha_c \log L_{hcvt} + \beta_c \log N_{hcvt} + \gamma_c \log K_{hcvt} + \sigma_c \log O_{hcvt} + \phi_h + \phi_c + \phi_t + \varepsilon_{hcvt}, \tag{F2}$$

where y_{hcvt} is log output of household h growing crop c in village v in year t, and L, N, K, and O are sown area, household labor days, machinery costs, and other input costs such as seeds, fertilizer, and pesticides. Costs are deflated to 2002 prices using provincial CPI and enter as $\log(1+x)$. ϕ_h, ϕ_c, and ϕ_t are household, crop, and year fixed effects.[41] Table F1 reports the estimates. The land elasticity averages 0.53 across crops, slightly above S. Zhu et al. (2016) and Gong (2018) and close to Liu et al. (2023).

The land elasticity is similar under alternative specifications estimated on NFP data from 2000 to 2010, which cover more households and crops. With household–year, household–crop, and village–crop–year fixed effects, the average land elasticity is 0.51. Restricting to household–crop pairs observed in all 11 years gives 0.50, and a translog specification gives 0.54 at the sample mean. Estimating each crop by system GMM with lagged inputs as instruments gives a higher average of 0.60.

[41] The limited number of pre-reform observations prevents a richer fixed-effects specification.

Table F1: Production function estimates by crop, 1986–2003

	Sowing Area	Labor Days	Machinery Cost	Other Cost
Wheat	0.536***	0.154***	0.083***	0.257***
	(0.021)	(0.014)	(0.007)	(0.019)
Rice	0.554***	0.144***	0.020***	0.238***
	(0.023)	(0.014)	(0.004)	(0.020)
Corn	0.630***	0.179***	0.021***	0.278***
	(0.017)	(0.013)	(0.004)	(0.015)
Soybean	0.549***	0.276***	0.077***	0.145***
	(0.016)	(0.016)	(0.008)	(0.013)
Potato	0.546***	0.275***	0.021**	0.077***
	(0.022)	(0.019)	(0.010)	(0.012)
Other Grains	0.495***	0.211***	0.078***	0.167***
	(0.031)	(0.029)	(0.014)	(0.031)
Cotton	0.672***	-0.080**	0.021**	0.352***
	(0.055)	(0.038)	(0.010)	(0.036)
Oilseed	0.531***	0.276***	0.026***	0.146***
	(0.015)	(0.014)	(0.006)	(0.011)
Sugar	0.336***	0.529***	0.076***	0.235***
	(0.084)	(0.101)	(0.027)	(0.045)
Hemp	0.887***	-0.036	-0.039	0.119
	(0.121)	(0.202)	(0.044)	(0.076)
Tobacco Leaf	0.976***	-0.767***	0.095***	0.344***
	(0.093)	(0.085)	(0.022)	(0.074)
Vegetables	0.374***	0.299***	0.083***	0.111***
	(0.022)	(0.015)	(0.007)	(0.007)
Other Cash Crops	0.942***	-0.158***	-0.149***	0.164***
	(0.054)	(0.058)	(0.026)	(0.030)
Fruit	0.240***	0.405***	0.069***	0.188***
	(0.031)	(0.030)	(0.013)	(0.016)
Other Orchard	0.733***	-0.265***	-0.178***	0.351***
	(0.096)	(0.093)	(0.043)	(0.039)
Other Farm	0.235***	0.096**	0.209***	-0.016
	(0.039)	(0.044)	(0.021)	(0.026)
Total	0.527***	0.199***	0.045***	0.190***
	(0.009)	(0.007)	(0.003)	(0.006)
Total Observations	53339			

Notes: This table reports estimates of equation (F2) using NFP data from 1986 to 2003, excluding 1994. All elasticities vary by crop. Household, crop, and year fixed effects are included. The last row reports the average elasticity across crops. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F.2 Robustness of the MPL measure

Two features of our baseline measure could affect the ranking of households. First, under Cobb–Douglas technology the MPL equals $\alpha_c Y/L$, so it is mechanically lower for households farming more land. Because farm size is directly related to the land at stake under insecure tenure, this could confound the comparison. Second, output is measured in physical units, which differ across crops, so averaging across crops may partly reflect crop composition rather than productivity. In practice, these characteristics play a limited role: crop shares, log sown area, and the number of crops together explain about 5% of the within-village variation in log MPL.

Table F2 addresses both concerns directly. Columns (1)–(3) split households using the

residual from a regression of log MPL on log sown area in 2003 and village fixed effects, so that households are compared with others of similar farm size in the same village. The estimates are nearly identical to Table 2: exposure to the RLCL raises high school completion by 24.6 percentage points among boys from low-MPL households and by 8.2 percentage points among boys from high-MPL households (p-value = 0.026). Columns (4)–(6) follow Chari et al. (2021) and compare each household only with households growing the same crop in the same village. For each crop, we compute the household's percentile in the village–crop distribution of MPL and average across crops using sown area as weights. Because comparisons within a village–crop cell rely on few observations per household in a given year and are sensitive to plot-specific transitory shocks, we average this measure over the pre-reform years 2000–2003. The schooling response is again about three times larger among low-MPL households (23.7 vs. 7.5 percentage points; p-value = 0.069). In both specifications, household income rises similarly in the two groups, and the effects for girls are small and do not differ significantly between groups.

Table F2: Heterogeneous impacts by household productivity: alternative MPL measures

	Controlling for land size			Within village–crop, 2000–2003		
	(1) HH Inc	(2) HS Boys	(3) HS Girls	(4) HH Inc	(5) HS Boys	(6) HS Girls
Panel A: Low-MPL households						
RLCL	0.045**	0.246***	0.067	0.037	0.237***	0.083
	(0.022)	(0.055)	(0.071)	(0.026)	(0.073)	(0.074)
Dep. Var. Mean	9.420	0.433	0.402	9.418	0.426	0.389
Clusters	28	28	28	28	28	28
Observations	99126	1852	1716	96351	1843	1683
Panel B: High-MPL households						
RLCL	0.040	0.082*	0.057	0.048**	0.075	0.003
	(0.031)	(0.047)	(0.084)	(0.022)	(0.066)	(0.083)
Dep. Var. Mean	9.487	0.437	0.409	9.487	0.444	0.422
Clusters	28	28	28	28	28	28
Observations	93578	1829	1588	96630	1845	1624
Diff p-value	0.880	0.026	0.732	0.475	0.069	0.234

Notes: This table repeats Table 2 using alternative measures of pre-reform household productivity. In Columns (1)–(3), households are split at zero of the residual from a regression of log MPL in 2003 on log sown area in 2003 and village fixed effects. In Columns (4)–(6), productivity is the household's percentile in the village–crop distribution of MPL, weighted by sown area and averaged over 2000–2003, residualized on village fixed effects (Chari et al., 2021). Household income is estimated using equation (D2), and high school completion for boys and girls using equation (1). Standard errors in parentheses are clustered at the province level. The p-value tests the equality of the effects across groups in a pooled specification. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure F1 repeats the descriptive comparison in Figure 4 using the within village–crop measure. The pattern for boys is similar. Among older cohorts, high school completion does not decline with agricultural productivity, whereas among younger cohorts exposed to the RLCL by age 15, boys from lower-productivity households are more likely to complete high school. For

girls, high school completion is, if anything, increasing in productivity in both cohorts.

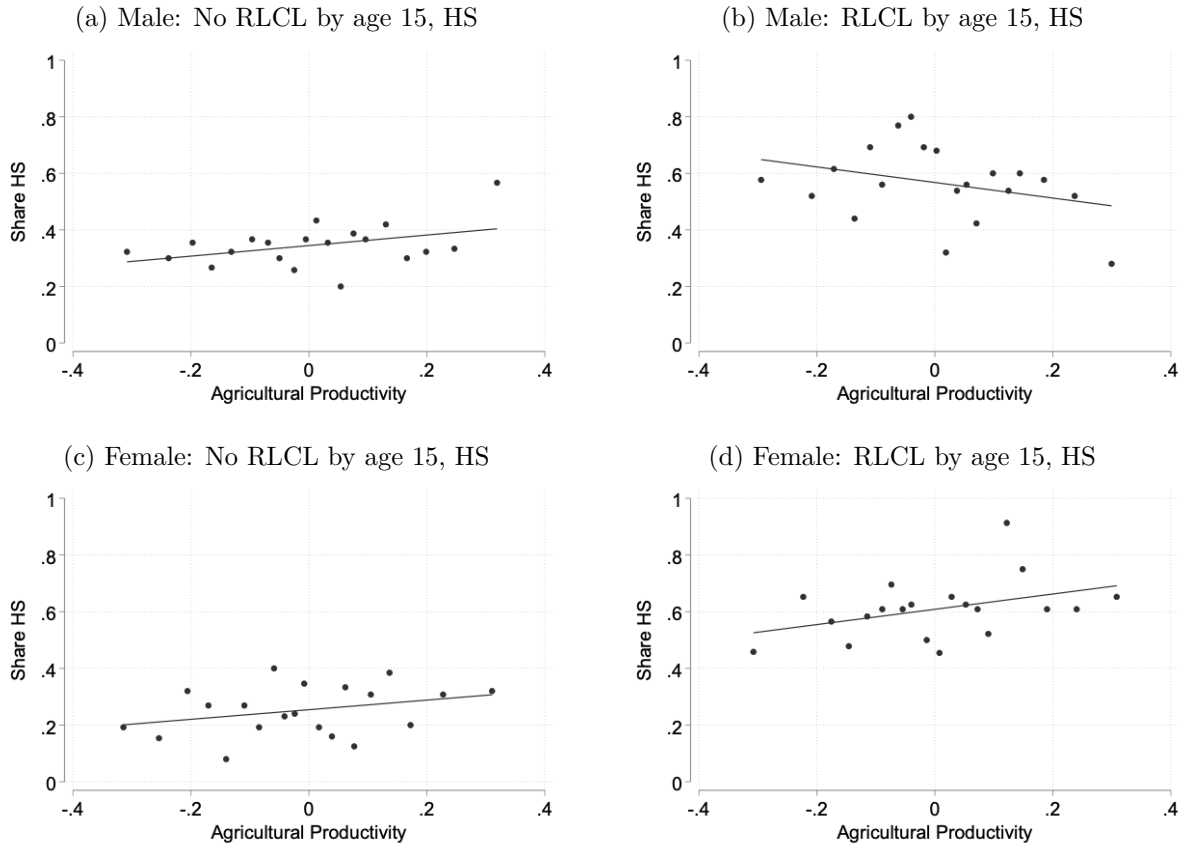


Figure F1: Agricultural productivity and education for the younger and older cohorts: within village–crop measure

Note: This figure repeats Figure 4 using an alternative measure of agricultural productivity: the household's percentile in the village–crop distribution of the marginal product of land, weighted by sown area, averaged over 2000–2003, and expressed relative to the village mean. The sample and cohorts are as in Figure 4. Panels (a) and (c) show individuals born in 1985–1987, who were at least 16 when the RLCL took effect; Panels (b) and (d) show individuals born in 1992–1994, who turned 15 after the RLCL. The solid line is the estimated linear relationship. Data are based on the NFP.

Appendix G Quantitative Analysis

G.1 Parameters and the common land market

Table G1 reports the benchmark parameters. We assume that log abilities $(\log z_F, \log z_M)$ are jointly normal with zero means, standard deviations (σ_F, σ_M), and correlation ρ. The land-loss parameter satisfies $\log \phi_i \sim N(\mu_\phi, \sigma_\phi^2)$, and the mobility friction satisfies $\log[\kappa_i/(1-\kappa_i)] \sim N(\mu_\kappa, \sigma_\kappa^2)$. We jointly calibrate six parameters and hold the other nine fixed using normalizations, external estimates, and benchmark assumptions.

After the reform, the older group (O) retains its pre-reform schooling while the younger group (Y) can choose schooling. Both groups have secure land rights. The common post-reform rent q^* solves

$$(1-\lambda)D_O(q^*) + \lambda D_Y(q^*) = 0, \tag{G1}$$

where D_O and D_Y denote the net land demands of the two groups. The pre-reform economy clears separately at q_B. We re-solve both markets whenever parameters change. The two post-reform groups have the same distribution of abilities and frictions.

The parameter λ is the share of the population that can still adjust schooling when the reform takes effect. Empirically, it corresponds to the share of men aged 15 or younger among rural men aged 45 or younger in the reform year; we exclude older men, who are less likely to participate in the same land and labor markets. In the NFP, this share is 25.1% in each province's reform year, so we set $\lambda = 0.251$. The corresponding share in the 2005 Census is 35.6%. Table G4 reports results for $\lambda = 0.15$ and $\lambda = 0.35$; the results are nearly unchanged.

Table G1: Benchmark parameters and equilibrium rents

Parameter	Value	Description and source
Panel A. Jointly calibrated parameters		
τ	0.2909	Return to schooling
c	0.0540	Schooling cost
w_M	0.8592	Non-agricultural wage shifter
μ_ϕ	0.6147	Mean log land-loss valuation
ρ	0.6510	Cross-sector log-ability correlation
σ_M	0.8984	Standard deviation of log non-agricultural ability
Panel B. Parameters held fixed during calibration		
p_a	1.0000	Agricultural output price; normalization
$\bar{\ell}$	1.0000	Equal land endowment per agent; normalization
θ	0.5330	Land elasticity; close to Table F1 (0.527)
A_a	1.0000	Agricultural productivity shifter; income-scale normalization
σ_F	0.5770	Standard deviation of log farm ability; Adamopoulos et al. (2024, Table B.5)
σ_ϕ	0.4070	Standard deviation of log land-loss valuation; Adamopoulos et al. (2024, Table B.5)
η	0.0510	Pre-reform land-loss probability; survey in Adamopoulos et al. (2024)
μ_κ	0.3740	Location of logistic-normal mobility friction; Adamopoulos et al. (2024, Table B.5)
σ_κ	0.6270	Dispersion of logistic-normal mobility friction; Adamopoulos et al. (2024, Table B.5)
Panel C. Demographic weight and endogenous rents		
λ	0.2510	Share of men aged 15 or younger among men aged 45 or younger in the reform year; NFP
q_B	0.6904	Pre-reform equilibrium land rent
q^*	0.6718	Common post-reform equilibrium land rent

Notes: Table B.5 refers to the online appendix of Adamopoulos et al. (2024). $A_a = 1$ fixes income units (see also their Appendix B). We use their household agricultural-ability dispersion for σ_F, as our model does not distinguish individuals from households. Land-loss risk is eliminated after the reform ($\eta = 0$). Because choices depend on $\eta\phi_i$, η and μ_ϕ are not separately identified; a different η is offset one for one by μ_ϕ and leaves all results unchanged. The demographic weight is separate from the 15 structural parameters; rents are equilibrium outcomes.

G.2 Calibration targets and local identification

We calibrate the model to the six targets reported in Panel A of Table 3. The schooling share comes from the 2015 Census for rural men who reached high school entry age before their province implemented the reform. The employment share uses the 2005 Census for rural men who turned 15 in 1998–2000, excluding low-skill employment such as farming and construction. The NFP existing-worker target is a 2.6-percentage-point response; its model counterpart compares the older group's post-reform employment with the pre-reform equilibrium. The two cohort responses compare younger and older groups at the same rent q^*.[42] For the NFP schooling-response ratio between low- and high-productivity farmers, $0.242/0.082 = 2.951$, the

[42]These are differences between two groups, so we do not multiply them by λ.

model splits farmers at the median of z_F.

We choose the six parameters jointly to minimize the squared target deviations, scaled by numerical fitting tolerances. We integrate over non-agricultural ability analytically and use scrambled Sobol nodes for the remaining heterogeneity. At 2^{20} nodes, the largest share-target error is 0.0026 percentage points. Table G2 shows how the model moments respond to changes in the calibrated parameters.

Table G2: Parameter–moment Jacobian

	τ	c	w_M	μ_ϕ	ρ	σ_M
HS completion share	0.633	−2.818	0.457	−0.033	−0.132	−0.015
Non-agricultural employment share	0.195	−0.521	0.398	−0.048	−0.048	−0.057
Existing-worker employment response	−0.095	0.343	−0.006	0.038	0.029	0.001
Cohort completion response	0.070	−0.396	0.017	0.035	0.030	−0.020
Cohort employment response	0.099	−0.378	0.011	0.012	0.021	−0.014
Low/high schooling-response ratio	6.156	−42.752	2.874	−0.091	−2.751	−5.873

Notes: Entries are central differences with respect to parameter levels; share moments are fractions. Each perturbation holds λ and other parameters fixed and re-solves both land markets. Calculations use 2^{18} nodes and a 0.5% relative step. Numerical rank remains six with an independent seed and a halved step, supporting local identification conditional on the fixed parameters, without establishing global uniqueness or statistical precision.

G.3 Counterfactuals, income, and decomposition

Counterfactuals. Table G3 reports allocations and income in five counterfactuals. Column (1) is the pre-reform equilibrium. Columns (2) and (3) eliminate land-loss risk and evaluate all agents at the calibrated post-reform rent q^*, holding schooling at its pre-reform level in Column (2) and allowing it to adjust in Column (3). Column (4) weights Columns (2) and (3) by $1-\lambda$ and λ; this is the economy that clears the land market in the reform year. Column (5) is the longer-run economy: we hold the calibrated parameters fixed, set $\lambda = 1$, and re-solve schooling, occupations, and the land rent.

Income. Non-agricultural earnings net of mobility costs are $(1-\kappa_i)w_M z_{M_i}(1+\tau h_i)$. For farmers, non-land income is $p_a A_a z_{F_i} L_i^\theta - qL_i$, agricultural revenue less the opportunity cost of all cultivated land. Total income adds the rent on owned land, $q\bar{\ell}$. We deduct neither education costs nor perceived land-loss costs. Panel B of Table 3 compares the difference in mean log non-land income between Columns (3) and (2) of Table G3, 0.0065, with the cohort effect on imputed earnings, 0.008.[43]

[43]Because the empirical earnings index and model income cover different sources of income, we use this comparison to assess the size of the model's response.

Table G3: Outcomes under alternative counterfactuals

	(1) Pre-reform	(2) Secure land, rent q^*	(3) Secure land, rent q^*	(4) Reform-year economy	(5) Long run
		Schooling fixed	Schooling flexible		
Panel A. Allocation					
High school share (%)	38.60	38.60	42.00	39.46	41.79
Non-agricultural share (%)	46.90	49.50	51.80	50.08	51.52
Land rent	0.690	0.672	0.672	0.672	0.667
Land cultivated per farmer	1.883	1.991	2.040	2.003	2.063
Panel B. Income					
Mean income	1.6725	1.6753	1.6783	1.6761	1.6781
Change from pre-reform (%)	–	0.17	0.35	0.22	0.34

Notes: Column (1) is the pre-reform equilibrium ($\eta = 0.051$, rent q_B). Columns (2)–(5) have no land-loss risk. Columns (2) and (3) evaluate all agents at q^* with schooling fixed at its pre-reform level and flexible, respectively. Column (4) weights Columns (2) and (3) by $1 - \lambda$ and λ. Column (5) allows all agents to adjust schooling and re-solves the land rent. Income is labor income, net of the mobility cost κ_i for non-agricultural workers, plus rental income on owned land; education costs and perceived land-loss costs are not deducted. Land endowment per agent is normalized to one. The mean-log difference between Columns (3) and (2) is the model counterpart of Panel B of Table 3.

Decomposition of individual income gains. Figure 5 decomposes the income gain of an individual who can adjust schooling, averaged over all types, with every individual facing the post-reform rent q^*. Let Y_B be pre-reform mean income. Y_L reoptimizes land holdings at q^* while keeping pre-reform schooling and occupations; Y_O also lets occupations adjust with schooling fixed; Y_H assigns post-reform schooling while keeping the occupations in Y_O; and Y_Y lets both schooling and occupations adjust. Then

$$Y_Y - Y_B = (Y_L - Y_B) + (Y_O - Y_L) + (Y_H - Y_O) + (Y_Y - Y_H),$$

where the four terms capture land reallocation, occupational reallocation with schooling fixed, direct earnings from schooling, and schooling-enabled occupational reallocation. The parameter λ determines q^* through land-market clearing but is not used as a weight.[44]

Longer-run scenarios. Column (5) of Table G3 reports the longer-run economy. We also lower the non-agricultural wage by up to 3% to examine the sensitivity of these results, rather than modeling urban labor markets. The stationary extrapolation in Section 5 uses $C = 3.8$ million men per annual cohort and $K = 40$ cohorts. Additional college attendance is $KC \times 0.029$, additional high-skill employment is $KC \times 0.873 \times 0.013$, and the annual earnings gain is $KC \times 0.873 \times 40{,}000\,(e^{0.008} - 1)$.

[44] Intermediate assignments do not separately clear the land market.

G.4 Robustness

Table G4 recalibrates the model under alternative targets and fixed parameters. Education accounts for 42–59% of the individual income gain in every case, and the long-run increases in high school completion and non-agricultural employment remain close to the reduced-form benchmark. The land elasticity matters most, because it governs the gains from land reallocation itself. Results are not sensitive to the schooling-response ratio or to λ, which affects outcomes only through the post-reform rent.

Table G4: Robustness of the quantitative results

	Decomposition of income gain (%)					Long run (pp)	
	Land	Occupation (schooling fixed)	Education: Direct	Education: Enabled occ.	Income gain (%)	High school	Non-agri. employment
Benchmark	42.3	7.1	15.3	35.3	0.35	3.19	4.62
Low/high schooling-response ratio (benchmark 2.95)							
2	39.7	7.3	13.3	39.7	0.27	3.11	4.62
3	42.3	7.0	15.4	35.2	0.35	3.19	4.62
4	41.2	6.8	16.5	35.5	0.43	3.22	4.65
Land elasticity θ (benchmark 0.533)							
0.45	33.2	8.4	17.1	41.3	0.34	3.13	4.54
0.60	52.0	5.7	13.2	29.1	0.35	3.25	4.69
Flexible-schooling share λ (benchmark 0.251)							
0.15	42.0	7.1	15.5	35.4	0.35	3.16	4.58
0.35	42.6	7.1	15.1	35.2	0.35	3.22	4.65

Notes: Each row changes one target or one fixed parameter and recalibrates the six parameters in Panel A of Table G1 to the six targets. Columns (1)–(4) report the decomposition in Figure 5 as shares of the total income gain. Column (5) reports the total gain relative to pre-reform income for an individual who can adjust schooling at the post-reform rent. Columns (6) and (7) report the long-run changes when all cohorts can adjust schooling and the rent is re-solved (Column (5) of Table G3). All targets are matched closely except when the ratio is 2. In that case the model matches the ratio but fits the cohort completion response at 0.034 (target 0.034), the existing-worker employment response at 0.026 (target 0.026), and the cohort employment response at 0.023 (target 0.023) only to within about 0.5 percentage points of relative error.