Judicial Independence and Structural Transformation*

Jingyuan Guo[a]

a. Department of Economics, National University of Singapore. Email: jingyuan.guo@u.nus.edu.

July 20, 2026

Abstract

Can independent courts promote economic development? I exploit China's staggered Trans-regional Jurisdiction of Administrative Litigation (TJAL) reform, which reassigns administrative land disputes from local to external courts. Using difference-in-differences with household micro-data, I show that TJAL strengthens rural land rights. Compensation per land loss rises 16.3%, with the largest gains for off-farm households most exposed to expropriation. Households are better sorted across sectors, raising off-farm participation by 9%; remaining farmers consolidate land and increase inputs, together lifting rural income by 7.3%. But the same reallocations constrain the urban area (-7.8%) and depress urban incumbents' income (-1.4%). Sectoral composition shifts toward less land-intensive industries, with service-sector employment rising 6% and no significant change in manufacturing. A calibrated two-sector model reproduces these patterns and suggests that judicial independence can match fiscal transfers in narrowing the urban-rural gap while improving factor allocation, an efficiency gain transfers cannot deliver.

Keywords: Judicial independence; land expropriation; court reform; structural transformation

JEL: O18; K41; Q15; O17

*I am especially grateful to Siddharth George, and Joris Mueller for their invaluable guidance and suggestions. I also thank David Dorn, Ying Feng, Frederico Finan, Giacomo De Giorgi, Kenneth Khoo Chian Yian, Jingyi Liang, Haoming Liu, Yiting Ma, Andreas Madestam, Martin Mattson, Dilip Mookherjee, Pinchuan Ong, Rohini Pande, Yogita Shamdasani, Tuan Hwee Sng, Ran Song, Ao Wang, Shaoda Wang, Rita Zihe Wang, and the seminar participants at Dev PEC 2026 (Stanford) and SIG in applied economics (NUS) for their insightful feedback. The author used AI-assisted tools for editing in accordance with journal guidelines; all analysis, arguments, and conclusions are the author's own.

1 Introduction

Structural transformation, the reallocation of land and labor from agriculture toward industry and services, lies at the heart of economic development (Mitrunen, 2025; Gollin et al., 2014, 2002; Lewis, 1955). Institutions that govern and adjudicate competing claims over factors are central to enabling this reallocation. Yet how an independent court, one insulated from local government and able to check its power, shapes this process remains underexplored. I study a Chinese reform that moved land disputes to courts outside the reach of the government being sued.

How judicial independence advances or obstructs the structural transformation is not obvious *a priori*. On one hand, independent courts can secure rural property rights, reducing the threat of government expropriation and freeing rural households to reallocate labor without fear of losing their land (Hornbeck, 2010; Field, 2007; Laeven and Woodruff, 2007), thereby letting factors move across sectors. On the other, stronger enforcement of land rights may exacerbate holdup problems. By raising the cost of acquiring rural land for urban infrastructure and expansion, courts can constrain the very physical growth of cities that structural transformation typically requires (Brooks and Liscow, 2023; Holland, 2022). The net effect on structural transformation, and which margin dominates, is therefore an empirical question.

Studying how judicial independence shapes structural transformation faces two empirical hurdles. First, the relevant frictions, especially disputes over land and other immobile assets that govern the movement of labor and capital, are often latent and poorly measured (Michelson, 2018; Felstiner et al., 1980), and researchers rarely have reliable data that link land rights to subsequent employment and land rental outcomes. Second, even when outcomes are observed, judicial independence is endogenous to local political and economic conditions: governments may selectively intervene in adjudication precisely where the stakes for development are highest, making comparisons difficult to interpret causally.

China provides a setting addressing these challenges. Rapid urbanization has made government expropriation of rural land pervasive. Urban built-up area nearly tripled between 2000 and 2020, and off-farm households face expropriation risk more than seven times that of farming households. The very act of reallocating labor therefore raises land insecurity (Ruan and Wang, 2022; Sargeson, 2013). Government expropriations generate widespread urban-rural disputes, which cannot be resolved by traditional within-village resolutions,[1] channeling such disputes to courts. Local courts, captured

[1]Typical within-village institutional resolutions include land tenure, land titling, and land rental

by the corresponding-level governments that expropriate (Mehmood and Ali, 2024),[2] provide no effective remedy: farmers win only 12.9% of land-related administrative cases,[3] a quarter of the civil litigation rate, making judicial capture a direct drag on structural transformation.

The efforts of China's central government to discipline lower-level officials and curb such misbehaviors have created a quasi-experimental setting (Cao et al., 2023). The central authorities initiated the Trans-regional Jurisdiction of Administrative Litigation (TJAL) reform in 2013 and rolled it out in subsequent years across prefectures, reassigning administrative cases from local government's own courts to neighboring or higher-level courts.

The data link land rights to subsequent employment, land-rental and urban outcomes, tracing the reform from courts through rural households to cities. I date prefecture adoption from the Supreme People's Court's account of the rollout, checked against official notices and local media. Rural outcomes come from the National Fixed Point Survey, an annual panel of 21,418 households in 326 villages across all 31 provinces from 2008 to 2015, which records land holdings, expropriation, labor allocation, and income. Urban outcomes come from the Urban Household Survey, city yearbooks for urban area and employment, land-transaction records for prices, and the national business registry for firm entry. China Judgments Online supplies the case-level evidence.

I estimate the effect with a staggered difference-in-differences design, comparing households in treated and untreated prefectures before and after the reform. Adoption timing was set administratively by provincial courts (Cao et al., 2023), so identification does not require random assignment, only selection on time-invariant characteristics rather than on trends in rural outcomes. Beyond parallel trends, two further assumptions matter: no anticipation, and no spillovers across prefectures. The evidence below supports all three.

The reform reallocates labor out of agriculture and raises rural incomes, while

reforms (Liu et al., 2023; Chari et al., 2021; Castañeda Dower and Pfutze, 2020).

[2]To be more specific, local courts in China are heavily influenced by corresponding-level governments: on one hand, due to promotion and supervision pressures, local governments are incentivized to infringe on rural land rights to promote local economy; on the other, they possess the authority to make personnel and fiscal decisions, and provide administrative support for courts. Consequently, judgments have long been skewed in favor of local government, a phenomenon known as judicial capture (Zhao et al., 2024; Liu et al., 2022).

[3]The administrative case refers to a broad category of cases in which citizens, legal persons or other organizations (acting as plaintiffs) believe that the specific administrative actions of administrative departments and their staff (acting as defendants) are illegal or improper, or infringe their legitimate rights and interests. Prior to the TJAL reform, these cases were adjudicated by courts within the same administrative jurisdiction as the defendant government (effectively, the government's own court).

tightening the land constraint on cities. Off-farm work participation rises by 9%, and rural household income by 7.3%, with the income gain driven by off-farm earnings rather than farming, compensation, or transfer income. The same protections that release rural labor also restrict the land cities can acquire. Urban area falls by 7.8%, and the inflow of workers lowers urban incumbents' income by 1.4%. Stronger rural land rights account for this pattern.

As a first step, I show that TJAL strengthens rural land rights. TJAL increases compensation per unit of land lost by 16.3%. While aggregate effects on expropriation area and probability are small, off-farm households, those who exactly face government expropriation risk, experience a significant reduction in both land loss (-5.2%) and expropriation probability (-2.9%). These household-level patterns are corroborated by case-level evidence. In administrative land-dispute and related police-enforcement cases against local governments, TJAL raises the inter-regional jurisdiction rate by 38% and increases the win probability of non-government litigants by 16%. These results indicate that external adjudication constrains local governments' expropriation of rural land.

The movement of labor out of agriculture also activates village land markets. Off-farm households rent out their land. Those who stay in farming become more productive relative to those who leave. They consolidate the released land and increase agricultural inputs (e.g., materials and machinery), consistent with stronger incentives under more secure land rights.

The income gains are larger in villages closer to cities or with flatter terrain, areas with higher commercial land value and thus greater baseline expropriation risk, suggesting that expropriation risk, rather than a general improvement in labor-market opportunity, is the operative force.

However, the observed rural reallocation could instead be driven by a demand-side pull from the city. TJAL may simultaneously improve adjudication of firm-related administrative cases, raise urban productivity, push up wages, and draw rural labor into cities. Under this alternative channel, the observed reallocation would require urban wages to rise and cities to expand as greater firm activity absorbs more workers and land. The land rights mechanism predicts the opposite: falling wages from labor inflows and contracting city footprints from tighter expropriation constraints.

Urban adjustment rules out the productivity-pull alternative and reveals the trade-off. By reducing expropriation risk, TJAL frees rural labor to enter off-farm work, generating a positive labor-supply shock to urban areas; at the same time, it limits cities' ability to acquire rural land for expansion, generating a negative land-supply

shock. Comparing city outcomes in treated and untreated prefectures up to six years after the reform, I find that city employment rises by 4.6% (significant at 10%), while urban natives' wage income falls by 5.8% and total household income by 1.4%, with no change in non-wage income. These are consistent with wage pressure from labor inflows and inconsistent with the rising wages that productivity-driven demand would require. On the land margin, urban area declines by 7.8% and urban land prices increase by 6%, confirming a tighter urban land constraint rather than urban expansion. Reflecting differences in land intensity, employment gains are concentrated in services (+6%), whereas manufacturing employment and output are insignificantly 2.7% and 5.7% lower in reformed prefectures, a result of competing forces. Overall, judicial independence reallocates labor toward cities yet constrains urban land, sharpening the tradeoff inherent in stronger rural land rights.

Several checks support the identifying assumptions. Pre-treatment event-study coefficients are flat across main outcomes, and the effects remain significant even if parallel trends is violated after treatment by as much as the largest pre-treatment deviation (Rambachan and Roth, 2023). Because adoption is staggered, I re-estimate with heterogeneity-robust estimators (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Borusyak et al., 2024), which agree with the two-way fixed-effects results. Pre-reform GDP per capita, government spending, population, and industrial structure do not predict which or when prefectures adopt. Out-migration falls by 9.9%, though the absolute change is too small to move village population, and attrition is unaffected. The estimates also hold when I control for other contemporaneous judicial reforms, use alternative outcome scalings, and vary the control set.

To help interpret the empirical findings and quantify the economic forces behind the tradeoff of stronger rural land rights in structural transformation (i.e., off-farm labor inflow vs. tighter land supply for urban expansion), I construct a structural model. The model features usage-dependent expropriation risk, endogenous employment and land-leasing decisions, and rural and urban production. Calibrated to key empirical moments, the model predicts how changes in expropriation risk affect structural transformation. Quantitatively, the model implies that TJAL raises the share of rural households in off-farm work by about 2.2 percentage-point. It replicates the unintended distributional benefit of narrowing the urban-rural income gap (10.24 percentage-point in the data; 9.6 percentage-point in the simulation). Counterfactuals reveal a key advantage of institutional reform over fiscal transfers: both can narrow the urban-rural income gap, but judicial reform simultaneously improves factor allocation, an efficiency gain that transfers cannot deliver.

This paper identifies an independent judiciary as a determinant of structural transformation. While a large literature explains the reallocation of labor and land through rising agricultural productivity, trade, and relative prices (Bustos et al., 2016; McCaig and Pavcnik, 2018; Ngai and Pissarides, 2007; Duarte and Restuccia, 2010; Acemoglu and Guerrieri, 2008; Song et al., 2011), factors often fail to reach their most productive use (Restuccia and Rogerson, 2017; Hsieh and Klenow, 2009). Insurance networks that keep workers in their village (Munshi and Rosenzweig, 2016), migration institutions (Tombe and Zhu, 2019; Ngai et al., 2019), credit constraints (Buera et al., 2011), and land-market institutions that keep plots from their most productive user (Chen et al., 2023; Chari et al., 2021) all hold factors in place. This paper adds a friction of a different kind, a captured legal system rather than a missing market. The court that should secure rural property is controlled by the local government expropriating it, so the gain from moving a factor to higher-value use can be seized (Jacoby et al., 2002). An independent court removes this friction. By protecting rural land rights against state predation, judicial independence reallocates rural factors, raises rural incomes, and narrows the urban-rural gap. Judicial independence therefore shapes structural transformation alongside technology and market access.

This paper speaks to a growing economics of the judiciary, which divides along the line between enforcing private bargains and constraining the state (Acemoglu and Johnson, 2005). One strand shows that faster, more predictable courts raise investment, entrepreneurship, and industrial growth by lowering the cost of contracting between private parties (Amirapu, 2021; Chemin, 2009*b*, 2020, 2009*a*). A second shows that a judiciary's independence from political power changes how the state itself behaves (Mehmood, 2022; Mehmood and Ali, 2024; Lambais and Sigstad, 2023; Cao et al., 2023; Chemin, 2021). While it is not obvious whether courts can check the state under one-party rule (Llanos et al., 2015; Solomon, 2010), recent work finds that authoritarian regimes still sustain judicial institutions that mediate citizen-elite relations (Cao et al., 2023; Ríos-Figueroa and Aguilar, 2017; Albertus and Menaldo, 2012). This paper sits in the second strand, identifies that check, and extends it to development. The closest paper is Liu et al. (2022), who show that centralizing court finances and personnel curbs local protectionism between firms and raises cross-regional investment. I instead study the reassignment of administrative cases to external courts (Cao et al., 2023), and ask how a check on the state's expropriation of citizens drives structural transformation. This paper adds to the literature by showing that an insulated judiciary shapes not only how the state behaves but the development that follows.

Finally, this paper adds to the land-rights literature a case where the state that

should protect property is the one taking it. That literature shows how securing farmers' rights raises investment and moves land and labor to higher-value uses. Titling turns land into usable capital (Galiani and Schargrodsky, 2010; De Soto, 2000); secure tenure raises investment and reallocates factors (Besley and Ghatak, 2010; Goldstein and Udry, 2008; Banerjee et al., 2002); and certification delinks rights from use, letting households leave without losing their land (de Janvry et al., 2015; de Brauw and Mueller, 2012). Such evidence extends to rural China, on titling (Liu et al., 2023), on expropriation and tenure insecurity (Jacoby et al., 2002; Giles and Mu, 2017), and on land misallocation (Chari et al., 2021). But these solutions assume the government enforces land rights, an assumption that fails when local authorities are themselves the source of expropriation. Where state predation is the binding constraint, formalizing title on paper does little (Jacoby and Minten, 2007). External adjudication works differently. It does not require the state's cooperation but constrains the state directly, protecting citizens against expropriation. An independent court can thus shape structural transformation where formalizing title alone cannot.

2 Institutional background

2.1 Rural land expropriation for urban development

Over the past decades, China's growth and urbanization have been accompanied by large-scale conversion of rural land into urban construction land for commercial and industrial use. Official statistics show that urban built-up area nearly tripled, from 22.4 to 60.7 thousand square kilometers, between 2000 and 2020. This expansion has made land a central input into urban growth and accordingly increased urban demand for rural land.

That demand, in turn, has generated pervasive urban–rural land disputes. Institutionally, local governments hold a *de facto* monopoly over the supply of construction land and can exercise substantial discretion in converting collectively owned rural land into state-owned urban land through expropriation. They also face strong incentives: land conveyance revenue finances local budgets and supports investment and measured growth, and they possess the administrative capacity to implement takings (He et al., 2022; Chen and Kung, 2019). Yet rural land institutions were largely designed to govern within-village disputes,[4] offering limited protection when the local state itself is

[4]The rural land institutions at that point include: The introduction of the Household Production Responsibility System in 1979 granted households control over farming decisions and output. The Land Management Law of 1998 provided farmers with long-term formal land-use contracts, and the

the counterparty. This combination of monopoly power and institutional gaps leaves farmers' land rights insecure (Ruan and Wang, 2022; Sargeson, 2013).[5]

Consequently, land security is strongly usage-dependent (Figure 1), a pattern also documented in other settings with weak property rights (albeit for different reasons) (Gottlieb and Grobovšek, 2019; de Janvry et al., 2015; de Brauw and Mueller, 2012; Galiani and Schargrodsky, 2010). Farmers who remain in agriculture face an expropriation risk of about 2%, whereas households engaged in off-farm work face a risk exceeding 14%, more than seven times higher. The disparity highlights the tension between urban expansion and rural land rights and motivates institutional mechanisms that can credibly adjudicate urban–rural land disputes.

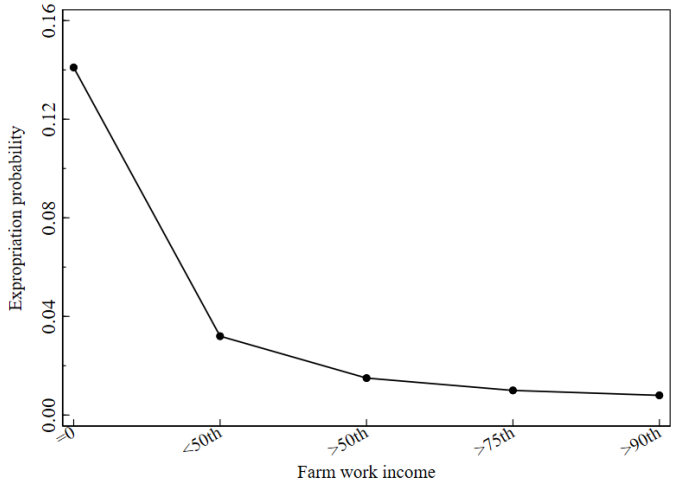


Figure 1: Farm work participation and expropriation probability

Note: This figure presents the relationship between farm work participation and expropriation probability. The statistics are based on pre-TJAL data. "Farm work income=0" refers to off-farm rural households. Appendix Table A.1 presents the OLS estimates of the correlation between off-farm work and expropriation probability.

Rural Land Contracting Law of 2003 extended transferable land-use rights among farmers.

[5]Illustrative disputes include Wukan (Guangdong, 2011–2013), where villagers protested alleged land seizures/opaque transfers and inadequate compensation (Reuters); Shangpu (Guangdong, 2013), where unrest followed a contested land deal. Reuters reports a long-term lease of roughly 33 hectares for an industrial project that villagers said lacked majority consent (Reuters); and Pingdu (Shandong, 2014), where residents claimed they were not properly informed about the land sale/compensation in a requisition dispute (Caixin). These episodes reflect recurring fault lines: the state (often the local government) is both regulator and counterparty, compensation is widely perceived as low relative to post-conversion values, and procedures/consent are frequently contested, against a backdrop in which rural land compensation has historically been calculated as multiples of agricultural output value.

2.2 Judicial capture by local power

In theory, any disputes over land between farmers and governments can be appropriately resolved in courts, providing a mechanism to rectify governmental abuse of power in land expropriation. In practice, however, local courts are deeply intertwined with and often captured by government authorities, driven by promotion incentives and supervision pressures to boost local economy (Zhao et al., 2024; Zhao, Yu and Guo, 2022; Liu et al., 2022). Two institutional features make such capture particularly feasible.

First, venue rules typically require plaintiffs to sue in the jurisdiction where the defendant government is located. China's four-tier court system mirrors the administrative hierarchy and shares the same territorial jurisdiction.[6] Under the "plaintiff accommodates to defendant" principle (*yuangao jiu beigao*), land-related disputes against local governments are therefore heard by courts operating within the defendant's own jurisdiction.[7] While this rule may reduce investigation and enforcement costs, it also makes it easier for local governments to influence adjudication.

Second, many court matters are influenced by the government. Politically, court presidents are subordinate to regional party secretaries and government heads. Local court presidents are typically appointed by corresponding-level governments and party committees. Fiscally, the operation of local courts is heavily dependent on local governments, which review budget accounts, allocate funds, and approve final accounts for courts. Administratively, courts require assistance from other departments, such as the police and procuratorate, for their daily affairs. As a result, the average winning probability of non-governmental litigants related to land disputes is only 0.129 (see Table 1), which is a quarter of that for civil cases (Zhao et al., 2024) and half that for other administrative cases (Zhao, Guo and Yu, 2022).

2.3 TJAL reform

Motivated by longstanding concerns that administrative litigation ("citizens suing the government") was vulnerable to local interference, and recognizing an opportunity to discipline lower-level officials while diverting citizen grievances from legitimacy-eroding protests into courts, China's central authority decided to roll out the TJAL reform in

[6]Specifically, central, provincial, prefecture, and county governments correspond to supreme, higher, intermediate, and basic courts, respectively.

[7]Since the analysis focuses on administrative litigation, the defendants are government departments (or government-affiliated agencies) by construction. In other types of cases (e.g., civil-civil cases), the defendant could be other entities (e.g., firms).

2013 (Cao et al., 2023).[8] The core design is to separate judicial adjudication from the defendant government's territorial control:[9] within reformed prefectures, cases that would otherwise be heard by the court at the defendant government's jurisdiction will be reassigned by the prefectural intermediate court to other designated courts (or heard by the intermediate court itself),[10] shifting adjudication from intra-regional to inter-regional assignment.[11]

Appendix Figure A.2 provides a case illustration of the TJAL reform. Before the reform, each county court handled cases within its own jurisdiction (namely intra-regional jurisdiction, Panel A.2a). After TJAL reform (Panel A.2b), jurisdiction becomes inter-regional: cases previously under the jurisdiction of county A are now judged by county B, cases from county B are judged by county C, and cases from county C are judged by county A.

Where TJAL is adopted, inter-regional adjudication of land-related disputes more than doubles, from 38% to 77%, whereas it remains near 40% in unreformed prefectures (Panel A.3b). Appendix Figure A.3 plots the share of cases judged inter-regionally over time. Both groups sit near 40% before TJAL (Panel A.3a), reflecting arbitrary jurisdictional practices, and the shares diverge afterward, reaching a 30% gap by late 2019. Although TJAL mandates inter-regional jurisdiction *de jure*, these patterns indicate it was implemented *de facto* for land-related disputes.

3 Data

To study how TJAL affects rural household outcomes, I use the National Fixed Point Survey (NFPS), a panel-structured survey conducted by the Research Center of Rural Economy under the Chinese Ministry of Agriculture. Initiated in 1986, this survey focuses solely on rural households and provides comprehensive information on agricul-

[8] Appendix Figure A.1 presents the reform timing across prefectures. I present two separate panels for years before 2016 (Panel A.1a) and all years (Panel A.1b), to fit the time scope of household and judgement datasets discussed below.

[9] The reform targets all and only administrative cases. And this reform is different from Liu et al. (2022), who study the centralization of below-province level court personnel and budgetary decisions to the provincial courts.

[10] Given China's intense political competition, counties are unlikely to collude on the case judgments (Fang et al., 2022; Zhou, 2007).

[11] Importantly, this is not a niche reform for a rare case type; land expropriation and demolition disputes are a major component of administrative litigation, accounting for roughly 13–17% of first-instance administrative suits in 2015–2017, making villagers' land-taking disputes a common and salient part of the policy-relevant docket affected by inter-regional jurisdiction (see Xinhua News). Appendix Section A.3 presents a set of representative cases illustrating how villagers challenge local government land practices through administrative litigation.

tural production, income, and employment at both household and village level, making it a valuable source for studying rural economic growth in China. I use annual waves of data between 2008 and 2015, covering 21,418 households in 326 villages from all 31 provincial administrative units in mainland China. By design, NFPS villages are selected to ensure representativeness across various socio-economic dimensions, including but not limited to region, population, cropping pattern, income, and off-farm activity. In each NFPS village, a random sample of households, ranging in size from 50 to 100 depending on the village population, is selected for the survey. If a household permanently relocates, it is replaced by a similar household with a new household identification code.[12]

To evaluate urban impacts, I use the Urban Household Survey (UHS). Similar to the Consumer Expenditure Survey in the United States, UHS is conducted by the National Bureau of Statistics of China. This dataset employs a 3-year rotating Panel design, collects detailed information (income, expenditure, labor supply, and other demographic characteristics) from urban households, and boasts a larger sample size compared with other surveys (Santaeulàlia-Llopis and Zheng, 2018). Herein, I utilize the UHS dataset between 2008 and 2015, covering major provinces across different regions of China.[13]

To validate the judicial responses, I draw on judicial judgments from China Judgments Online (CJO), administered by the Supreme People's Court (SPC), the official platform where all courts are required to upload their decisions since 2013. For this study, I focus on administrative judgments related to land and related-enforcement disputes between 2013 and 2019, encompassing 207,129 cases. For each case, I extract key information, including details about writs (e.g., length, dates of prosecution and judgment, causes of litigation, courts where the judgment took place), plaintiffs and defendants (e.g., names, types,[14] quantities), judges, as well as the winning party,[15]

[12]The source of this data is Huang et al. (2024). See Benjamin et al. (2005) for a thorough evaluation of the high quality of NFPS.

[13]The source of data is Zhao, Guo, Zou and Song (2022). The sample used in this paper includes data from 11 provinces (Beijing, Hebei, Shanxi, Liaoning, Jilin, Heilongjiang, Shanghai, Jiangsu, Zhejiang, Anhui, and Fujian) for the period 2008–2009 and from four provinces (Shanghai, Liaoning, Guangdong, and Sichuan) for the period 2010–2014. Since the TJAL reform begins in 2013, both pre- and post-treatment UHS observations fall within the same four-province sample, preserving identification.

[14]Plaintiffs are broadly classified into legal persons (e.g., firms, economic cooperatives, and resident committees) and natural persons. Due to the nature of administrative judgments, all defendants are government departments.

[15]The outcome of a case is primarily determined by the extent to which the plaintiff's claims are satisfied. If the majority of the plaintiff's claims are upheld, the case is considered a win for the plaintiff. Conversely, if most claims are not upheld, the case is deemed a win for the defendant. I also

following Zhao et al. (2024); Zhao, Yu and Guo (2022).

To measure urban and sectoral adjustments, I draw on four additional prefecture-level sources. Urban area comes from the China Urban Construction Statistical Yearbook, which reports urban land use by prefecture and year. Urban land prices come from the China Land Market Network, which records parcel-level land-conveyance transactions; I aggregate the transacted price to the prefecture by land-use category and month. Sectoral employment comes from the China City Statistical Yearbook, which reports employment by industry for each prefecture and year. Firm entry comes from the firm registry of the State Administration for Industry and Commerce (SAIC), which records firm registrations and exits by prefecture, industry, and year.

Table 1: Summary statistics

Variable	N	Mean	SD	Source
Any expropriation (=1 if yes; =0 if no)	149,042	0.036	–	
Off-farm work (=1 if yes; =0 if no)	149,042	0.526	–	
# of individuals who migrant out	2,278	1.321	5.256	
Total population at the village level	2,278	2073.26	1672.74	
Area of land cultivated (mu)	149,042	7.018	13.840	
Material expenditure (yuan)	149,042	833.52	1801.67	NFPS
Machinery expenditure (yuan)	149,042	509.25	1021.62	
Total income (yuan)	149,042	51461.47	81987.03	
Farm work income (yuan)	149,042	9917.29	17046.21	
Off-farm work income (yuan)	149,042	35510.91	77232.70	
Compensation income (yuan)	149,042	992.63	14471.56	
Other income (yuan)	149,042	4188.096	10910.20	
Total income (yuan)	142,169	64943.73	57680.65	UHS
Wage income (yuan)	142,169	41562.04	45885.75	
Plaintiffs win (=1 if yes; =0 if no)	207,129	0.129	–	CJO

Note: This table reports the summary statistics used in this paper. Farm work income refers to income generated from selling agricultural products. Off-farm work income refers to income generated from non-agricultural employment. Compensation for land expropriation refers to the payment made by the government for land lost through expropriation. Other income includes interest, dividends, bonuses, pensions, government transfers, gifts from relatives, insurance annuities, medical reimbursements, and other non-loan income. A household is defined as an off-farm work household if it generates income solely from off-farm activities. Additional statistics can be found in Appendix Table A.2. Yuan= Chinese yuan.

To code prefecture-level TJAL adoption dates, I collect reform timing from *Practice and Exploration of Administrative Litigation Reform*, a book which documents the rollout of administrative litigation pilots across localities. I then triangulate these dates

consider alternative ways of defining plaintiff win in Appendix Table B.3.

using official notices (e.g., court and government announcements) and contemporaneous local media reports. This source-combination closely follows the validation logic in related work (Cao et al., 2023; Zhao, Guo and Yu, 2022) while potentially providing a broad coverage of implementations. In my sample, the share of prefectures adopting TJAL rises from 0% in 2013 to 23% by 2015, and reaches 52% by 2019.

Table 1 reports summary statistics for the main variables. The average rural household faces an annual expropriation probability of 3.6 percentage points. Because many rural households combine farm and off-farm activities (Huang et al., 2024), I classify a household as participating in off-farm work only when it reports zero farm income; under this definition, about 25% of households exclusively engage in off-farm work.

At the village level, the mean population is 2,073, and permanent out-migration averages 1.3 individuals per year, implying that permanent migration to cities is uncommon during the sample period. In agriculture, the average household cultivates 7 mu and spends about 450 yuan on machinery and 122 yuan on irrigation, generating 9,917 yuan in farm income. Off-farm income averages 36,363 yuan, bringing total annual household income to 51,461 yuan. Urban households earn 64,943 yuan per year on average, roughly two-thirds of which comes from labor wage income (41,562 yuan).

Judicial statistics further show that non-government litigants prevail in only 12.9% of cases, and roughly 90% of plaintiffs are individuals (Appendix Table A.2). The dominance of citizen-versus-government litigation confirms courts, and in turn judicial independence, as the operative channel in shaping outcomes in administrative disputes.

4 Reduced-form empirical analysis

4.1 Model specification

To evaluate whether the TJAL reform affects structural transformation, I employ the following equation, where the unit of observation is household i in prefecture p, and year t:

$$Y_{i,p,t} = \beta_0 + \beta_1 TJAL_{p,t} + \gamma_i + \zeta_t + X_{i,t} + X_{p,t} + \varepsilon_{i,p,t}. \tag{1}$$

$Y_{i,p,t}$ denotes the household-level outcomes (e.g., off-farm work participation). $TJAL_{p,t}$ is an indicator that equals one if the prefecture p has reformed, otherwise zero. β_1 captures the average effect of TJAL on household outcomes, by comparing the households in treated prefectures with those in untreated prefectures.

Several assumptions underpin the causal interpretation of β_1. First, the parallel

trends assumption requires that, absent the TJAL reform, outcomes in treated and untreated prefectures would have evolved similarly, conditional on the fixed effects and covariates described below. Second, the no-anticipation assumption requires that households and local governments did not alter their behavior in advance of the reform's implementation. Third, the selection-into-treatment assumption permits systematic differences between treated and control prefectures, but requires that such differences remain stable over time, that is, selection into treatment is based on time-invariant characteristics rather than on time-varying trends in outcomes or policy conditions. Fourth, the stable unit treatment value assumption (SUTVA) requires that the treatment status of one prefecture does not affect outcomes in other prefectures, a concern if, for example, households migrate across boundaries in response to differential land rights protection.

To (partially) address these concerns, the model includes several sets of controls. Household fixed effects (γ_i) absorb all time-invariant household factors (e.g., tradition, local connection, agricultural ability) that could otherwise confound comparisons across treated and untreated areas. Year fixed effects (ζ_t) net out nationwide shocks and policy changes common to all locations in a given year (such as land-law changes). $X_{i,t}$ adjusts for within-household changes that affect exposure/vulnerability in expropriation (e.g., head demographics, political status, human capital), reducing residual confounding and improving precision.[16] $X_{p,t}$ accounts for evolving local economic and fiscal conditions that may jointly influence both TJAL rollout and land-taking/compensation dynamics (e.g., growth, fiscal capacity, population pressure, industrial structure); it includes the IHS transformation of GDP per capita, the IHS transformation of government expenditure, the IHS transformation of population, and the GDP share of secondary industry. $\varepsilon_{i,p,t}$ denotes the error term. As the treatment is assigned at the prefecture level, I cluster standard errors at the prefecture level throughout, following Abadie et al. (2023) who demonstrate that standard errors should align with the level of treatment assignment. Additional robustness checks in Appendix Section E verify the robustness of baseline results to other combinations of controls and inference choices (e.g., clustering at the household or village level).

[16]For NFPS dataset, I include a dummy=1 if the household head is a member of the Communist Party of China, a dummy=1 if the household has any religious affiliation, and a dummy=1 if the household belongs to a minority ethnic group. For later UHS dataset, I include the age of household head, a dummy=1 if household head is female, a set of dummies indicating education levels, and a dummy=1 if household head is in marriage. The difference is due to variable availability across surveys.

4.1.1 Design validity

The most direct threat to the parallel trends and no-anticipation assumptions is that treated prefectures may have been on diverging trajectories before the reform, or that agents adjusted behavior in anticipation of it. To assess this, I examine event-study estimates of TJAL on rural income and test the sensitivity of pre-reform coefficients to departures from parallel trends. The results are reported in Appendix Section E.1, and provide support for both assumptions.

A further concern is that, in a staggered adoption design, TWFE estimates may be contaminated by heterogeneous treatment effects across groups and time periods (Goodman-Bacon, 2021). To address this, I replicate the baseline using heterogeneity-robust estimators. The results, reported in Appendix Section E.2, closely align with the TWFE baseline, lending confidence to the main specification.

The selection-into-treatment assumption could be violated if prefectures adopting TJAL differ systematically from those that do not, or if reform timing responds to evolving local conditions. To evaluate this, I examine whether pre-reform observable characteristics predict treatment adoption and timing. The balance and timing tests, reported in Appendix Sections E.3 and E.4, find no evidence of systematic selection (over time).

Finally, the SUTVA assumption could be violated if the reform in treated prefectures influences outcomes in untreated ones, for instance through migration flows induced by differential land rights protection. To examine this, I test for differential sample attrition in Appendix Section E.9 and examine changes in village population in the main text below. Both find no evidence of significant spillovers, supporting a credible causal interpretation of the estimates.

4.2 Effects of TJAL on rural land rights

Table 2 reports DD estimates from Equation (1) using rural household–year data, with household and year fixed effects and a full set of time-varying household and prefecture controls. Columns (1) and (2) show that TJAL raises compensation per unit of land lost by 15.4% and 16.3%, respectively, indicating that the "price" rural households receive for land loss increases robustly after the reform. Because the NFPS does not directly record the area formally expropriated, I use the survey's measure of total land reductions, excluding reductions attributable to returning farmland to forest or land leasing; the remaining land loss is plausibly driven by expropriation-related takings. This construction implies that the per-unit estimates should be interpreted as a lower

bound on how TJAL affects compensation per unit of land expropriated, if some non-expropriation losses remain in the measure.

Table 2: The impact of TJAL on rural land rights

Dep. Var.	Compensation per land lost		Land lost		Expropriation probability	
	(1)	(2)	(3)	(4)	(5)	(6)
TJAL	0.154**	0.163**	-0.009	0.004	-0.004	0.011
	(0.072)	(0.076)	(0.007)	(0.004)	(0.005)	(0.012)
TJAL×				-0.052***		-0.029*
Off farm				(0.017)		(0.017)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes
MDV	0.062	0.062	0.051	0.051	0.041	0.041
Observations	149,042	149,042	149,042	149,042	149,042	149,042
Adjusted R^2	0.213	0.215	0.558	0.558	0.426	0.426

Note: This table reports the DD estimates of the impact of TJAL on rural land rights. The level of observation is the household-year. The dependent variable is the IHS transformation of household's compensation per mu of land lost for Column (1)–(2), the IHS transformation of household's area of land lost (mu) for Column (3)–(4), and a dummy variable that equals one if a household in a certain year is expropriated for Column (5)–(6). Land lost is defined as those not driven by returning farmland to forest or by rent. The model specification follows Equation (1). MDV = mean of the dependent variable. Robust standards (in parentheses) are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1. IHS-transformed estimates are robust to outcome unit scaling following (Chen and Roth, 2024); Poisson estimates confirm the sign and magnitude (Appendix SectionE.7, Appendix TableE.5).

Columns (3)–(6) examine the extensive margin of expropriation. The aggregate effects of TJAL on the area of land lost (Column 3) and the probability of experiencing expropriation (Column 5) are small and statistically indistinguishable from zero. However, this average masks important heterogeneity driven by usage-dependent expropriation risk. Suggested by both the theory and the styled facts in Section 2.1, off-farm households, whose land is left unattended, face a higher baseline risk of expropriation, as governments can more easily target parcels without active protection. Columns (4) and (6) test this prediction by interacting TJAL with an indicator for pre-reform off-farm status (measured in 2012, prior to any treated prefecture's adoption), which isolates a predetermined source of exposure. The interaction coefficients are negative and statistically significant: off-farm households in treated prefectures experience a significant reduction in land loss (−0.052***, Column 4) and a marginally significant reduction in expropriation probability (−0.029*, Column 6). These interactions thus

indicate targeted legal protection of the highest-risk rural landholders, consistent with stronger rural land rights where the risk of abuse is greatest.

To corroborate that the household-level effects reflect the judicial reform rather than concurrent changes in land policy or local economic conditions, similar to Cao et al. (2023), I turn to case-level evidence with a particular focus on land-related disputes. If TJAL is the operative reform, it should generate observable shifts in adjudication: more cases should be heard by non-local courts, and non-government litigants should fare better in disputes against local governments. I therefore estimate an analogous difference-in-differences specification using case–year data; Appendix Table B.1 reports the results.

I first assess *de facto* implementation. Column (1) shows that TJAL increases the inter-regional jurisdiction rate by 14.6 percentage points (38%), consistent with the raw patterns in Appendix Figure A.3 and indicating that the reform meaningfully reassigns cases away from courts embedded in the defendant locality. Under this new institutional arrangement, non-government litigants, who are plausibly disadvantaged when the defendant government influences local adjudication, should become more likely to prevail. Consistent with this prediction, I find a 2.1 percentage-point increase in their win rate, or 16.3% relative to the sample mean (Column (2)).

A remaining concern is that the estimated improvement in plaintiff outcomes could reflect changes in the flow or composition of cases rather than adjudication itself: for example, if the reform induces selection such that only "easier-to-win" cases are filed or pursued. The data provide little support for this alternative explanation. Appendix Table B.5 shows only small (0.3–0.5%) and statistically insignificant changes in the number of cases in the relevant categories, and Appendix Figure B.2 shows no meaningful shift in case composition.[17] Together, these patterns suggest that the post-reform increase in plaintiff success is unlikely to be driven by changes in the caseload. Cao et al. (2023) report an 11% rise in overall administrative case volume after the reform. Because the estimates here cover land and related enforcement disputes alone, stable volume within that narrower category is consistent with a rise in total filings.

I also conduct a set of robustness checks. Appendix Figure B.1 shows no differential pre-trends in case outcomes between treated and untreated prefectures, supporting the parallel-trends assumption. To address concerns that the results are confounded by other contemporaneous judicial reforms (e.g., Liu et al. (2022)), I implement a

[17]The dependent variable is a dummy=1 if the plaintiff falls into one of four categories: individual, resident committee, firm, or economic cooperative. The estimates for each category are insignificant and close to zero, indicating no significant changes in case composition.

placebo test using non-administrative cases, which should not be directly affected because TJAL applies only to administrative litigation. Appendix Table B.4 shows no corresponding change in outcomes for non-administrative cases: the estimates are small, statistically indistinguishable from zero, and in some specifications opposite in sign to the land-case results. This is a sharp contrast that supports the specificity of TJAL's effect to administrative land disputes. Finally, the estimates are robust to other combinations of control (Appendix Table B.2) and to alternative measures of plaintiff success.[18] Taken together, the case-level evidence matches the household-level pattern, showing how independent adjudications could be a source of rural land rights against predatory expropriation by local governments.

4.3 From legal protection to rural factor allocation

Building on section 4.2, this section examines whether TJAL-induced improvements in rural land rights translate into changes in rural factor allocation, a core channel through which legal institutions may shape structural transformation. Prior research suggests that insecure land rights can keep labor and land in low-return uses and distort investment incentives (Field, 2007; Besley, 1995). Although a large literature studies how land policies (e.g., land titling, land tenure) affect allocation, whether judicial reforms such as TJAL generate meaningful reallocation, and through which margins, remains an open question. I address this by estimating TJAL's effects on employment, migration, land rental, agricultural input use, and income of rural households.

I begin by examining whether TJAL affects rural households' employment choices. By estimating Equation (1), I compare rural households in treated and untreated prefectures over time, and use a dummy indicator equal to one if the household reports income from off-farm work as the outcome variable. Column (1) of Panel A in Table 3 shows that TJAL increases the probability of participating in off-farm work by 4.7 percentage points (or 8.9%). Notably, before the reform, households with off-farm participation are more likely to experience land expropriation, consistent with off-farm employment reducing households' ability to monitor, contest, or negotiate over land claims when local authorities initiate takings. After TJAL, this positive association becomes small and statistically indistinguishable from zero, suggesting that the reform mitigates the vulnerability of off-farm households to expropriation and weakens the

[18] In the main regressions, I define case outcomes based on the satisfaction of the plaintiff's claims. I define case outcomes based on the allocation of litigation costs. If defendants take more litigation costs than plaintiffs, the case is deemed a win for the plaintiff. The estimates are consistent with main results (Appendix Table B.3).

Table 3: The impact of TJAL on rural factor allocation

Dep. var.	A. Employment			B. Migration	
	Any off-farm work			Migrants	Population
	(1)	(2) Before	(3) After	(1)	(2)
TJAL	0.047***			-0.099**	0.017
	(0.016)			(0.047)	(0.017)
Any expropriation		0.045**	-0.011		
		(0.021)	(0.019)		
AVMDV				1.321	2073.256
MDV	0.526	0.515	0.699	0.615	8.079
Observations	149,042	139,887	9,155	2,278	2,278
	C. Land rental			D. Agriculture inputs	
Dep. var.	(1) Rent in	(2) Rent out	(3) Total area	(1) Material	(2) Machine
TJAL	-0.012	0.051	-0.034***	0.155**	0.144
	(0.009)	(0.063)	(0.011)	(0.071)	(0.121)
TJAL×Farming	0.018**	-0.110*	0.048***		
	(0.009)	(0.063)	(0.012)		
MDV	0.098	0.223	1.749	6.765	5.395
Observations	149,042	149,042	149,042	111,506	111,506
	E. Household income				
Dep. var.	(1) Farm	(2) Off-farm	(3) Comp	(4) Other	(5) Total
TJAL	-0.180	0.092**	0.094	0.005	0.073**
	(0.184)	(0.045)	(0.126)	(0.100)	(0.029)
MDV	7.172	9.631	0.343	7.584	11.139
Observations	149,042	149,042	149,042	149,042	149,042
Controls	Yes	Yes	Yes	Yes	Yes

Note: This table reports the impact of TJAL on rural factor allocation. The unit of observation is household-year for Panel A, C, D, and E. The unit of observation is village-year for Panel B. Only Panel D restricts the sample to farming households, and the rest uses the whole sample. For Panel A, C, D, and E, the specification follows Equation (1). For Panel B, the specification follows: $Y_{v,p,t} = \mu_0 + \mu_1 TJAL_{p,t} + \zeta_t + \gamma_v + X_{v,t} + X_{p,t} + \varepsilon_{v,p,t}$, where $Y_{v,p,t}$ denotes the outcome variable of village v in prefecture p and year t; $TJAL_{p,t}$, ζ_t and $X_{p,t}$ are defined the same as in Equation (1); γ_v denotes village fixed effects; $X_{v,t}$ denotes village-level controls, including the IHS transformation of the number of party members, the IHS transformation of the number of individuals with minority, and the IHS transformation of the number of individuals with religion; $\varepsilon_{v,p,t}$ is the error term. The dependent variables are: a dummy=1 if a household participates in off-farm work for Panel A; the IHS transformation of the number of individual migrating out and village's total population for Columns (1)–(2) of Panel B; the IHS transformation of the area of land rented in, rented out, and total land for Columns (1)–(3) of Panel C; the IHS transformation of material and machine expenses for Columns (1)–(2) of Panel D; the IHS transformation of farm work income, off-farm work income, compensation income from land expropriation, other income, and total income for Columns (1)–(5) of Panel E. Controls include household FE, year FE, household-level and prefecture-level characteristics for Panel A, C, D, and E; include village FE, year FE, village-level and prefecture-level characteristics for Panel B. AVMDV= Absolute value of mean of the dependent variable. MDV=mean of the dependent variable. Standard errors (in parentheses) are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

link between off-farm employment and land insecurity.

As TJAL effectively affects households' employment choices, a natural next question is whether it also changes the productivity composition of those who remain in agriculture. Following (Chari et al., 2021), I measure household agricultural TFP as the average of fixed effect components among all cultivated crops, conditional on observed capital, material, and labor inputs. Column (1) of Table 4 shows little average effect of TJAL on agricultural TFP, which is unsurprising because this measure is intended to capture underlying farming productivity rather than changes driven by capital or other input use. Column (2), however, reveals substantial heterogeneity by farming status: the coefficient on TJAL × Farming is positive and precisely estimated, while the standalone TJAL coefficient is significantly negative. Together, these estimates imply that after the reform, households that remain in farming are positively selected on agricultural productivity relative to other non-farming households. In other words, TJAL does not merely affect whether households engage in farm work; it also improves selection into farming, with agriculture increasingly undertaken by more productive cultivators.

Table 4: The impact of TJAL on the selection into farm work

Dep. Var.	Agricultural TFP	
	(1)	(2)
TJAL	-0.002	-0.074***
	(0.001)	(0.012)
TJAL×Farming		0.094***
		(0.015)
Controls	Yes	Yes
MDV	-0.028	-0.028
Observations	124,223	124,223
Adjusted R^2	0.659	0.660

Note: This table reports the DD estimates of the impact of TJAL on the selection into farm work. The level of observation is the household-year. The dependent variable is the agricultural TFP, following the approach by (Chari et al., 2021). The model specification follows Equation (1). In Column (2), I add an interaction between reform indicator and a dummy that equals one if the household participates in farm work. Controls include village FE, year FE, household-level and prefecture-level characteristics. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

While increased off-farm employment may encourage rural-to-urban migration (de Janvry et al., 2015), lower expropriation risk can reduce expropriation-induced migration (Ma and Mu, 2020). These competing predictions warrant empirical testing. Using village-level data, Column (1) of Panel B shows that TJAL reduces the number of

out-migrants by 9.9%, suggesting that the decline in expropriation-induced migration dominates any employment-driven increase. At the same time, the implied change in outflow is small in levels, about 0.131 individuals relative to an average village population of 2,073, so it is unsurprising that I find no detectable change in total village population (0.017, see Column (2)). As a primary evidence, this suggest limited SUTVA violation. Combined with employment results above, the labor reallocation effects of TJAL operate primarily through employment choice rather than permanent out-migration.

Increased off-farm employment can trigger land reallocation by shifting cultivation toward households that remain in farming, thereby activating land rental markets Chari et al. (2021). If TJAL strengthens land rights and reduces expropriation risk, it may encourage households to adjust landholdings through renting rather than holding land defensively. To test for land reallocation, I examine three outcomes: the area of land rented in, the area of land rented out, and total cultivated area (the equilibrium landholding for farming). In each specification, I interact the TJAL indicator with an indicator for farm participation to allow differential effects for farming versus non-farming households.[19] Panel C reveals a clear land consolidation pattern. The key mechanism is that secure property rights unlock a land rental market that was previously suppressed by expropriation risk: non-farming households that previously held land defensively, reluctant to rent out for fear of weakening their tenure claim, can now do so safely under stronger judicial protection. Accordingly, non-farming households in TJAL-treated prefectures rent out 5.1% more land and reduce their total cultivated area by 3.4%. On the other side of the same market, farming households absorb this land: they rent in 0.6% more and reduce renting out by 5.9%, expanding their total cultivated area by 1.4%. The total-area results, which capture the equilibrium landholding for each group, provide the clearest summary: TJAL shifts land from non-farming to farming households, concentrating cultivation among those who actively use it.

Stronger land rights may induce better incentives for farmers to expand land-attached inputs whose returns depend on continued control of the plot (Goldstein et al., 2018). To test whether TJAL strengthens investment incentives, I examine agricultural input use among farming households, restricting the sample to farms to

[19]This specification is particularly appropriate because employment choices are endogenous before and after TJAL. For instance, among households engaging in farm work before TJAL, those who switch to off-farm work after the reform may transfer their land to others. A scenario like this will complicate the interpretation of land per household estimates, making conventional methods (e.g., Huang et al. (2024); Liu et al. (2023); Chari et al. (2021)) that distinguish households based on pre-TJAL farming status unsuitable. Therefore, I use contemporaneous farming status that incorporates the employment decision after the reform.

abstract from endogenous exits from agriculture. Panel D of Table 3 shows that TJAL increases material input expenses by 15.5% (significant at the 5% level), with machine expenses rising 14.4% in the same direction. These results suggest that stronger judicial protection improves rural factor allocation not only by reallocating land toward active and more productive farmers (via the rental market), but also by inducing capital deepening among those who continue to farm.

To bring the above mechanisms in the same scope, I examine how TJAL affects rural households' income. Column (1) of Panel E shows that TJAL is associated with an 18% decline in farm income, though this estimate is imprecise. This decline need not reflect lower agricultural productivity; instead, it is consistent with a contraction in farm labor as households shift time toward nonfarm activities. Restricting the sample to farming households, Appendix Table D.1 shows a small and statistically insignificant 1.1% increase in farm income per farming household, which becomes economically and statistically significant (10.4%) when interacted with plausibly exogenous favorable weather conditions, a case whose returns are higher when the marginal product of inputs is elevated. The compensation income channel points in the same direction (9.4%), though less precisely estimated. This is not a contradiction of the per-unit compensation effect (+16.3%**) in Table 2, but rather reflects that relatively few households and less land actually experience expropriation in any given year, so the aggregate compensation income channel is inherently noisy. At the same time, TJAL raises off-farm income by 9.2% and total household income by 7.3%. A back-of-the-envelope calculation implies average changes of –1,785 yuan in farm income, +3,267 yuan in off-farm income, and +3,757 yuan in total income, indicating that off-farm earnings account for most of the overall gain. Consistent with the reform operating through land rights and allocation margins rather than general transfers, I find no meaningful change in "other income" (e.g., pensions and interest; +0.5% or +20.94 yuan), which serves as a placebo outcome.

Taken together, the income results reinforce the factor-allocation interpretation: TJAL is associated with labor shifting away from farming and toward off-farm activities, alongside evidence that returns to farming among remaining farmers improve when conditions are favorable, patterns consistent with greater rural reallocation and higher household welfare.

4.3.1 Heterogeneity

If observed effects are driven by land expropriation risk, villages with higher commercially valued lands may benefit more from the presence of independent courts. Two

naturally occurring sources of variation, plausibly orthogonal to the reform, merit attention: geographic proximity to cities and terrain ruggedness. Consistent with the literature (Ma and Mu, 2020), I observe this geographically determined land expropriation risk using NFPS data (see Appendix Table D.2). Specifically, a 1% increase in the distance to the nearest prefecture (in the terrain ruggedness index) is associated with a 0.51% (0.6%) decrease in the proportion of households expropriated, significant at the 1% (5%) level.

Appendix Figure D.1 splits the sample at the medians of distance to the nearest prefecture and terrain ruggedness. The TJAL's effect on total income is larger in villages closer to prefectures (0.093 vs. 0.035 in more distant areas) and on flatter terrain (0.112 vs. 0.051 in more rugged areas). Additionally, the effect is larger where non-governmental litigants were more disadvantaged prior to TJAL in land cases, confirming that the reform operates through the land-rights channel. As a placebo, I repeat the split using the pre-TJAL winning rate for non-administrative cases, which captures general local judicial quality rather than land-specific disadvantage; this split yields no significant differential effect. The contrast between the two splits supports the interpretation that TJAL's household effects run through its specific impact on land-dispute adjudication, not through a general improvement in local court quality. Similar patterns hold for off-farm employment and off-farm income. These results are consistent with expropriation risk as the underlying factor driving the observed reallocation effects.

4.3.2 Theoretical validation

In theory, the observed reallocation effects can be rationalized by a partial-equilibrium model with insecure land rights (see Appendix Section D). The model assumes a stand-in village v inhabited by F farmers ranked by their agricultural TFP, A_i. Farmers choose between farm and off-farm work, but not both, to maximize their total income. Engaging in off-farm work can incur an expected land loss (c_v) due to expropriation. Given the setup, the model generates a threshold farmer z who is indifferent between farm work and off-farm work. Farmers with $A_i > A_z$ ($A_i < A_z$) will participate in farm work (off-farm work). More secure land rights ($c_v = 0$) will increase the net return to off-farm work, leading to more off-farm work participation, more land rentals and higher average income. These theoretical predictions mirror the empirical reallocation effects.

4.3.3 Other explanations

A potential concern is that TJAL, as a broad reform to administrative litigation, may affect the business environment beyond its direct effects on land expropriation disputes. If TJAL improves the adjudication of firm-related administrative cases, such as business permit denials or regulatory disputes, it may raise urban firm productivity, push up urban wages, and thereby improve farmers' outside option. Under this alternative channel, the observed rural reallocation would reflect a demand-side pull from a more productive urban sector, rather than a supply-side push from more secure rural land rights.

The two channels, however, generate sharply different predictions for the urban sector. A productivity-driven pull would require urban wages to rise and cities to expand as greater firm activity absorbs more workers and investment. The land rights mechanism predicts the opposite: as TJAL constrains government expropriation, urban land supply tightens, city expansion slows, land prices rise, and land-intensive sectors face higher costs, even as rural labor flows in. Section4.4 presents urban-side evidence that is consistent with the latter and inconsistent with the former, supporting the interpretation that TJAL operates primarily through strengthening rural land rights rather than through improvements to the broader business environment.

4.3.4 Additional robustness checks

Before concluding this section, I conduct two additional robustness checks not discussed above. First, I examine whether contemporaneous judicial reforms confound the estimates. Adding indicators for four concurrent reforms (interprovincial circuit tribunals, court management centralization, judge quota reform, and anti-corruption patrols) leaves the TJAL coefficient essentially unchanged, with these reforms collectively accounting for less than 5% of the estimated effects (Appendix SectionE.5). Second, to address concerns about the arbitrary scaling of the dependent variable, I re-estimate the baseline using Poisson estimation. The results remain consistent with the baseline (Appendix Section E.7).

4.4 Urban adjustments to stronger rural land rights

Having established that TJAL strengthens rural land rights and improves rural factor allocation, I now turn to the other side of the transformation process: urban adjustments. By reducing expropriation risk, TJAL frees rural labor from farming and promotes off-farm employment, yet simultaneously constrains cities' access to land,

dampening the expansion of urban sectors. This tradeoff generates a positive labor-supply shock and a negative land-supply shock to urban areas. This section examines the urban response to these shocks.

As TJAL releases rural labor into off-farm work, its first-order urban implication is an increase in city employment (i.e., manufacturing plus service employment). I test this prediction by comparing city employment outcomes in treated and untreated prefectures. Column (6) of Table 5 shows that TJAL raises city employment by 4.6%, significant at the 10% level. Together with the rural evidence on off-farm participation in Section 4.3, these results document a labor-side reallocation: stronger legal protection of rural land shifts workers out of agriculture and into urban activities, consistent with structural transformation.

The same labor inflow may depress urban wages, especially for urban natives who are fully attached to nonfarm work. To assess this, I use the urban household survey and estimate Equation (1), comparing urban natives in treated and untreated prefectures before and after TJAL. Column (1) of Table 5 shows that wage income declines by 5.8% for urban natives. As a placebo, non-wage income is unchanged (Column (2)), implying that the wage channel accounts for most of the decline in total income (a 1.4% drop; Column (3)). Event-study estimates exhibit parallel pre-trends (Appendix Figure F.1), supporting the identification strategy. Overall, the evidence is consistent with labor-supply-driven wage pressure and confirms that stronger rural land rights is not uniformly beneficial.

By strengthening legal protection against expropriation, TJAL plausibly tightens cities' access to rural land and generates a negative land-supply shock to the urban sector. An important implication is slower physical expansion of cities in treated prefectures. Consistent with this prediction, Column (4) shows that TJAL reduces the urban area by 7.8% relative to untreated prefectures. This decline indicates that the enhanced legal protection of rural land translates into a meaningful constraint on urban spatial growth.

A tighter land supply should also be capitalized into higher urban land prices. Using transaction-level land auction data for manufacturing and services, I construct average prices at the use–year–month level and estimate a similar difference-in-differences specification. Column (5) shows that TJAL raises urban land prices by 6%; event-study estimates support parallel pre-trends of both area and price outcomes (Appendix Figure F.1). Together with the city-expansion results, this evidence highlights the land-side cost of stronger rural land rights for structural transformation and echoes the quantitative spatial-equilibrium perspective that land scarcity can impede urban growth

Table 5: The impact of TJAL on urban sector

Dep. Var.	(1) Wage inc	(2) Non-wage inc	(3) Total inc	(4) Urban area	(5) Land price	(6) Employment	(7) Employment	(8) Industrial output
TJAL	-0.058**	0.043	-0.014**	-0.078**	0.060**	0.046*		-0.057
	(0.024)	(0.038)	(0.007)	(0.039)	(0.025)	(0.037)		(0.037)
TJAL× Manufacturing							-0.027	
							(0.091)	
TJAL× Service							0.060***	
							(0.022)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MDV	9.067	8.839	11.504	8.060	15.894	9.612	9.612	26.427
Observations	142,169	142,169	142,169	2,332	145,212	45,961	45,961	2,332
Adjusted R^2	0.927	0.854	0.935	0.919	0.494	0.876	0.876	0.972

Note: This table reports the DD estimates of urban responses to TJAL reform. The level of observation is the household-year for Columns (1)–(3); the city-year for Columns (4) and (8); the city-usage-year-month for Column (5); the city-industry-year for Columns (6)–(7). For Columns (1)–(3), the specification follows Equation (1). For Columns (4) and (8), the specification follows: $Y_{p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \zeta_t + \eta_p + X_{p,2012} \times \zeta_t + \epsilon_{p,t}$, where $Y_{p,t}$ denotes the outcome variables for prefecture p in year t; ζ_t and η_p are year and prefecture fixed effects; $X_{p,2012} \times \zeta_t$ denotes the baseline characteristics in 2012 (i.e., the IHS transformation of GDP per capita) interacted with year fixed effects; $\epsilon_{p,t}$ is the error term; the rest terms are defined the same as in Equation (1). For Column (5), the specification follows: $Price_{u,p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \zeta_{u,t} + \eta_{u,p} + X_{p,t} + \epsilon_{u,p,t}$, where $Price_{u,p,t}$ denotes the urban land price of usage u in prefecture p and year-month t; $\zeta_{u,t}$ denotes usage-year-month fixed effects; $\eta_{u,p}$ denotes usage-city fixed effects; $\epsilon_{u,p,t}$ is the error term; the rest terms are defined the same as in Equation (1). I only include non-public usages (i.e., wetland, residence, commerce and service, industry and mining, land water surface, and others), where the land price is determined by auctions rather than direct allocation (therefore no price information). For Columns (6) and (7), the specification follows: $Y_{i,p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \zeta_{i,t} + \eta_{i,p} + X_{p,2012} \times \zeta_t + \epsilon_{i,p,t}$, where $Y_{i,p,t}$ denotes the outcome variables for industry i of prefecture p in year t; $\zeta_{i,t}$ and $\eta_{i,p}$ are industry-year and industry-city fixed effects; $X_{p,2012} \times \zeta_t$ denotes the baseline characteristics in 2012 (i.e., the IHS transformation of GDP per capita) interacted with year fixed effects; $\epsilon_{i,p,t}$ is the error term; the rest terms are defined the same as in Equation (1). The dependent variables are wage income for Column (1); non-wage income for Column (2); total income for Column (3); areas of prefectures (in kilometers) for Column (4); land price (in CNY per hectare) for Column (5); employment for Columns (6)–(7); industrial output for Column (8), all in IHS transformations. Controls are defined accordingly in the specifications. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level, except for Columns (1)–(3) at the household level due to the small number of clusters. *** p<0.01, ** p<0.05, * p<0.1.

(Hsieh and Moretti, 2019).

The interaction of the reform-induced labor inflow and the tighter urban land constraint implies heterogeneous sectoral responses. The literature suggests that manufacturing firms treat land and non-land inputs as weak substitutes (Behrens et al., 2025; Zhao, Huang, Ploegmakers, Lan, van der Krabben and Ma, 2022), so higher land prices and tighter land-use regulations disproportionately raise their costs and limit expansion (He et al., 2022; Tang et al., 2020; Wu et al., 2014). By contrast, service activities cluster in dense, high-rent prefecture centers and rely more on human and intangible capital than on physical space (Davis and Dingel, 2020, 2019; Combes et al., 2019); their output growth can be decoupled from the expansion of service-industry land (Zhang et al., 2023). Because manufacturing is more land-intensive than modern services, the negative land-supply shock should bind more strongly in manufacturing.

At the same time, the labor-supply shock relaxes scale constraints by making additional workers available in reformed cities. As a result, while the net effect is likely positive for services, it is theoretically ambiguous for manufacturing, reflecting the balance between a tighter land constraint and a looser labor constraint.

Empirically, service employment rises by 6% ($p < 0.01$) in treated prefectures after the reform (Column (7)). While a concurrent paper documents increased service-firm exit following TJAL, this is combined with higher firm entry; the net stock of active service firms rises by 4.8% (Appendix Table F.1), consistent with the employment pattern (see footnote below for detail).[20] In contrast, the estimated effects on manufacturing employment (-2.7%) and industrial output (-5.7%, Column (8)) are negative but statistically indistinguishable from zero; event-study estimates confirm parallel pre-trends for industrial output (Appendix Figure F.1). This null result is consistent with two offsetting forces, tighter land supply and looser labor supply, working in opposite directions on manufacturing, leaving the net effect small relative to within-prefecture variation. Urban employment gains are therefore absorbed primarily by the services sector.

Overall, the results sharpen the central tradeoff of rural land rights in structural

[20] In one concurrent paper, Zhao, Guo and Yu (2022) document increased service-firm exit following TJAL. Using the same firm registration data, I replicate this result but show that it coincides with higher service-firm entry, implying net expansion rather than contraction. TJAL raises entry by 8% and exit by 18%. Given that percent changes do not, by themselves, pin down the net effect on firm stocks, translating these estimates into levels using sample means implies roughly 32 additional entries and 12 additional exits per prefecture-year, an increase of about 5.6% in the annual change of service firms. Consistent with this implication, the stock of active service firms rises by 4.8%. Taken together, these firm dynamics reinforce the sectoral heterogeneity results and provide additional support for service-led urban employment growth after the reform, with potentially more severe competition.

transformation: stronger rural land rights drive structural transformation through labor reallocation into cities and nonfarm work, yet constrain transformation on the land margin by tightening urban land supply and limiting the expansion of cities and land-intensive manufacturing.

Table 6: The impact of TJAL on local economy

Dep. Var.	GDP				
	(1)	(2)	(3) Primary	(4) Secondary	(5) Tertiary
TJAL	0.003	0.003	0.021	-0.026	0.030**
	(0.018)	(0.018)	(0.015)	(0.027)	(0.013)
Prefecture FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Baseline	No	Yes	Yes	Yes	Yes
MDV	26.252	23.902	25.477	25.239	25.303
Observations	2,899	2,899	2,899	2,899	2,899
Adjusted R^2	0.987	0.987	0.986	0.974	0.993

Note: This table reports the DD estimates of TJAL's impacts on GDP. The level of observation is city-year. The specification follows: $Y_{p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \zeta_t + \eta_p + X_{p,2012} \times \zeta_t + \epsilon_{p,t}$, where $Y_{p,t}$ denotes the outcome variables for prefecture p in year t; ζ_t and η_p are year and prefecture fixed effects; $X_{p,2012} \times \zeta_t$ denotes the baseline characteristics in 2012 (i.e., the IHS transformation of GDP) interacted with year fixed effects; $\epsilon_{p,t}$ is the error term; the rest terms are defined the same as in Equation (1). Columns (1)–(2) report the effect on total GDP; Column (1) excludes baseline controls and Column (2) includes them. Columns (3)–(5) decompose GDP by sector: Column (3) primary industry (agriculture and forestry), Column (4) secondary industry (manufacturing and construction), and Column (5) tertiary industry (services). All dependent variables are in IHS transformation. Controls are defined accordingly in the specifications. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

4.5 Overall effect on local economy

In this section, I bring together the different transformation forces and evaluate their net effect on the local economy. The effect is theoretically ambiguous. The reform strengthens land rights and compensation, and improves factor allocation, in the direction that should expand GDP (Ghatak and Mookherjee, 2014; Adamopoulos and Restuccia, 2014). But it simultaneously tightens urban land supply for manufacturing, constraining the sector that produces tradable goods and benefits from trade, and potentially generating aggregate output losses (Hsieh and Moretti, 2019). Therefore, the sign of the net effect is an open empirical question.

Table 6 provides the final empirical piece: the net effect on local GDP. TJAL raises tertiary GDP by roughly 3%, and primary GDP by roughly 2%, while secondary

(manufacturing and construction) GDP falls by roughly 2.5%. The net effect on total GDP is roughly +0.3%, positive in sign though the aggregate estimate is imprecise. The services gain is the only estimate that clears conventional significance (5%); the primary and secondary estimates carry their predicted signs but lack statistical precision.

The sectoral asymmetry has a clean theoretical interpretation. On the one hand, rural-to-urban labor inflow (Section 4.4) directly expands the urban services workforce and raises local output. On the other hand, a binding land constraint limits manufacturing scale, reducing aggregate value-added. At the same time, less labor supply yet higher average productivity leads to an ambiguous 2% increase in their agricultural value-added. These competing forces produce a positive but statistically imprecise net effect on local GDP, consistent with the structural transformation process documented in Sections 4.2 to 4.4.

5 General-equilibrium analysis

The conceptual partial-equilibrium model in Appendix D establishes the mechanism for rural factor reallocation under insecure land rights: usage-dependent expropriation risk distorts households' sector choices, and securing land rights removes that distortion. This section extends those mechanisms to a general-equilibrium setting with endogenous urban production, land markets, and wages, which permits quantitative counterfactuals. The two models share the core expropriation-risk mechanism but differ in scope: the appendix model isolates rural reallocation; this section integrates the urban response and calibrates the full system to empirical moments.

5.1 Model

5.1.1 Rural production

There are N rural households. Each household i is endowed with one unit of labor, one unit of land, and an i.i.d. distributed agricultural productivity $\theta_i \sim U[0,1]$. Rural households choose between farm work and off-farm work (but not both) to maximize their total income. Let $l_{r,i}$ denote the amount of labor devoted in farm work. Following the macro-development literature (Adamopoulos and Restuccia, 2014; Restuccia et al., 2008; Britos et al., 2022), households participate in farm work ($l_{r,i} = 1$) with the following production technology

$$y_{r,i} = \theta_i t_{r,i}^{\alpha}.$$

Here, $t_{r,i}$ denotes land inputs. $\alpha \in (0,1)$ denotes the output elasticity of land. This reflects the percent change in output resulting from a 1% change in land inputs. Households choose the optimal land inputs to maximize total income from farm work

$$\pi_i(\theta_i) = \max_{t_{r,i}} \{\theta t_{r,i}^{\alpha} - R(t_{r,i} - 1)\}.$$

The optimal land input $t_{r,i}^{*}$ satisfies the condition that the marginal product of land, $\alpha\theta t_{r,i}^{\alpha-1}$, equals the land rental rate, R. Solving the household's maximization problem yields

$$t_{r,i}^{*}(\theta_i) = (\frac{\alpha\theta_i}{R})^{\frac{1}{1-\alpha}}.$$

When participating in off-farm work, households face a land-expropriation risk $\mu \in [0,1]$. If expropriation occurs, they lose their land and consequently forfeit any rental income they would have earned by leasing it to other farmers.[21] The off-farm work income equals

$$w_l + (1-\mu)R.$$

w_l is the marginal outputs of labor in the city sector. I can always define a threshold productivity $\bar{\theta}$ at which a household is indifferent between exclusively farming its own land (i.e., $t_{r,i} = 1$) and participating in off-farm work. At this threshold, the equilibrium land rental rate is $R = \alpha\bar{\theta}$; farming income is $\pi(\bar{\theta}) = \bar{\theta}$. The indifference condition can therefore be written as: $\pi(\bar{\theta}) = w_l + (1-\mu)R$. With this in mind, the threshold agricultural productivity follows

$$\bar{\theta} = \frac{w_l}{1 - \alpha + \mu\alpha}. \tag{2}$$

There are three points worth noting. First, rural households with agricultural productivity above $\bar{\theta}$ will remain in farm work, while those below will switch to off-farm work. Second, a decrease in expropriation risk (μ) raises $\bar{\theta}$, implying that more secure land rights shift labor from farm to off-farm activities–consistent with my empirical findings. Third, the proportion of rural households in off-farm work is $Pr(\theta < \bar{\theta}) = \bar{\theta}$, so the number of off-farm households is $\bar{\theta}N$, and the remaining $(1-\bar{\theta})N$ households farm. Total land expropriated for urban production is $E = \mu\bar{\theta}N$. These will be incorporated into the subsequent urban production.

[21] I do not consider compensation here. This will be discussed in the following sections.

5.1.2 Urban production

There are N_u urban households. Each is endowed with one unit of labor and h units of human capital.[22] The city sector is initially endowed with c units of land managed by the government. Similar to Ahlfeldt et al. (2015); Rossi-Hansberg and Wright (2007), I assume a constant return to scale in urban production, with the following aggregate production technology

$$Y_u = L^{\gamma} H^{\eta} T^{1-\gamma-\eta}.$$

Y_u denotes aggregate urban outputs. L, H, and T denote the aggregate labor, human capital, and land inputs respectively. Following section 5.1.1, I have explicit expressions of these inputs: $L^* = \bar{\theta}N + N_u$; $H^* = hN_u$; $T^* = c + \mu\bar{\theta}N$. Hence, the optimal labor wage level is implicitly given by (see details in Appendix section G.1)

$$w_l^* = \gamma L^{*\gamma-1} H^{*\eta} T^{*1-\gamma-\eta}.$$

Similarly, I can derive expressions for the optimal human capital and land wage (presented below):

$$w_h^* = \eta L^{*\gamma} H^{*\eta-1} T^{*1-\gamma-\eta}, \text{ and } w_t^* = (1-\gamma-\eta) \cdot L^{*\gamma} H^{*\eta} T^{*-\gamma-\eta}.$$

5.1.3 Equilibrium conditions and income

There are three equilibrium conditions for labor, land, and outputs. The market clearing conditions for labor is

$$\sum_i l_{r,i} + L = N + N_u,$$

where the total labor supply $(N + N_u)$ is allocated between farm work and off-farm work. Similarly, the total rural land endowment (N) is split between rural and urban production (expropriation)

$$\sum_i t_{r,i} + E = N.$$

While the outputs generated by labor and human capital accrue to households, the

[22] The introduction of human capital is to differentate urban native labor from rural labor. As urban labor is usually more educated, they could participate in more skilled and valued activities. Meanwhile, without agricultural productivity, urban households cannot participate in farm work.

returns on land are retained as government income, $y_{gov} = w_t^*(c+E)$. I assume the government solely as a distributor of land outputs and I relax this in section 5.2.4. For now, outputs from land are shared between urban and rural households as transfers: a fraction of $\kappa \in [0,1]$ is returned to rural households. Let b denote the city-reserved benefits; the urban-rural transfer balance is

$$(c+E)w_t^* = \kappa(c+E)w_t^* + b.$$

The average rural and urban household income therefore are

$$y_{rural} = \bar{\theta}\pi(\bar{\theta}) + (1-\bar{\theta})y_{agri} + \frac{\kappa(c+E)w_t^*}{N}, \text{ and } y_{urban} = w_l^* + hw_h^* + \frac{b}{N_u}.$$

$y_{agri} = \frac{(1-\mu\bar{\theta})^{\alpha}}{(1-\bar{\theta})(\int_{\bar{\theta}}^{1}\theta^{\frac{1}{1-\alpha}}d\theta)^{\alpha-1}}$ is the average farm work outputs per farming household. Detailed derivations can be found in Appendix section G.2.

5.2 Parameterization and counterfactual analysis

In this section, I discuss how the parameters of the model are determined. Then, I conduct a series of counterfactual analyses to quantify the impact of more secure rural land rights on rural employment decision and household income.

5.2.1 Parameterisation

I normalize the rural population to one for simplicity (Gai et al., 2025; Chen, 2017). Since the key is the relative urban-rural population size–and China's urbanization rate was approximately 50% in 2010–I also set set $N_u = 1$ to reflect this parity.[23] I set $h = 2$ so that the aggregate labor endowment equals the aggregate human capital. Given a rural land endowment of 1, I set the urban land endowment $c = 0.27$, reflecting the ratio of urban and transportation land (36,681 thousand hectares) to arable land (135,385 thousand hectares) at that time.[24]

[23] See Communiqué of the National Bureau of Statistics of People's Republic of China on Major Figures of the 2010 Population Census[1] (No. 1) for details (https://www.stats.gov.cn/english/NewsEvents/201104/t20110428_26449.html).

[24] See Communiqué on the Main Data Results of the Second National Land Survey for Details (http://www.mlr.gov.cn/zwgk/zytz/201312/t20131230_1298865.htm).

Table 7: Parameterising the model

Parameter	Meaning	Value
N	Population size of rural area	1
N_u	Population size of urban area	1
h	Human capital per urban household	2
c	Land endowment of urban area	0.27
α	Land elasticity in rural production	0.35
γ	Labor elasticity in urban production	0.35
η	Human capital elasticity in urban production	0.55
κ	Share of government land income redistributed to rural communities	0.3
μ	The probability of expropriation	0.15

Note: This table reports the values of parameters used for the counterfactual analysis.

For the elasticity of land in rural production, I take the values directly from the macro-development literature and set a mid value of $\alpha = 0.35$ (Adamopoulos et al., 2022; Adamopoulos and Restuccia, 2014; Restuccia et al., 2008). For urban production, the literature identifies human capital as a dominant factor (Moretti, 2004). In China's 2010, a 10-percentage-point increase in the share of adults holding a bachelor's degree leads to 52–67% increase in average wages (Chauvin et al., 2017). Relative to an average of 9 percentage points in 2010,[25] this implies an elasticity of wages with respect to the human capital stock of approximately 0.47–0.60. My own city-level production function estimates yield an education elasticity of 0.587–0.671 (Appendix Table G.1), of which $\eta = 0.55$ is a conservative mid-value. For urban land, the literature suggests it accounts for 0.03–0.2 of outputs (Ahlfeldt et al., 2015; Valentinyi and Herrendorf, 2008). Accordingly, I set $\eta = 0.55$ and assign a value of 0.10 to land; the residual labor share is $\gamma = 0.35$, which is also consistent with previous research (Cao and Birchenall, 2013; Chow, 1993).

Table 8: Model fit

Moment	Target	Model
$\bar{\theta}$	0.526	0.503
G	0.713	0.687

Note: This table reports model fitting results. I report the target and simulated gap without government transfer.

[25]The statistics of the share of adults with bachelor's degree or above is based on Communiqué of the National Bureau of Statistics of People's Republic of China on Major Figures of the 2010 Population Census.

I set $\kappa = 0.3$ as the baseline, representing the share of government income redistributed to rural communities. The regulatory framework mandates multiple rural-directed expenditure streams from land income, including agricultural land development spending (no less than 15% of land revenue), direct compensation, and additional rural support spending.[26] These mandatory obligations make $\kappa = 0.3$ institutionally plausible. Finally, I take the baseline expropriation probability for off-farm rural households to be $\mu = 0.15$, based on NFPS data (as shown in Figure 1). This also serves as the key policy parameter.

I report empirical moments alongside their model counterparts in Table 8. First key metrics is the share of rural households in off-farm work. The model predicts an off-farm share of 0.503, close to the data value of 0.526. While simulated levels of income are not directly comparable, relative incomes are. I define the urban-rural income gap as $G = \frac{y_{urban} - y_{rural}}{y_{rural}}$. The model yields $G = 0.687$, close to the empirical counterpart of 0.713. Overall, the model closely reproduces the observed moments.

5.2.2 Comparative statics of secure rural land rights

Figure 2 presents simulated results as the expropriation probability (μ) varies. Lowering μ increases the share of rural households in off-farm work (Panel 2a), consistent with the argument that insecure land rights drive rural labor oversupply; accordingly, urban wages decline (Panel 2b).

Farm income per farming household rises (Panel 2c) because the remaining farmers are, on average, more productive and cultivate larger plots. When μ falls from 1 to 0, rural income increases by 25.6% (Panel 2d), urban income decreases by 8.1% (Panel 2e), and the urban-rural income gap narrows by 49.6% (Panel 2g). Government income rises monotonically with μ (Panel 2f): higher expropriation risk expands the urban land base and urban output, increasing total land-generated revenue from 0.149 at $\mu = 0$ to 0.158 at $\mu = 1$. This property motivates the government behavior analysis in Section 5.2.4. Transfers redistribute government income without changing factor allocation:κ does not enter the migration equilibrium, so the off-farm participation rate and factor prices are unaffected by the transfer share (Panel2h). This highlights an important asymmetry: while transfers can reduce the income gap mechanically, they cannot correct the distorted migration decisions caused by expropriation risk. Only institutional reform (by lowering μ) achieves both allocative efficiency and distributional equity simultaneously.

[26] See Ministry of Finance (2004) and Ministry of Finance (2006) for details.

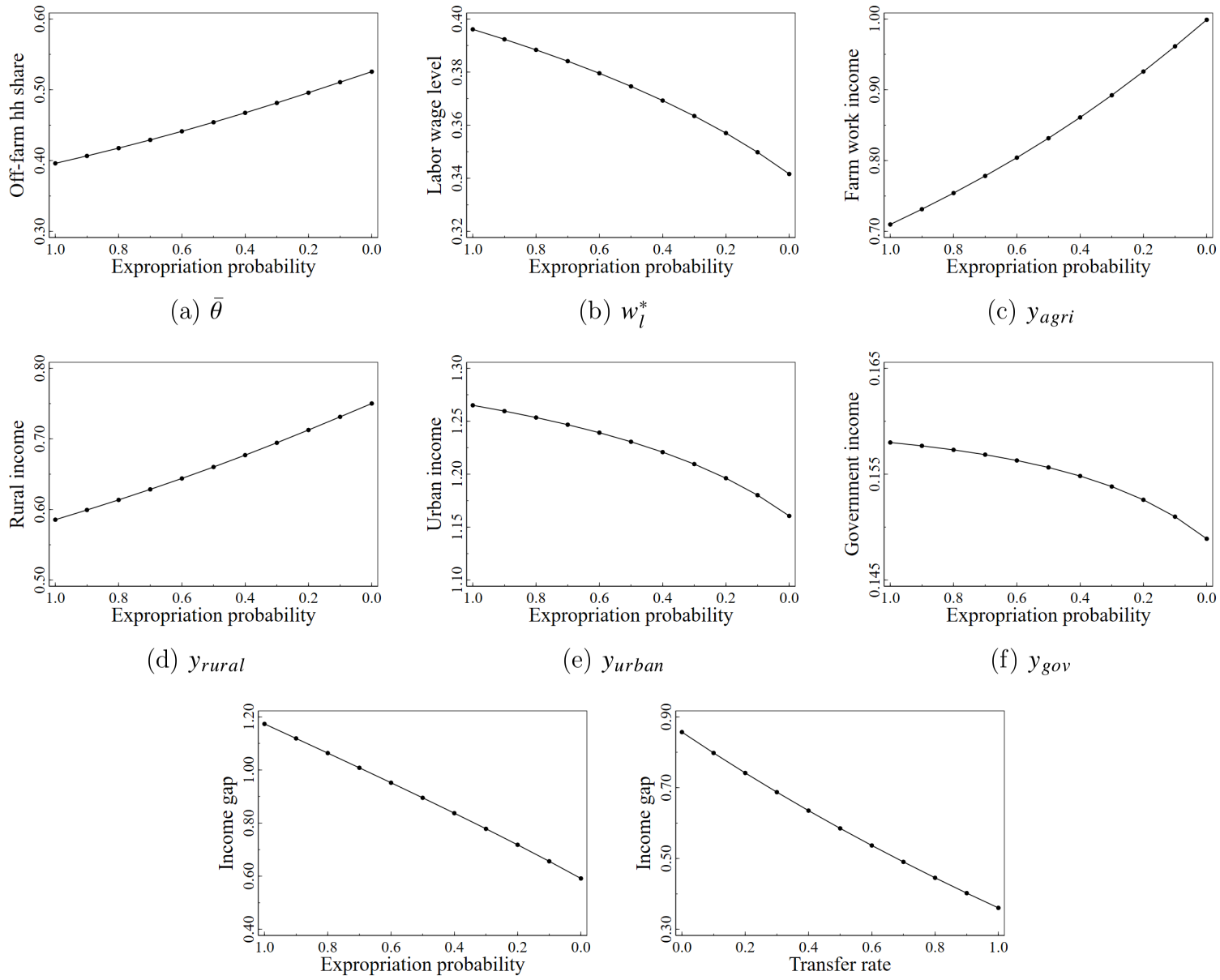


Figure 2: Simulated impacts of secure rural land rights

Note: This figure presents the simulation results. I present the share of off-farm rural households ($\bar{\theta}$), urban labor wage level (w_l^*), farm work income per farming household (y_{agri}), average income of rural households (y_{rural}), average income of urban households (y_{urban}), government income (y_{gov}), urban-rural income gap (G).

5.2.3 Distributional effects of judicial independence

An unintended general-equilibrium benefit of TJAL is the narrowing of urban-rural income gap due to factor reallocation. Empirically, pre-TJAL urban income averages 87,540.46 yuan versus 51,109.52 yuan in rural areas (a gap of 36,430.94 yuan). A 7.3% rural income gain (+3,730.99 yuan) coupled with a modest urban decline (predicted post-TJAL urban income: 86,402.43 yuan) implies roughly a 10.24 percentage-point reduction in the pre-TJAL gap.

Quantitatively, the counterfactual with zero expropriation risk predicts about a 2.2 percentage-point (5%) increase in the off-farm share among rural households and a 9.6 percentage-point reduction in the urban-rural income gap (Figure 3). These magnitudes align closely with the reduced-form estimates and confirm TJAL's distributional effects operating through stronger rural land rights. Appendix Table G.2 confirms robustness: varying each structural parameter by ±20% around its baseline, the increase in off-farm participation ranges from 0.012 to 0.039 and the income gap narrowing from 0.08 to 0.12, preserving both the direction and approximate magnitude of the baseline results.

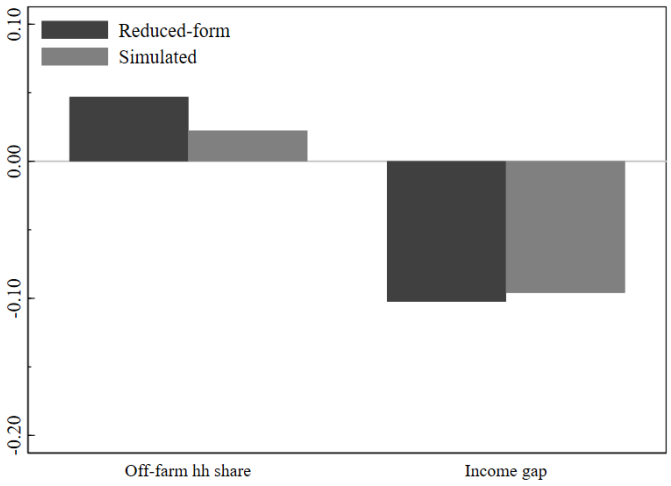


Figure 3: Comparison between reduced-form and simulated TJAL effects

Note: This figure presents the comparison between reduced-form and simulated effects of more secure rural land rights.

This distributional gain cannot be replicated by fiscal redistribution alone. Achieving the same narrowing of the income gap via transfers would require raising the transfer share from the baseline 0.30 to 0.49, a 19-percentage-point increase that purchases no efficiency gain in labor allocation. Institutional reform thus dominates redistribution: it achieves equivalent equity improvements alongside better factor allocation.

To map this between-group change into overall inequality, note that the literature identifies the urban-rural divide as a major contributor, though estimates vary widely for early-2010s China. Using the Chinese Household Income Project for 2007 and 2013, Wan et al. (2018) attribute 50.06% and 29.99% of overall inequality to the urban-rural gap, close to Luo (2017)'s 35% (2013). Applying the same method to other surveys, Wan et al. (2018) report 19.34%/16.58% (Chinese General Social Survey 2008/2013), 10.86%/4.43% (China Health and Retirement Longitudinal Study 2011/2013), and 5.36%/5.96% (China Health and Nutrition Survey 2006/2011). Given this range, a back-of-the-envelope mapping of the TJAL-induced 9.6 percentage-point reduction in the urban-rural income gap implies a roughly 0.5–5 percentage-point decline in overall inequality.

5.2.4 Role of government

In this section, I endogenize government behavior and consider two polar types: an extractive government that maximizes fiscal take ($\max_{\mu} y_{gov}$); an egalitarian government that minimizes the urban-rural income gap ($\min_{\mu,\kappa} G$). The policy instruments are the expropriation probability μ and the transfer share κ.

For the extractive type, κ does not affect total land-generated output, so the problem reduces to choosing μ. Solving yields a corner solution $\mu = 1$ and government revenue $y_{gov} = 0.158$, consistent with Panel 2f; in effect, an extractive authority expropriates land from all households that move off-farm.

For the egalitarian type, the government can lower the gap either directly by reducing μ or indirectly by pairing a higher μ with redistribution via κ. The simulated results suggest the optimum is $\mu = 0$ and $\kappa = 1$, yielding a gap of$G = 0.291$, a corner solution, since G is strictly decreasing in both lower μ and higher κ. Any government with mixed objectives will choose an interior μ between these extremes. The TJAL reform acts as an external constraint on local governments, preventing them from sustaining high μ and shifting the equilibrium away from the extractive optimum.

6 Conclusion

This paper asks whether independent courts can serve as a development lever in settings where insecure rural land rights stem from local-government predation in pursuit of urban expansion. Exploiting prefecture-year variation in China's TJAL reform, which established inter-regional jurisdiction for administrative land-related cases, I

show that stronger rural land rights improve rural factor allocation: off-farm employment rises, expropriation-induced migration falls, land rental markets deepen, and agriculture becomes more mechanized, collectively leading to higher rural household income. However, consistent with the tradeoff of rural land rights in structural transformation, this strengthening generates a positive-labor and negative-land shock to the city sector, restricting city expansion and unevenly promoting less land-intensive industries. A two-sector general-equilibrium model reproduces these transformation patterns and attributes factor reallocation to lower expropriation risk. Counterfactual analysis replicates the unintended city-village distributional effects, narrowing the income gap between urban and rural households. The absence of expropriation risk, rather than urban-rural transfer, is the key force mitigating economic inequality.

These findings position independent courts as a development institution in their own right, operating through a distinct channel from the conventional fiscal and regulatory levers yet comparable to them in quantitative force. By raising the cost of local-government expropriation, an insulated judiciary alters the rules under which the developmental state interacts with citizens over property, securing tenure without requiring tax-financed transfers or centrally imposed regulation. What makes the judicial channel economically distinct is that it operates on the risk attached to factor ownership rather than on after-the-fact redistribution: by lowering expropriation risk, it unlocks reallocation of labor and land that transfers cannot induce, delivering the efficiency gain alongside the distributional one. While China's context is specific, the institutional logic generalizes wherever rapid urbanization outpaces protection of rural land rights: a credible judicial check on local-government predation widens the development-policy menu.

Several open questions follow from these results and motivate a broader research agenda. First, while this paper documents the reallocation in labor and land markets, less is known about the effects on productivity and innovation: whether tighter urban land constraints slow entry and agglomeration in land-intensive activities, and whether increased rural security encourages longer-horizon human-capital and entrepreneurship investments. Second, the welfare implications likely depend on general-equilibrium incidence: who bears the costs of higher urban land prices and lower native wages, how these effects propagate through housing markets, local public finance, and intra-household bargaining, and whether compensating policies can preserve efficiency gains and manage distributional effects. Third, judicial reforms may induce strategic adaptation by local governments and firms (e.g., shifting from expropriation to alternative instruments of land acquisition or regulation), raising questions about institutional

substitution and long-run compliance. This paper establishes that judicial independence shapes structural transformation not only through the well-studied channel of contract enforcement, but through the direct resolution of state-farmer land conflicts, with aggregate distributional consequences that rival formal transfer programs.

Bibliography

Abadie, A., Athey, S., Imbens, G. W. and Wooldridge, J. M. (2023), 'When should you adjust standard errors for clustering?', *The Quarterly Journal of Economics* **138**(1), 1–35.

Acemoglu, D. and Guerrieri, V. (2008), 'Capital deepening and nonbalanced economic growth', *Journal of political Economy* **116**(3), 467–498.

Acemoglu, D. and Johnson, S. (2005), 'Unbundling institutions', *Journal of political Economy* **113**(5), 949–995.

Adamopoulos, T., Brandt, L., Leight, J. and Restuccia, D. (2022), 'Misallocation, selection, and productivity: A quantitative analysis with panel data from china', *Econometrica* **90**(3), 1261–1282.

Adamopoulos, T. and Restuccia, D. (2014), 'The size distribution of farms and international productivity differences', *American Economic Review* **104**(6), 1667–1697.

Ahlfeldt, G. M., Redding, S. J., Sturm, D. M. and Wolf, N. (2015), 'The economics of density: Evidence from the berlin wall', *Econometrica* **83**(6), 2127–2189.

Albertus, M. and Menaldo, V. (2012), 'Dictators as founding fathers? the role of constitutions under autocracy', *Economics & Politics* **24**(3), 279–306.

Amirapu, A. (2021), 'Justice delayed is growth denied: The effect of slow courts on relationship-specific industries in india', *Economic Development and Cultural Change* **70**(1), 415–451.
URL: *https://www.journals.uchicago.edu/doi/abs/10.1086/711171*

Banerjee, A. V., Gertler, P. J. and Ghatak, M. (2002), 'Empowerment and efficiency: Tenancy reform in west bengal', *Journal of political economy* **110**(2), 239–280.

Behrens, K., Mayneris, F. and Biéda, T. N. (2025), 'How densely do manufacturing establishments occupy land?', *Review of Economics and Statistics* pp. 1–45.

Benjamin, D., Brandt, L. and Giles, J. (2005), 'The evolution of income inequality in rural china', *Economic Development and Cultural Change* **53**(4), 769–824.

Besley, T. (1995), 'Property rights and investment incentives: Theory and evidence from ghana', *Journal of political Economy* **103**(5), 903–937.

Besley, T. and Ghatak, M. (2010), Property rights and economic development, *in* D. Rodrik and M. Rosenzweig, eds, 'Handbook of Development Economics', Vol. 5, Elsevier, pp. 4525–4595.

Borusyak, K., Jaravel, X. and Spiess, J. (2024), 'Revisiting event-study designs: Robust and efficient estimation', *Review of Economic Studies* **91**(6), 3253–3285.

Britos, B., Hernandez, M. A., Robles, M. and Trupkin, D. R. (2022), 'Land market distortions and aggregate agricultural productivity: Evidence from guatemala', *Journal of Development Economics* **155**.

Brooks, L. and Liscow, Z. (2023), 'Infrastructure costs', *American Economic Journal: Applied Economics* **15**(2), 1–30.

Buera, F. J., Kaboski, J. P. and Shin, Y. (2011), 'Finance and development: A tale of two sectors', *American Economic Review* **101**(5), 1964–2002.

Bustos, P., Caprettini, B. and Ponticelli, J. (2016), 'Agricultural productivity and structural transformation: Evidence from brazil', *American Economic Review* **106**(6), 1320–65.
URL: *https://www.aeaweb.org/articles?id=10.1257/aer.20131061*

Callaway, B. and Sant'Anna, P. H. C. (2021), 'Difference-in-differences with multiple time periods', *Journal of Econometrics* **225**(2), 200–230.

Cao, G., Liu, C. and Zhou, L.-A. (2023), 'Suing the government under weak rule of law: Evidence from administrative litigation reform in china', *Journal of Public Economics* **222**.

Cao, K. H. and Birchenall, J. A. (2013), 'Agricultural productivity, structural change, and economic growth in post-reform china', *Journal of Development Economics* **104**, 165–180.

Carlitz, R., Morjaria, A., Mueller, J. and Osafo-Kwaako, P. (2024), 'State building in a diverse society', *Review of Economic Studies* .

Castañeda Dower, P. and Pfutze, T. (2020), 'Land titles and violent conflict in rural mexico', *Journal of Development Economics* **144**.

Chari, A., Liu, E. M., Wang, S.-Y., Wang, Y. and Schoenberg, U. (2021), 'Property rights, land misallocation, and agricultural efficiency in china', *The Review of Economic Studies* **88**(4), 1831–1862.

Chauvin, J. P., Glaeser, E., Ma, Y. and Tobio, K. (2017), 'What is different about urbanization in rich and poor countries? cities in brazil, china, india and the united states', *Journal of Urban Economics* **98**, 17–49.

Chemin, M. (2009*a*), 'Do judiciaries matter for development? evidence from india', *Journal of Comparative Economics* **37**(2), 230–250.

Chemin, M. (2009*b*), 'The impact of the judiciary on entrepreneurship: Evaluation of pakistan's "access to justice programme"', *Journal of Public Economics* **93**(1-2), 114–125.

Chemin, M. (2020), 'Judicial efficiency and firm productivity: Evidence from a world database of judicial reforms', *The Review of Economics and Statistics* **102**(1), 49–64.

Chemin, M. (2021), 'Can judiciaries constrain executive power? evidence from judicial reforms', *Journal of Public Economics* **199**.

Chen, C. (2017), 'Untitled land, occupational choice, and agricultural productivity', *American Economic Journal: Macroeconomics* **9**(4), 91–121.

Chen, C., Restuccia, D. and Santaeulàlia-Llopis, R. (2023), 'Land misallocation and productivity', *American Economic Journal: Macroeconomics* **15**(2), 441–465.

Chen, J. and Roth, J. (2024), 'Logs with zeros? some problems and solutions', *The Quarterly Journal of Economics* **139**(2), 891–936.

Chen, T. and Kung, J. K.-s. (2019), 'Busting the "princelings": The campaign against corruption in china's primary land market*', *The Quarterly Journal of Economics* **134**(1), 185–226.

Chow, G. C. (1993), 'Capital formation and economic growth in china', *The Quarterly Journal of Economics* **108**(3), 809–842.

Combes, P.-P., Duranton, G. and Gobillon, L. (2019), 'The costs of agglomeration: House and land prices in french cities', *The Review of Economic Studies* **86**(4), 1556–1589.

Davis, D. R. and Dingel, J. I. (2019), 'A spatial knowledge economy', *American Economic Review* **109**(1), 153–170.

Davis, D. R. and Dingel, J. I. (2020), 'The comparative advantage of cities', *Journal of International Economics* **123**, 103291.

de Brauw, A. and Mueller, V. (2012), 'Do limitations in land rights transferability influence mobility rates in ethiopia?', *Journal of African Economies* **21**(4), 548–579.

de Chaisemartin, C. and Ramirez-Cuellar, J. (2024), 'At what level should one cluster standard errors in paired and small-strata experiments?', *American Economic Journal: Applied Economics* **16**(1), 193–212.

de Janvry, A., Emerick, K., Gonzalez-Navarro, M. and Sadoulet, E. (2015), 'Delinking land rights from land use: Certification and migration in mexico', *American Economic Review* **105**(10), 3125–3149.

De Soto, H. (2000), *The Mystery of Capital: Why Capitalism Triumphs in the West and Fails Everywhere Else*, Basic Books, New York.

Do, Q. and Iyer, L. (2008), 'Land titling and rural transition in vietnam', *Economic Development and Cultural Change* **56**(3), 531–579.

Duarte, M. and Restuccia, D. (2010), 'The role of the structural transformation in aggregate productivity', *The quarterly journal of economics* **125**(1), 129–173.

Fang, H., Li, M. and Wu, Z. (2022), Tournament-style political competition and local

protectionism: theory and evidence from china, Report, National Bureau of Economic Research.

Felstiner, W. L., Abel, R. L. and Sarat, A. (1980), 'The emergence and transformation of disputes: Naming, blaming, claiming', *Law and Society Review* **15**(3), 631.

Field, E. (2007), 'Entitled to work: Urban property rights and labor supply in peru', *The Quarterly Journal of Economics* **122**(4), 1561–1602.

Gai, Q., Guo, N., Li, B., Shi, Q. and Zhu, X. (2025), 'Rural pensions, labor reallocation, and aggregate income: An empirical and quantitative analysis of china', *Econometrica* **Forthcoming**.

Galiani, S. and Schargrodsky, E. (2010), 'Property rights for the poor: Effects of land titling', *Journal of Public Economics* **94**(9-10), 700–729.

Ghatak, M. and Mookherjee, D. (2014), 'Land acquisition for industrialization and compensation of displaced farmers', *Journal of Development Economics* **110**, 303–312.

Giles, J. and Mu, R. (2017), 'Village political economy, land tenure insecurity, and the rural to urban migration decision: Evidence from china', *American Journal of Agricultural Economics* **100**(2), 521–544.

Goldstein, M., Houngbedji, K., Kondylis, F., O'Sullivan, M. and Selod, H. (2018), 'Formalization without certification? experimental evidence on property rights and investment', *Journal of Development Economics* **132**, 57–74.

Goldstein, M. and Udry, C. (2008), 'The profits of power: Land rights and agricultural investment in ghana', *Journal of Political Economy* **116**(6), 981–1022.

Gollin, D., Lagakos, D. and Waugh, M. E. (2014), 'Agricultural productivity differences across countries', *American Economic Review* **104**(5), 165–170.

Gollin, D., Parente, S. and Rogerson, R. (2002), 'The role of agriculture in development', *American Economic Review* **92**(2), 160–164.

Goodman-Bacon, A. (2021), 'Difference-in-differences with variation in treatment timing', *Journal of econometrics* **225**(2), 254–277.

Gottlieb, C. and Grobovšek, J. (2019), 'Communal land and agricultural productivity', *Journal of Development Economics* **138**, 135–152.

He, Z., Nelson, S. T., Su, Y., Zhang, A. L. and Zhang, F. (2022), Zoning for profits: How public finance shapes land supply in china, Report, National Bureau of Economic Research.

Holland, A. C. (2022), 'Roadblocks: How property rights undermine development in colombia', *American Journal of Political Science* **67**(3), 639–655.

Hornbeck, R. (2010), 'Barbed wire: Property rights and agricultural development', *The Quarterly Journal of Economics* **125**(2), 767–810.

Hsieh, C.-T. and Klenow, P. J. (2009), 'Misallocation and manufacturing tfp in china and india', *The Quarterly Journal of Economics* **124**(4), 1403–1448.

Hsieh, C.-T. and Moretti, E. (2019), 'Housing constraints and spatial misallocation', *American Economic Journal: Macroeconomics* **11**(2), 1–39.

Huang, K., Guo, J. and Zhao, D. (2024), 'Positive rainfall shocks, overoptimism, and agricultural inefficiency in china', *Journal of the Association of Environmental and Resource Economists* **11**(4), 887–919.

Jacoby, H. G., Li, G. and Rozelle, S. (2002), 'Hazards of expropriation: tenure insecurity and investment in rural china', *American Economic Review* **92**(5), 1420–1447.

Jacoby, H. G. and Minten, B. (2007), 'Is land titling in sub-saharan africa cost-effective? evidence from madagascar', *The World Bank Economic Review* **21**(3), 461–485.

Laeven, L. and Woodruff, C. (2007), 'The quality of the legal system, firm ownership, and firm size', *The Review of Economics and Statistics* **89**(4), 601–614.

Lambais, G. and Sigstad, H. (2023), 'Judicial subversion: The effects of political power on court outcomes', *Journal of Public Economics* **217**, 104788.
URL: *https://www.sciencedirect.com/science/article/pii/S0047272722001906*

Lewis, W. A. (1955), *The Theory of Economic Growth*, Allen & Unwin, London.

Lin, J. Y. (1992), 'Rural reforms and agricultural growth in china', *American Economic Review* **82**(1), 34–51.

Liu, E., Lu, Y., Peng, W. and Wang, S. (2022), 'Judicial independence, local protectionism, and economic integration: Evidence from china', *NBER Working Paper 30432* .

Liu, S., Ma, S., Yin, L. and Zhu, J. (2023), 'Land titling, human capital misallocation, and agricultural productivity in china', *Journal of Development Economics* **165**.

Llanos, M., Tibi Weber, C., Heyl, C. and Stroh, A. (2015), 'Informal interference in the judiciary in new democracies: a comparison of six african and latin american cases', *Democratization* **23**(7), 1236–1253.

Luo, C. (2017), 'Urban-rural income gap and its effects on national income inequality', *Studies in Labor Economics* **5**(1), 21–47.

Ma, S. and Mu, R. (2020), 'Forced off the farm? farmers' labor allocation response to land requisition in china', *World Development* **132**.

McCaig, B. and Pavcnik, N. (2018), 'Export markets and labor allocation in a low-income country', *American Economic Review* **108**(7), 1899–1941.
URL: *https://www.aeaweb.org/articles?id=10.1257/aer.20141096*

Mehmood, S. (2022), 'The impact of presidential appointment of judges: Montesquieu or the federalists?', *American Economic Journal: Applied Economics* **14**(4), 411–445.

Mehmood, S. and Ali, B. (2024), 'Judicial capture', *The Economic Journal* **134**(659), 1287–1301.

Michelson, E. (2018), 'Village dispute mediation in china, 2002–10: an enduring institution amid rural change', *Asian Journal of Law and Society* **5**(2), 433–452.

Mitrunen, M. (2025), 'War reparations, structural change, and intergenerational mobility', *The Quarterly Journal of Economics* **140**(1), 521–584.

Moretti, E. (2004), 'Estimating the social return to higher education: evidence from longitudinal and repeated cross-sectional data', *Journal of Econometrics* **121**(1-2), 175–212.

Munshi, K. and Rosenzweig, M. R. (2016), 'Networks and misallocation: Insurance, migration, and the rural-urban wage gap', *American Economic Review* **106**(1), 46–98.

Ngai, L. R. and Pissarides, C. A. (2007), 'Structural change in a multisector model of growth', *American economic review* **97**(1), 429–443.

Ngai, L. R., Pissarides, C. A. and Wang, J. (2019), 'China's mobility barriers and employment allocations', *Journal of the European Economic Association* **17**(5), 1617–1653.

Nunn, N. and Puga, D. (2012), 'Ruggedness: The blessing of bad geography in africa', *Review of Economics and Statistics* **94**(1), 20–36.

Rambachan, A. and Roth, J. (2023), 'A more credible approach to parallel trends', *Review of Economic Studies* **90**(5), 2555–2591.

Restuccia, D. and Rogerson, R. (2017), 'The causes and costs of misallocation', *Journal of Economic Perspectives* **31**(3), 151–174.

Restuccia, D., Yang, D. T. and Zhu, X. (2008), 'Agriculture and aggregate productivity: A quantitative cross-country analysis', *Journal of Monetary Economics* **55**(2), 234–250.

Rossi-Hansberg, E. and Wright, M. L. (2007), 'Urban structure and growth', *The Review of Economic Studies* **74**(2), 597–624.

Ruan, J. and Wang, P. (2022), 'Elite capture and corruption: The influence of elite collusion on village elections and rural land development in china', *The China Quarterly* **253**, 107–122.

Ríos-Figueroa, J. and Aguilar, P. (2017), 'Justice institutions in autocracies: a framework for analysis', *Democratization* **25**(1), 1–18.

Santaeulàlia-Llopis, R. and Zheng, Y. (2018), 'The price of growth: Consumption insurance in china 1989–2009', *American Economic Journal: Macroeconomics* **10**(4), 1–35.

Sargeson, S. (2013), 'Violence as development: land expropriation and china's urbanization', *Journal of Peasant Studies* **40**(6), 1063–1085.

Solomon, P. H. (2010), 'Authoritarian legality and informal practices: Judges, lawyers and the state in russia and china', *Communist and Post-Communist Studies* **43**(4), 351–362.

Song, Z., Storesletten, K. and Zilibotti, F. (2011), 'Growing like china', *American Economic Review* **101**(1), 196–233.

Sun, L. and Abraham, S. (2021), 'Estimating dynamic treatment effects in event studies with heterogeneous treatment effects', *Journal of Econometrics* **225**(2), 175–199.

Tang, T., Li, Z., Ni, J. and Yuan, J. (2020), 'Land costs, government intervention, and migration of firms: The case of china', *China Economic Review* **64**, 101560.

Tombe, T. and Zhu, X. (2019), 'Trade, migration, and productivity: A quantitative analysis of china', *American Economic Review* **109**(5), 1843–1872.

Valentinyi, A. and Herrendorf, B. (2008), 'Measuring factor income shares at the sectoral level', *Review of Economic Dynamics* **11**(4), 820–835.

Wan, G., Wu, T. and Zhang, Y. (2018), 'The decline of income inequality in china: Assessments and explanations', *Asian Economic Papers* **17**(3), 115–140.

Wu, Y., Zhang, X., Skitmore, M., Song, Y. and Hui, E. C. (2014), 'Industrial land price and its impact on urban growth: A chinese case study', *Land use policy* **36**, 199–209.

Zhang, P., Yang, X., Chen, H. and Zhao, S. (2023), 'Matching relationship between urban service industry land expansion and economy growth in china', *Land* **12**(6), 1139.

Zhao, A., Huang, J., Ploegmakers, H., Lan, J., van der Krabben, E. and Ma, X. (2022), 'Can land prices be used to curb urban industrial land expansion? an explanation from the perspective of substitutability of land in production', *International Journal of Urban Sciences* **26**(4), 651–671.

Zhao, D., Guo, J. and Yu, L. (2022), 'How could stronger property right protection undermine economic development? evidence from china', *SSRN Working Paper* .

Zhao, D., Guo, J., Yu, S. and Yu, L. (2024), 'Tradeoff between local protection and public sector performance: Lessons from judicial fiscal centralization', *Journal of Economic Behavior & Organization* **220**, 254–278.

Zhao, D., Guo, J., Zou, H. and Song, Z. (2022), 'From price to gain: The evolution of household income volatility and consumption insurance in urban china', *China & World Economy* **30**(6), 113–136.

Zhao, D., Yu, A. and Guo, J. (2022), 'Judicial institutions, local protection and market segmentation: Evidence from the establishment of interprovincial circuit tribunals in china', *China Economic Review* **75**.

Zhou, L.-a. (2007), 'Governing china's local officials: An analysis of promotion tournament model', *Economic Research Journal* **7**(36), 36–50.

Appendix

A Additional information for institutional background and data

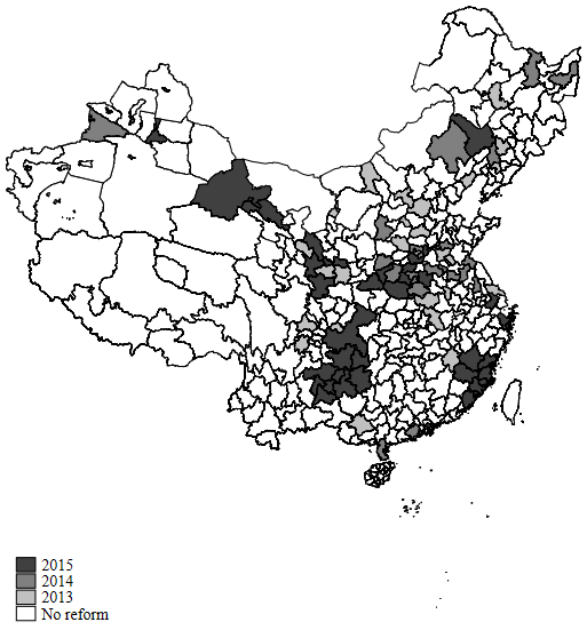
A.1 Correlation between off-farm work and expropriation

Table A.1: Correlation between off-farm work and land expropriation

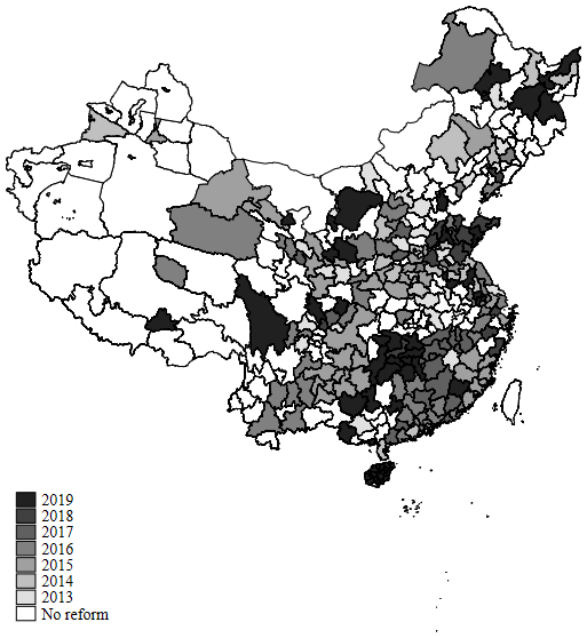
Dep. Var.	Dummy=1 if expropriated	
	(1)	(2)
Dummy=1 if off-farm	0.019**	0.020**
work	(0.009)	(0.010)
Year FE	Yes	Yes
Household FE	Yes	Yes
Controls	No	Yes
Observations	149,042	149,042
Adjusted R^2	0.419	0.426

Note: This table reports the correlation between off-farm work and expropriation probability. The level of observation is the household-year. The dependent variable is a dummy that equals one if a household in a certain year is expropriated. I include year fixed effects, household fixed effects in Column (1), and add household-level controls and city-level controls in Column (2), as described in Equation (1). MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

A.2 The staggered rollout of TJAL



(a) Before 2016



(b) All years

Figure A.1: The staggered rollout of TJAL

Note: This figure presents the staggered rollout of TJAL at the prefecture level.

A.3 Examples of land administrative litigation

A.3.1 Kong Qingfeng vs. Sishui County Government (2011)

Basic information. Case Number: Not disclosed in Supreme People's Court's published summary; Court: Jining Intermediate People's Court; Date: 2011-04-06; Location: Sishui County, Jining, Shandong; Parties: Kong Qingfeng (homeowner) vs. Sishui County People's Government; Source: Supreme People's Court "Ten Typical Expropriation/Demolition Cases" (Supreme People's Court, 2014).

Facts. The county government issued an expropriation decision covering two blocks and attached a compensation plan. The plan effectively priced compensation/settlement for the main house using an preferential price linked to discounted resettlement housing rather than the market price of comparable local housing. Kong challenged the legality of the expropriation decision and its compensation plan. The dispute arose after the county issued a formal expropriation decision; the core conflict was whether the compensation reflected 'fair compensation' rather than a low administrative standard.

Judgment. The court held that the principle of 'fair compensation' under the State-owned Land House Expropriation & Compensation Regulation requires that compensation not be lower than the market price of comparable real estate on the announcement date. Because the plan's preferential pricing was clearly below market and produced an unfair outcome, the court revoked the county's expropriation decision. No appeal was reported in the SPC summary.

Significance. This case illustrates substantive (not only procedural) judicial review in expropriation disputes: courts may invalidate even large-scope government expropriation decisions when compensation standards are plainly inconsistent with statutory 'market price' requirements. Useful for showing how courts can police under-compensation as a key driver of villager–government conflict.

A.3.2 He Gang vs. Huaiyin District Government (2012)

Basic information. Case Number: Not disclosed in Supreme People's Court's published summary; Court: Huaiyin District People's Court (first instance); Date: 2012-06-14; Location: Huai'an, Jiangsu; Parties: He Gang (homeowner) vs. Huai'an Huaiyin District People's Government; Source: Supreme People's Court "Ten Typical Expropriation/Demolition Cases" (Supreme People's Court, 2014).

Facts. After a district announced an expropriation project, He Gang indicated he would choose property exchange. Negotiations failed, and the district issued a formal compensation decision that unilaterally imposed cash compensation and set a

short relocation deadline. He challenged the decision, arguing it violated his statutory right to choose the compensation method. A key evidentiary issue was whether the household had clearly chosen property exchange (rather than cash), and whether the government could unilaterally override that choice.

Judgment. The court found the evidence showed He had chosen property exchange, yet the challenged decision imposed cash compensation. Because the relevant regulation explicitly grants the expropriated party the choice between cash compensation and property exchange, the court revoked the district's compensation decision. No appeal was reported in the SPC summary.

Significance. This case shows courts enforcing a concrete 'micro-right' that frequently triggers disputes in practice: the government cannot convert a party's property-exchange preference into cash compensation through a unilateral decision. This case is a clean example of how judicial review targets a specific administrative defect that meaningfully affects bargaining power.

A.3.3 Ye Chengsheng, Ye Chengchang, and Ye Chengfa vs. Renhua County Government (2013)

Basic information. Case Number: Not disclosed in Supreme People's Court's published summary; Court: Shaoguan Intermediate People's Court; Date: 2013-07-12; Location: Renhua County, Shaoguan, Guangdong; Parties: Ye Chengsheng et al. (villagers) vs. Renhua County People's Government; Source: Supreme People's Court "Ten Typical Expropriation/Demolition Cases" (Supreme People's Court, 2014).

Facts. For an industrial-base project, the county planned to expropriate part of a villagers' group's land. The plaintiffs' houses sat within the planned expropriation footprint and were treated by the county as illegal structures because they lacked required approvals/certificates. Despite earlier notices and communications, the county organized a forced demolition at dawn without issuing a formal forced-demolition notice and without posting an announcement requiring self-demolition within a deadline. The demolition was carried out without completing the statutory 'apply-to-court-for-enforcement' step, a recurring procedural flashpoint in rural demolition disputes.

Judgment. The court held that, even assuming the structures were illegal, the county's forced demolition violated the procedural requirements of the Administrative Coercion Law, including the obligation to notify and announce before enforcing, and the prohibition on night-time enforcement absent exceptional circumstances. The court therefore confirmed the county's forced demolition act as unlawful. No appeal was reported in the SPC summary.

Significance. This case demonstrates the court's role in constraining 'hard' enforcement tactics through procedural law. Courts may protect basic due-process rights even when villagers' underlying construction status is contested.

A.3.4 Liao Mingyao vs. Longnan County Government (2013)

Basic information. Case Number: Not disclosed in Supreme People's Court's published summary; Court: Anyuan County People's Court; Date: 2013-03; Location: Longnan County, Ganzhou, Jiangxi; Parties: Liao Mingyao (villager) vs. Longnan County People's Government; Source: Supreme People's Court "Ten Typical Expropriation/Demolition Cases" (Supreme People's Court, 2014).

Facts. Liao's house was included in a hospital construction project's relocation scope. After negotiations failed, the county's land/planning bureau identified part of Liao's structure as illegal and issued a self-demolition notice. The county then organized demolition without the statutory steps required by the Administrative Coercion Law (no prior 'urge' notice, no formal enforcement decision, no notice of litigation rights), and it also partly demolished the lawful portion, rendering the house unusable. Liao sued to confirm illegality of the county's demolition act. The dispute highlights how governments sometimes re-label disputed structures as 'illegal' to shortcut expropriation procedures; courts in this line of cases scrutinize such re-characterization.

Judgment. The court emphasized that the defendant bears the burden to produce evidence supporting the challenged specific administrative act within the statutory period. Because the county failed to submit evidence and legal basis for the forced demolition act (submitting only evidence of administrative punishment for illegal construction), the court confirmed the demolition act unlawful. After the judgment, a subsequent compensation suit was reportedly resolved through a mediated settlement.

Significance. This case is a textbook illustration of evidence-based judicial control: when governments do not (or cannot) document the legal steps behind coercive actions, they risk an illegality finding. It also shows the practical channel from 'illegality judgment' to 'real remedy' through later compensation bargaining/settlement.

A.3.5 Villagers of Zhuoma Village (represented by Liu Qijun) vs. Linxiang Municipal Government (2014)

Basic information. Case Number: Not applicable (reported as an 'administrative ruling' issued by Yueyang Junshan District People's Court); Court: Yueyang Junshan District People's Court; Date: 2014-06-16 (ruling dismissing the case); Location: Linx-

iang, Yueyang, Hunan; Parties: Zhuoma Village Liu-2 Group villagers (with Liu Qijun as litigation agent) vs. Linxiang Municipal Government/Mayor; Source: The Paper's journalist report (The Paper, 2014).

Facts. Villagers disputed the compensation standard and procedures used during land-taking for the Hangrui Expressway (Linxiang–Yueyang section). They alleged the local government did not properly issue required expropriation announcements or sign/perform compensation agreements, but instead relied on a local compensation-and-resettlement implementation plan (reported as Linxiang Government Office document (2012) No. 139). They brought an administrative suit seeking to revoke the plan and require higher compensation consistent with provincial standards.

Judgment. The court issued an administrative ruling dismissing the suit, reasoning that disputes over compensation standards set during the land administration process should first be handled via the statutory coordination route, rather than being filed directly as an administrative lawsuit.

Significance. This case is valuable because it shows how courts can function as 'gatekeepers' in rural expropriation disputes: a claim framed as challenging a compensation plan may be redirected to administrative coordination channels before courts will hear it.

A.3.6 Sha Mingbao vs. Huashan District Government (2015)

Basic information. Case Number: (2015) Anhui Administrative Compensation Final Judgment No. 00011; Court: Anhui High People's Court; Date: 2015; Location: Huashan District, Ma'anshan, Anhui; Parties: Sha Mingbao vs. Huashan District Government; Source: Supreme People's Court Guiding Case No. 91 (Supreme People's Court, 2017).

Facts. Anhui Province approved expropriation of collectively owned land in Huashan District, Ma'anshan. Before completing mandatory court-enforcement procedures and without first registering/notarizing household property, the district government demolished a villager's house. The heirs sought compensation for household items and other losses. The court emphasized that demolition of houses on collectively owned land typically requires prior administrative ordering and (if resisted) judicial enforcement, not direct demolition by the local government.

Judgment. The court held the demolition unlawful and shifted the burden of proof to the government because its procedural violations made evidence collection difficult. It awarded compensation for reasonable household items and applied a 'favor the higher end' approach for disputed-value items.

Significance. A core evidentiary rule for expropriation-related demolitions: local governments cannot benefit from their own procedural illegality. Courts may shift burdens and use common-sense benchmarks when the government's conduct destroys or prevents evidence.

A.3.7 Xu Shuiyun vs. Wucheng District Government (2018)

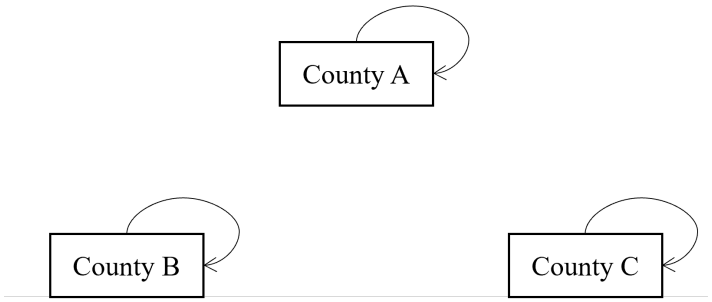
Basic information. Case Number: (2018) Supreme Court No. 1; Court: Supreme People's Court; Date: 2018-01-25; Location: Jinhua, Zhejiang; Parties: Xu Shuiyun vs. Wucheng District Government; Source: Supreme People's Court "Typical Cases for Property Rights Protection" (Supreme People's Court, 2018).

Facts. In an expropriation project in Jinhua, Zhejiang, a villager's two houses were demolished before a formal expropriation decision and without a compensation agreement. The government asserted the demolition was 'accidental'. The villager sought state compensation and argued that valuation should reflect current market value rather than values at the time of demolition. The Supreme People's Court trial opinion treats 'accidental demolition' as a factual claim that must be proven against the record; where demolition precedes lawful procedures, compensation liability is triggered.

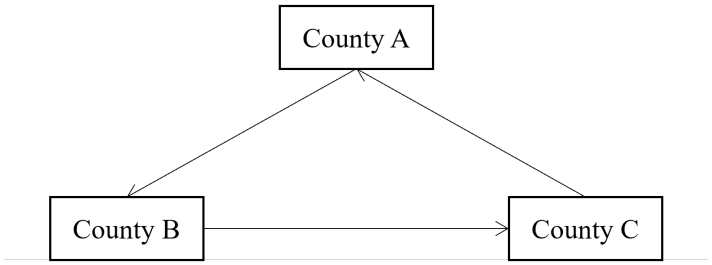
Judgment. The Supreme People's Court rejected the accidental demolition defense based on the record and clarified that unlawful demolition triggers administrative state compensation, not merely administrative compensation. It required compensation to be calculated at current market value, and ordered the government to provide comparable replacement housing or monetary compensation plus certain loss items.

Significance. This case demonstrates meaningful judicial oversight over demolition practices: (1) courts scrutinize accident narratives; (2) damages are tied to current market value to prevent governments from profiting through delay; (3) the choice between monetary compensation and replacement housing is treated as a substantive right.

A.4 Case illustration of TJAL



(a) Before TJAL

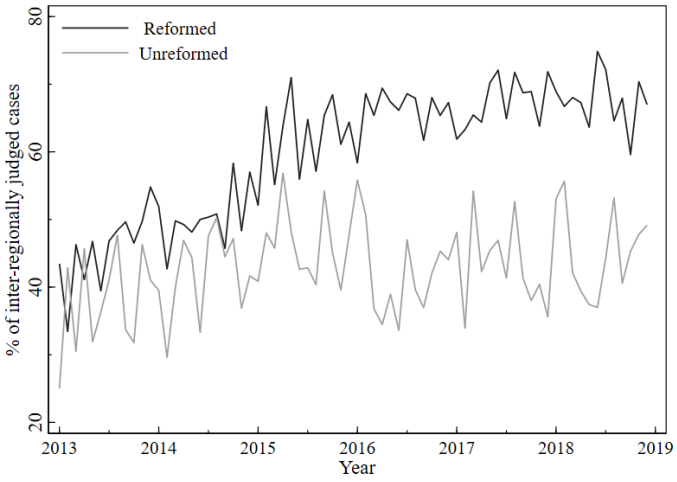


(b) After TJAL

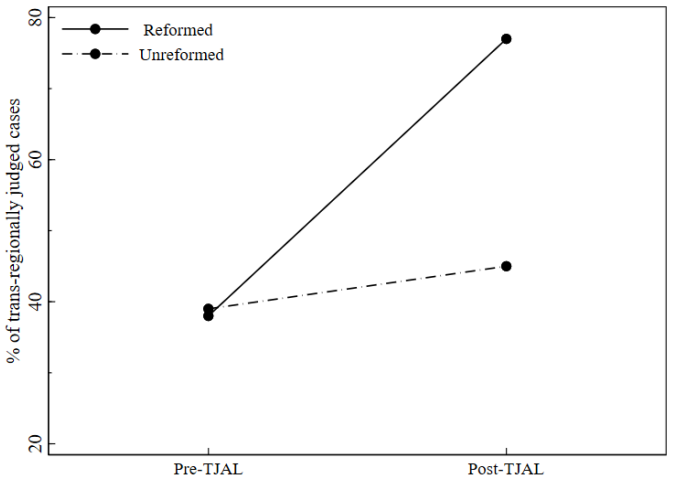
Figure A.2: Jurisdiction of administrative cases before and after TJAL

Note: This figure compares the jurisdiction of administrative cases before and after TJAL.

A.5 *De facto* implementation of TJAL



(a) By calendar year-month



(b) By reform periods

Figure A.3: *De facto* implementation of TJAL

Note: This figure presents the *de facto* implementation of TJAL. The sample is restricted to land cases where both defendant government and court are at county level. For the unreformed prefectures, the pre-TJAL period refers to 2013; the post-TJAL period refers to 2019.

A.6 Additional summary statistics

Table A.2: Additional summary statistics

Variable	N	Mean	SD	Source
Any party members (=1 if yes; =0 if no)	149,042	0.152	–	NFPS
Any religions (=1 if yes; =0 if no)	149,042	0.050	–	
Ethnic minorities (=1 if yes; =0 if no)	149,042	0.114	–	
Individual plaintiffs (=1 if yes; =0 if no)	207,129	0.908	—	CJO
Firm plaintiffs (=1 if yes; =0 if no)	207,129	0.068	—	
Other entities plaintiffs (=1 if yes; =0 if no)	207,129	0.024	—	

Note: This table reports the additional summary statistics used in this paper. Yuan= Chinese yuan.

B Additional results for rural land right

B.1 Baseline case-level evidence on rural land rights

Table B.1: The impact of TJAL on rural land rights: case-level evidence

Dep. Var.	(1) Inter-regional jurisdiction	(2) Winning probability
TJAL	0.146***	0.021***
	(0.008)	(0.008)
Year FE	Yes	Yes
Court FE	Yes	Yes
Controls	Yes	Yes
MDV	0.380	0.129
Observations	207,129	207,129
Adjusted R^2	0.688	0.143

Note: This table reports the DD estimates of the impact of TJAL on rural land rights using case-level data. The level of observation is the case-year. The sample is restricted to land dispute and police-enforcement dispute. The dependent variable is a dummy variable that equals one if the place of court is different from that of defendant government for Column (1); a dummy variable that equals one if the non-governmental litigant (plaintiff) wins the case for Column (2). The model specification follows $Y_{j,c,p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \eta_c + \zeta_t + X_{j,t} + X_{c,t} + X_{p,t} + \epsilon_{j,c,p,t}$, where $Y_{j,c,p,t}$ denotes the outcome variable of case j judged by court c in prefecture p and year t; η_c denotes court fixed effects; $X_{j,t}$ denotes case-level controls, including the number of plaintiffs, the number of defendants, case type fixed effects, and judge fixed effects; $X_{c,t}$ denotes court-level controls, including the total number of cases judged; $\epsilon_{j,c,p,t}$ denotes the error term; other terms are defined the same as in Equation 1. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the court level. *** p<0.01, ** p<0.05, * p<0.1.

B.2 Sequentially adding controls for plaintiff winning

Table B.2: Effects of TJAL on property right: Sequentially adding controls

Dep. Var.	Non-governmental litigant's winning probability			
	(1)	(2)	(3)	(4)
TJAL	0.026**	0.027**	0.020***	0.021***
	(0.010)	(0.010)	(0.008)	(0.008)
Year FE	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes
Case controls	No	Yes	Yes	Yes
Court controls	No	No	Yes	Yes
Prefecture controls	No	No	No	Yes
MDV	0.129	0.129	0.129	0.129
Observations	207,129	207,129	207,129	207,129
Adjusted R^2	0.075	0.089	0.144	0.143

Note: This table reports the DD estimates of the impact of TJAL on property right by sequentially adding different levels of controls. The level of observation is the case-year. The dependent variable is a dummy variable that equals one if the non-governmental litigant (plaintiff) wins the case. The model specification follows: $Y_{j,c,p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \zeta_t + \eta_c + X_{j,t} + X_{c,t} + X_{p,t} + \epsilon_{j,c,p,t}$, where $Y_{j,c,p,t}$ denotes the outcome variable of case j in court c, prefecture p and year t; $TJAL_{p,t}$, ζ_t and $X_{p,t}$ are an indicator of TJAL reform, year fixed effects, and city-level controls respectively, and are defined the same as in Equation (1); η_c denotes court fixed effects; $X_{j,t}$ denotes case-year level controls, including the number of plaintiffs, the number of defendants, case type fixed effects, and judge fixed effects; $X_{c,t}$ denotes court-level controls, including the number of cases judged; $\epsilon_{j,c,p,t}$ is the error term. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the court level. *** p<0.01, ** p<0.05, * p<0.1.

B.3 Dynamics of TJAL's impacts on plaintiff winning

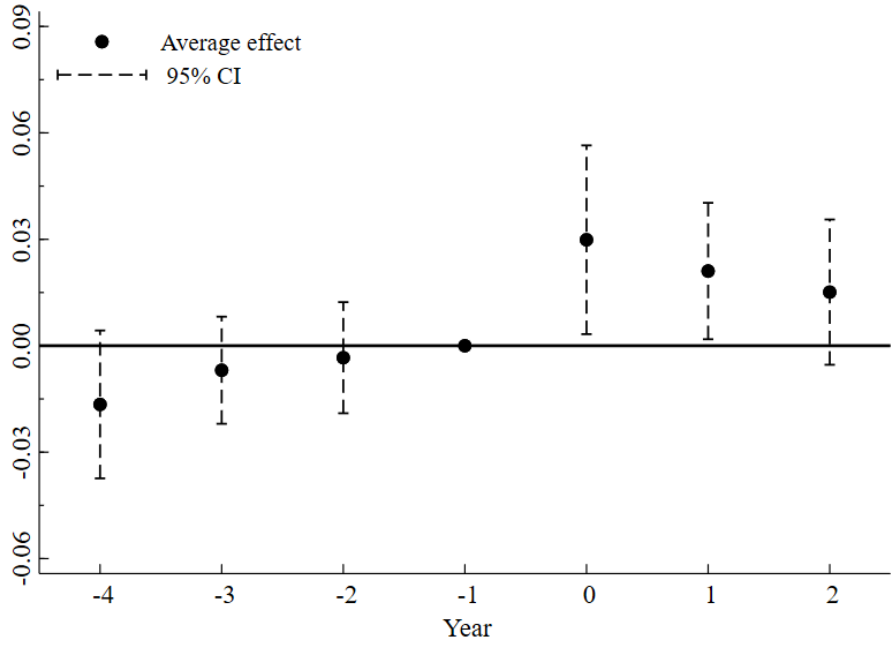


Figure B.1: Parallel trend test for plaintiff winning

Note: This figure presents the dynamic effects of TJAL on the winning of non-governmental litigants. The level of observation is the case-year. The dependent variable is a dummy variable that equals one if the non-governmental litigant (plaintiff) wins the case. The model specification follows $Y_{j,c,p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \eta_c + \zeta_t + X_{j,t} + X_{c,t} + X_{p,t} + \epsilon_{j,c,p,t}$, where $Y_{j,c,p,t}$ denotes the outcome variable of case j judged by court c in prefecture p and year t; η_c denotes court fixed effects; $X_{j,t}$ denotes case-level controls, including the number of plaintiffs, the number of defendants, case type fixed effects, and judge fixed effects; $X_{c,t}$ denotes court-level controls, including the total number of cases judged; $\epsilon_{j,c,p,t}$ denotes the error term; other terms are defined the same as in Equation 1. Robust standard errors are in parentheses and are clustered at the court level. The 95% confidence intervals are calculated based on standard errors.

B.4 Alternative measures of plaintiff winning

Table B.3: The impact of TJAL on land right: Alternative measures

Dep. Var.	Non-governmental litigant's winning probability	
	(1) Baseline	(2) Litigation cost
TJAL	0.021***	0.020***
	(0.008)	(0.008)
Year FE	Yes	Yes
Court FE	Yes	Yes
Controls	Yes	Yes
MDV	0.129	0.127
Observations	207,129	207,129
Adjusted R^2	0.143	0.145

Note: This table reports the DD estimates of the impact of TJAL on property right. The level of observation is the case-year. The dependent variable is a dummy variable that equals one if the non-governmental litigant (plaintiff) wins the case. Column (1) defines case outcomes based on the satisfaction of the plaintiff's claims. Column (2) defines case outcomes based on the allocation of litigation costs. The model specification follows: $Y_{j,c,p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \zeta_t + \eta_c + X_{j,t} + X_{c,t} + X_{p,t} + \epsilon_{j,c,p,t}$, where $Y_{j,c,p,t}$ denotes the outcome variable of case j in court c, prefecture p and year t; $TJAL_{p,t}$, ζ_t and $X_{p,t}$ are an indicator of TJAL reform, year fixed effects, and city-level controls respectively, and are defined the same as in Equation (1); η_c denotes court fixed effects; $X_{j,t}$ denotes case-year level controls, including the number of plaintiffs, the number of defendants, case type fixed effects, and judge fixed effects; $X_{c,t}$ denotes court-level controls, including the number of cases judged; $\epsilon_{j,c,p,t}$ is the error term. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the court level. *** p<0.01, ** p<0.05, * p<0.1.

B.5 Placebo test for non-administrative cases

Table B.4: Placebo test for non-land disputes

Dep. Var.	Winning probability	
	(1) Land disputes	(2) Non-administrative cases
TJAL	0.021***	-0.000
	(0.008)	(0.005)
Year FE	Yes	Yes
Court FE	Yes	Yes
Controls	Yes	Yes
MDV	0.129	0.565
Observations	207,129	10,027,202
Adjusted R^2	0.143	0.232

Note: This table reports the DD estimates of the impact of TJAL on property right. The level of observation is the case-year. The dependent variable is a dummy variable that equals one if the non-governmental litigant (plaintiff) wins the case. Column (1) replicates the baseline results in Column (2) of Table B.5. Column (2) restricts the sample to non-administrative cases. The model specification follows: $Y_{j,c,p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \zeta_t + \eta_c + X_{j,t} + X_{c,t} + X_{p,t} + \epsilon_{j,c,p,t}$, where $Y_{j,c,p,t}$ denotes the outcome variable of case j in court c, prefecture p and year t; $TJAL_{p,t}$, ζ_t and $X_{p,t}$ are an indicator of TJAL reform, year fixed effects, and city-level controls respectively, and are defined the same as in Equation (1); η_c denotes court fixed effects; $X_{j,t}$ denotes case-year level controls, including the number of plaintiffs, the number of defendants, case type fixed effects, and judge fixed effects; $X_{c,t}$ denotes court-level controls, including the number of cases judged; $\epsilon_{j,c,p,t}$ is the error term. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the court level. *** p<0.01, ** p<0.05, * p<0.1.

B.6 Examine the change in number of cases

Table B.5: The impact of TJAL on number of cases

Dep. Var.	# of cases	
	(1)	(2)
TJAL	0.005	0.003
	(0.015)	(0.014)
Year FE	Yes	Yes
Court FE	Yes	No
Controls	No	Yes
MDV	0.363	0.363
Observations	226,632	226,632
Adjusted R^2	0.422	0.422

Note: This table reports the DD estimates of the impact of TJAL on property right. The level of observation is the court-year-month. The sample is land dispute. The dependent variable is the IHS transformation of number of cases. The model specification follows: $Y_{c,p,t} = \delta_0 + \delta_1 TJAL_{p,t} + \zeta_t + \eta_c + X_{c,t} + X_{p,t} + \epsilon_{c,p,t}$, where $Y_{c,p,t}$ denotes the outcome variable of court c in prefecture p and year t; $TJAL_{p,t}$, ζ_t, η_c, $X_{c,t}$ and $X_{p,t}$ are an indicator of TJAL reform, year fixed effects, court fixed effects, court-level controls and city-level controls respectively; $\epsilon_{c,p,t}$ is the error term. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the court level. *** p<0.01, ** p<0.05, * p<0.1.

B.7 Examine composition change of cases

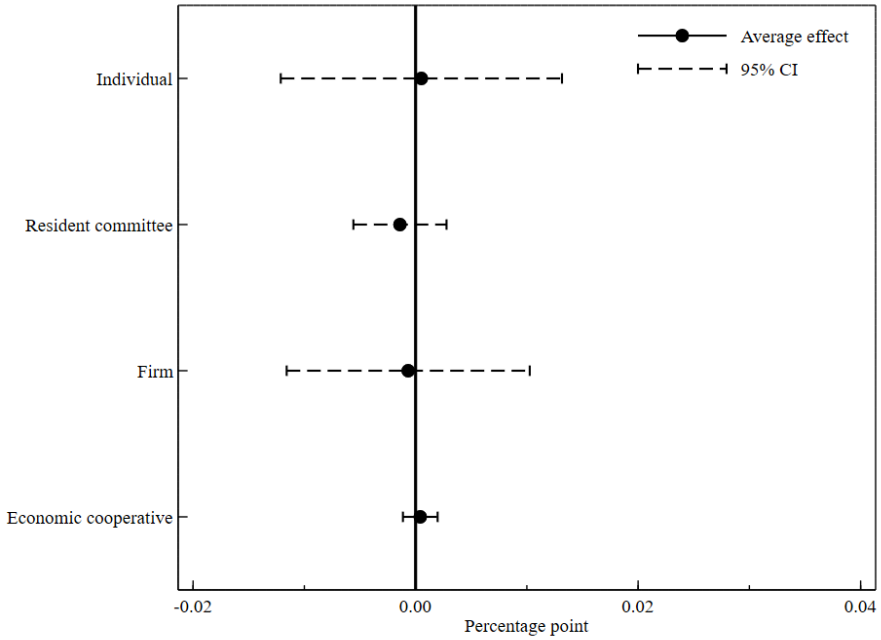


Figure B.2: Null composition effects

Note: This figure presents the composition change of cases induced by TJAL. The level of observation is the case-year. The dependent variable is a dummy variable that equals one if the non-governmental litigant (plaintiff) is a certain type of entity. The model specification follows: $Y_{j,c,p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \zeta_t + \eta_c + X_{j,t} + X_{c,t} + X_{p,t} + \epsilon_{j,c,p,t}$, where $Y_{j,c,p,t}$ denotes the outcome variable of case j in court c, prefecture p and year t; $TJAL_{p,t}$, ζ_t and $X_{p,t}$ are an indicator of TJAL reform, year fixed effects, and city-level controls respectively, and are defined the same as in Equation (1); η_c denotes court fixed effects; $X_{j,t}$ denotes case-year level controls, including the number of plaintiffs, the number of defendants, case type fixed effects, and judge fixed effects; $X_{c,t}$ denotes court-level controls, including the number of cases judged; $\epsilon_{j,c,p,t}$ is the error term. Robust standard errors are in parentheses and are clustered at the court level. The 95% confidence intervals are calculated based on standard errors.

C Additional results for rural factor reallocation

C.1 Farm work income: By weather condition

Table D.1: The impact of TJAL on farm work income: By weather condition

Dep. Var.	Farm work inc	
	(1)	(2)
TJAL	0.011	-0.007
	(0.041)	(0.040)
TJAL×Positive		0.104**
weather		(0.053)
Year FE	Yes	Yes
Household FE	Yes	Yes
Controls	Yes	Yes
MDV	9.586	9.586
Observations	111,506	111,506
Adjusted R^2	0.835	0.835

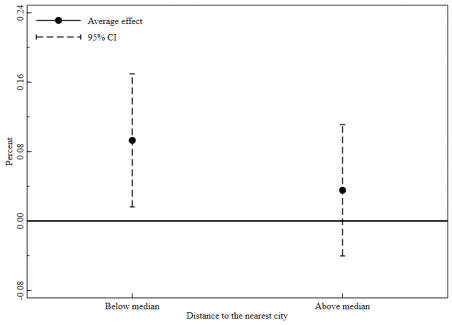
Note: This table reports the DD estimates of the impact of TJAL on rural income. The level of observation is the household-year. The dependent variable is the IHS transformation of household's farm work income. Column (1) only includes households that participate in farm work. Column (2) adds an interaction between TJAL indicator and a positive weather dummy that equals one if the precipitation is above the 75 percentile of average during the sample period. I include year fixed effects, household fixed effects, household-level controls, and city-level controls, as described in Equation (1). MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

C.2 Whether effects are driven by the risk of land expropriation?

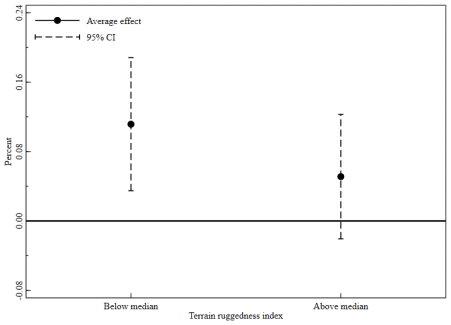
Table D.2: Correlation between land expropriation and village geography

Dep. Var.	Proportion of households being expropriated			
	(1)	(2)	(3)	(4)
Distance to the nearest city	-0.018***	-0.018***		
	(0.007)	(0.007)		
Terrain ruggedness index			-0.021**	-0.021**
			(0.009)	(0.010)
Controls	No	Yes	No	Yes
MDV	0.035	0.035	0.035	0.035
Observations	2,278	2,278	2,278	2,278
Adjusted R^2	0.014	0.043	0.008	0.025

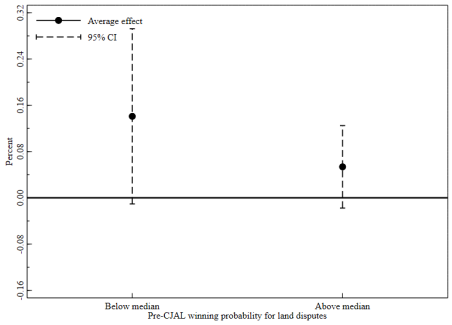
Note: This table reports the DD estimates of the impact of TJAL on migration. The level of observation is the village-year. The dependent variable is the proportion of households being expropriated. The variables of interest are the IHS transformation of distance to the nearest prefecture for Column (1) and (2), and the IHS transformation of terrain ruggedness index for Column (3) and (4). The model specification follows: $Y_{v,p,t} = \mu_0 + \mu_1 TJAL_{p,t} + \zeta_t + X_{p,t} + \varepsilon_{v,p,t}$, where $Y_{v,p,t}$ denotes the outcome variable of village v in prefecture p and year t; $TJAL_{p,t}$, ζ_t and $X_{p,t}$ are an indicator of TJAL reform, year fixed effects, and city-level controls respectively, and are defined the same as in Equation (1); $\varepsilon_{v,p,t}$ is the error term. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1.



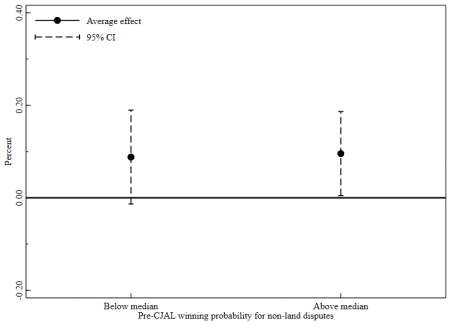
(a) Total inc: By proximity



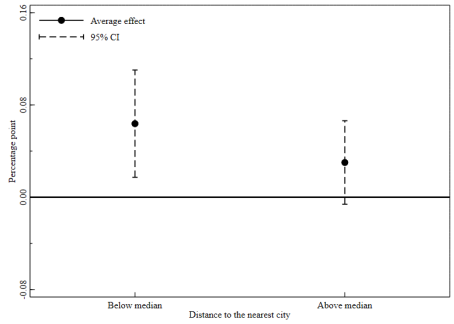
(b) Total inc: By terrain ruggedness



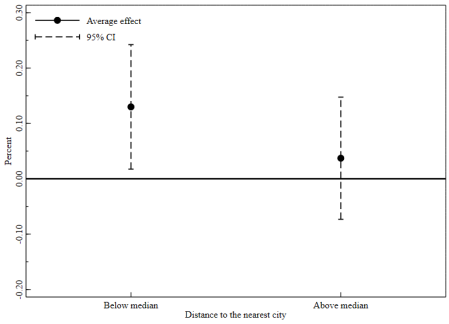
(c) Total inc: By pre-TJAL winning probability of land cases



(d) Total inc: By pre-TJAL winning probability of non-land cases



(e) Off-farm work: By proximity



(f) Off-farm work inc: By proximity

Figure D.1: Effects of TJAL on rural income: by land expropriation risk

Note: This figure presents the heterogeneous effects of TJAL. The sample is divided by the median distance to the nearest city for Panel D.1a, Panel D.1f, and Panel D.1e; by the median terrain ruggedness used in Nunn and Puga (2012) for Panel D.1b; by the median winning probability of land cases for Panel D.1c; by the median winning probability of non-land cases for Panel D.1d. Robust standard errors are in parentheses and are clustered at the prefecture level. The 95% confidence intervals are calculated based on standard errors.

C.3 Labor allocation: By pre-TJAL farming participation

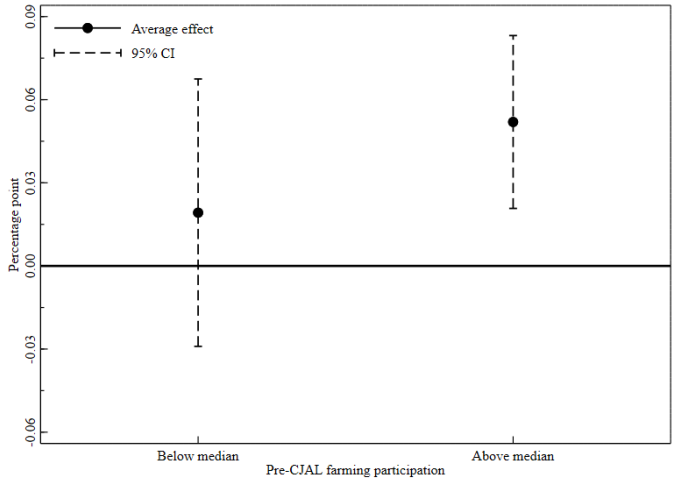


Figure D.2: Effects of TJAL on labor allocation: By pre-TJAL farming participation

Note: This table reports the DD estimates of the impact of TJAL on labor allocation by pre-TJAL farming participation. The level of observation is the household-year. The dependent variable is a dummy variable that equals one if a household participate in off-farm work. I include year fixed effects, household fixed effects, household-level controls, and city-level controls, as described in Equation (1). Robust standard errors are in parentheses and are clustered at the prefecture level. The 95% confidence intervals are calculated based on standard errors.

D Theoretical validation for rural factor reallocation

D.1 Setup

In a stand-in village v inhabited by F farmers, individual farmers $i \in [1, F]$ are ranked based on their agricultural total factor productivity (TFP), A_i.[27] Each farmer is endowed with one unit of land and one unit of labor. The labor of each farmer can be freely allocated between self-employed farm work or off-farm work, but not both.[28] The land can be transferred among farmers. The off-farm work wage, W^*, is exogenously determined, and increases with agricultural TFP.[29] The agricultural output of farmer i is given by:

$$Y_i = A_i T_i^{\alpha} L_i^{1-\alpha}, \tag{3}$$

where T_i and L_i denotes land and labor inputs (to farm work) respectively, with $\alpha \in (0, 1)$. However, without secure land rights, engaging in off-farm work can incur costs related to defending the endowed land or carry the risk of land loss (c_v).[30] This design intentionally incorporates heterogeneity across villages.[31] Hence, off-farm work income of farmer i follows:

[27] For simplicity, I assume that farmers with lower agricultural TFP have a higher comparative advantage in off-farm work. This will be relaxed and discussed in section D.3.

[28] This indivisibility of labor is intentionally designed to separate migrant work (year-round off-farm employment) from temporary mixed work (farm work during the agricultural season combined with related off-farm work during non-agricultural periods) and pure farm work. I consider only year-round migrant work as off-farm work for three reasons: (1) it is relatively more formal; (2) it typically requires farmers to relocate from their villages to prefectures; and (3) it is a common phenomenon in developing countries. This definition aligns with the measure of off-farm work participation in section 3, where a dummy variable equals one if a household generates income solely from off-farm work (with farm work income equal to zero). I will discuss further details in that section.

[29] This is because farmers with higher agricultural TFP are generally better educated, younger, healthier, and have greater access to information. As a result, these farmers are more likely to secure better-paid off-farm employment.

[30] In the literature, this is so called "usage-dependent land risk": land rights are primarily based on *de facto* land use rather than *de jure* certification (e.g., land titling) (Liu et al., 2023; Gottlieb and Grobovšek, 2019; Giles and Mu, 2017; de Janvry et al., 2015; de Brauw and Mueller, 2012; Galiani and Schargrodsky, 2010; Do and Iyer, 2008). Consequently, engaging in off-farm work that does not involve land can result in a risk of land loss or incur costs associated with defending the endowed land. A remarkable work by Gottlieb and Grobovšek (2019) suggests that such land right settings account for half of the cross-country variation in agricultural TFP. Similar patterns are observed and shown in Figure 1. Appendix Table A.1 shows that off-farm work explains more than 50% of the household level variation in expropriation probability.

[31] As discussed in Ma and Mu (2020), land expropriation largely depends on village-level (rather than household-level) characteristics such as proximity to prefectures. Villages closer to prefectures are more likely to be expropriated for urban construction (e.g., business, housing, and infrastructure) due to the higher commercial value of these lands.

$$W_i(L_i) = (1 - L_i)(W^* - c_v). \tag{4}$$

Since each farmer can allocate labor only to farm work or to off-farm work, the off-farm work income equals zero ($W_i = 0$) when he participates in farm work ($L_i = 1$), and equals $W^* - c_v$ when he participates in off-farm work ($L_i = 0$).

D.2 Equilibrium with insecure land rights

Each farmer determines the optimal allocation of labor and land resources to maximize total income, as follows:

$$I_i = \begin{cases} A_i T_i^{\alpha} L_i^{1-\alpha} - (T_i - 1)R & \text{for farm work} \\ (1 - T_i)R + (1 - L_i)(W^* - c_v) & \text{for off-farm work,} \end{cases} \tag{5}$$

where R is the equilibrium land rent. For those who participate in farm work ($L_i = 1$), the total income equals the farm output minus the land rents paid ($A_i T_i^{\alpha} - (T_i - 1)R$). For those who participate in off-farm work ($L_i = 0$), the total income equals the land rents received plus the off-farm income ($(1 - T_i)R + W^* - c_v$). Hence, the condition for engaging in farm work is that the marginal return from farm work exceeds the marginal return from off-farm work

$$(1 - \alpha)A_i T_i^{\alpha} > W^* - c_v. \tag{6}$$

Since farmers are ranked according to their agricultural TFP, there exists a threshold farmer $z \in [1, F]$ who is indifferent between farm work and off-farm work ($(1 - \alpha)A_z = W^* - c_v$).[32] For farmers with $A_i > A_z$ ($A_i < A_z$), they will participate in farm work (off-farm work). And the village reaches equilibrium when the land market clearing condition ($\sum_{i=z}^{F} T_i = V$) is met.

[32]Note that the minimum value of land input possible is one unit ($T_i = 1$), corresponding to the amount allocated to farm work by the threshold farmer z. This farmer, being the lowest in agricultural TFP among those engaged in farm work, cannot afford any more land transferred from those participating in off-farm work. Any land transferred will be rented by more productive farmers, leaving him with only his endowment.

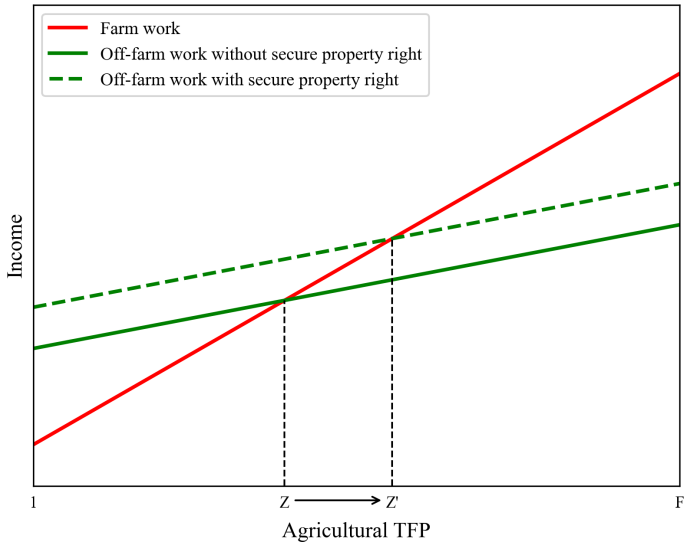


Figure C.1: Factor allocation when land rights are more secure

Note: This figure presents the labor reallocation when land rights are more secure. The assumption is that farmers with a lower agricultural TFP have a higher comparative advantage in off-farm work. The solid red line shows the farm work income (per unit of land). The solid green line shows the off-farm work without secure land right. The dashed green line shows the off-farm work with secure land right.

These are visually presented in Appendix Figure C.1. The solid red line presents the farm work income. The solid red line green line presents the off-farm income without secure land rights. And the intersection of these two lines defines the threshold farmer z without secure land right, where farmer $i > z$ ($i < z$) will participate in farm work (off-farm work).

D.3 Equilibrium with secure land rights

Now consider the scenario with secure land rights. When land rights are more secure, farmers no longer need to bear the risk or cost of defending land, so $c_v = 0$. This increases the marginal return of off-farm work (dashed green line), leading to a new threshold farmer z' such that $(1-\alpha)A_{z'} = W^*$. Formally, this model provides several empirically testable propositions:

Proposition 1: More secure land rights lead to more farmers participating in off-farm work.[33]

[33]Figure C.1 predicts that labor reallocation effects are concentrated among households that participate in farm work prior to TJAL. To verify this, I divide the sample based on the median share of years participating in farm work before TJAL and re-estimate Equation (1). Appendix Figure D.2 shows that the effect is both economically and statistically significant for pre-TJAL farming households, but insignificant for pre-TJAL off-farm households. These results serve as a robustness test for

Proposition 2: More secure land rights lead to more active land rental market. Those farmers who remain in the farm work will have larger land to cultivate.

Proposition 3: More secure land rights lead to higher total income per farmer, higher off-farm work income per farmer, and farm work income per farming farmer. But its effects on farm work income per farmer is ambiguous.

There are four points worth noting. First, the ambiguous predictions of farm work income per farmer in Proposition 3 are because more secure land rights decrease labor inputs but increase the average agricultural TFP and land holdings of farming farmers. This should not be interpreted as a mixed or negative predictions about farm work efficiency, as suggested in some recent literature (Liu et al., 2023). To examine this, one should evaluate the farm work income per farming farmer, in order to separate the overall impacts from changes in labor inputs.

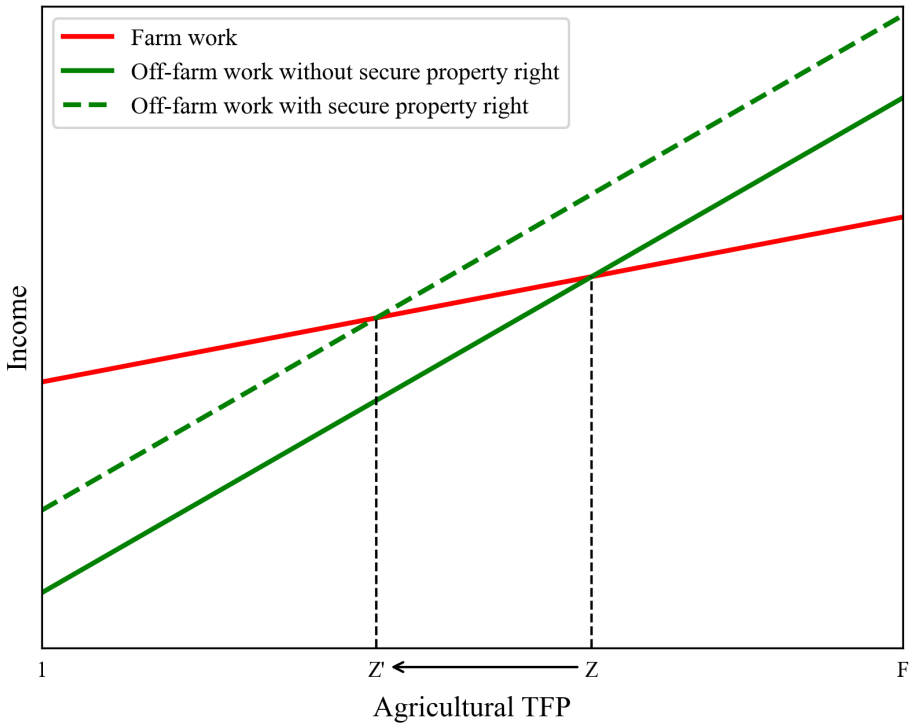


Figure C.2: Factor allocation under alternative comparative advantage assumption

Note: This figure presents the labor reallocation when land rights are more secure. The assumption is that farmers with a higher agricultural TFP have a higher comparative advantage in off-farm work. The solid red line shows the farm work income (per unit of land). The solid green line shows the off-farm work without secure land right. The dashed green line shows the off-farm work with secure land right.

Second, the model can alternatively assume that farmers with higher agricultural

observed effects.

TFP possess greater comparative advantages in off-farm work. Appendix Figure C.2 illustrates this scenario. All three propositions remain valid in this setup. There is only one slight difference in the rationale leading to Proposition 3: more secure land rights decrease labor inputs and the average agricultural TFP of farming farmers, but still increase average land inputs of these farmers.

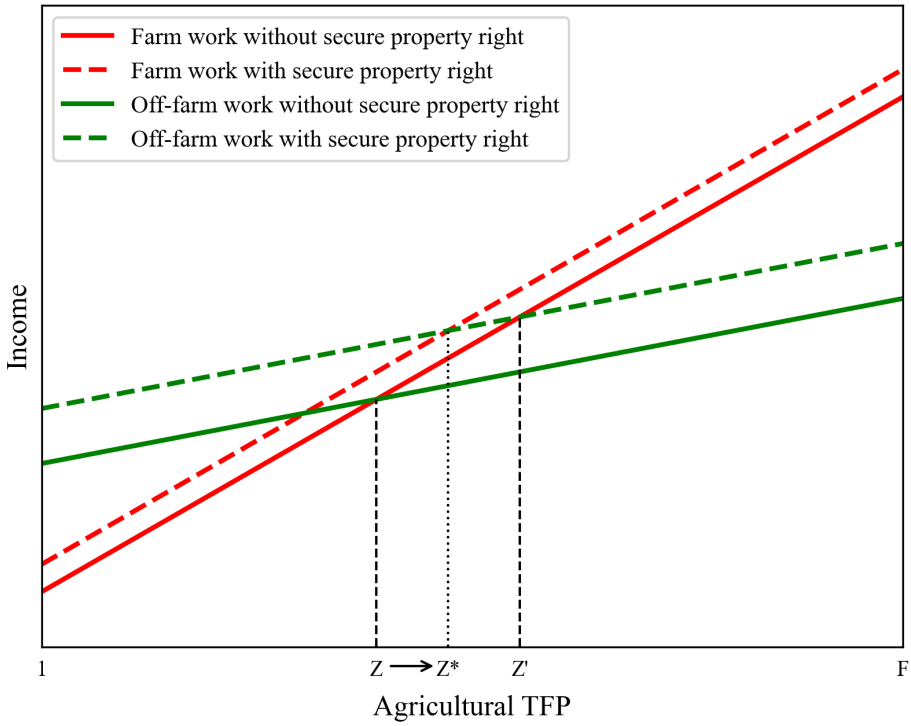


Figure C.3: Factor allocation when incorporating agricultural TFP gain

Note: This figure presents the labor reallocation and incorporates agricultural TFP gain when property right is more secure. The assumption is that farmers with a lower agricultural TFP have a higher comparative advantage in off-farm work. The solid red line shows the farm work income (per unit of land) without secure property right. The dashed red line shows the farm work income (per unit of land) with secure property right. The solid green line shows the off-farm work without secure property right. The dashed green line shows the off-farm work with secure property right.

Third, the baseline model assumes no productivity gains (fixed agricultural TFP (A_i)) when land rights become more secure. I present a graphical illustration in Appendix Figure C.3, assuming that there are productivity gains in farm work. The impacts on labor allocation now reflect the *net* effects of two opposing forces: the shift away effects and the attraction effects due to productivity gains. These result in a new threshold z^*. If the former dominates (i.e., z^* lies between z and z'), all my previous propositions hold. If the attraction effects are large enough to offset the shift away effects, there will be fewer farmers participating in off-farm work. While current lit-

erature generally rejects the second prediction, highlighting an oversupply of labor in farm work due to insecure land rights (Liu et al., 2023; Gottlieb and Grobovšek, 2019; Galiani and Schargrodsky, 2010; Do and Iyer, 2008), as well as the productivity gains already achieved in early deregulation and marketization, leaving less to adopt (Chari et al., 2021; Lin, 1992), this is subject to empirical tests.

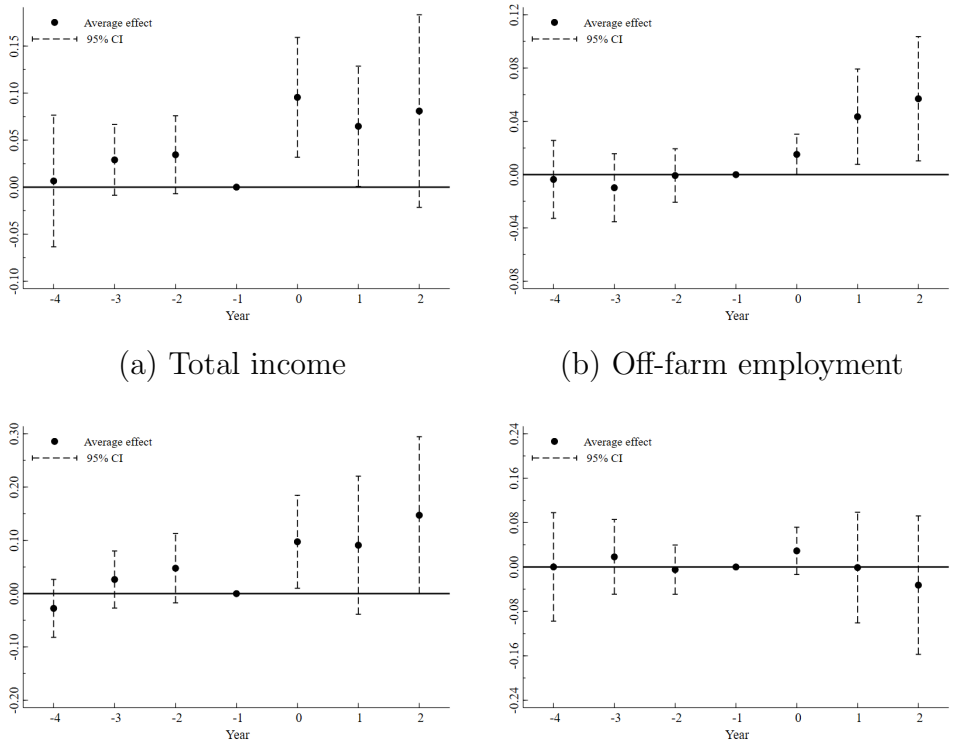
Fourth, in this model, I assume that off-farm wage (W^*) is exogenously determined and constant. If this assumption is relaxed, more secure land rights could yield competing predictions for W^*: (1) an increase due to more productive urban firms (since TJAL alters the jurisdiction of all administrative cases), or (2) a decrease due to more rural labor supply and higher development expenses in the prefecture. In the first scenario, labor participation in off-farm work would increase (even more); in the second, the off-farm income line (dashed green line) would shift downward, potentially offsetting or even reversing the impacts of secure land rights on labor allocation. These competing predictions warrant further empirical investigation.

E Validity checks for the effects of TJAL on rural income

This appendix presents robustness checks to assess the reliability of my findings on rural income.

E.1 Test for parallel trend assumption

To interpret the estimates causally, one key concern is whether unobserved factors are driving both the implementation of TJAL and the variation in outcome variables. If this is the case, reformed prefectures are likely to exhibit different trends compared to unreformed prefectures before reform. However, Appendix Figure E.1 shows no statistically significant difference. The pre-TJAL estimates are insignificant and close to zero, and the post-TJAL estimates become positive and significant for total income and off-farm income. This parallel pre-trend pattern supports a causal interpretation, suggesting that, in the absence of TJAL, households living in reformed or unreformed prefectures would have evolved similarly.



(a) Total income (b) Off-farm employment

(c) Off-farm income (d) Farm work income

Figure E.1: Parallel trend test for rural outcomes

Note: This figure presents the dynamic effects of TJAL on household total income, off-farm employment, off-farm income, and farm work income. The level of observation is the household-year. I include year fixed effects, household fixed effects, household-level controls, and city-level controls, as described in Equation (1). Panel E.1d only includes farming households. Robust standard errors are in parentheses and are clustered at the prefecture level. The 95% confidence intervals are calculated based on standard errors.

Some recent studies point out the low (statistical) power issue of pretrend tests, which may arise from arbitrary pre-treatment trends. Therefore, I assess the sensitivity of pretrend test, as proposed by (Rambachan and Roth, 2023). Assuming that the violation of the parallel trend assumption before treatment is not markedly different from that after treatment, I calculate the counterfactual confidence intervals–adjusting the original confidence interval by $\boldsymbol{M}$ times the largest violation of parallel trend assumption in the pre-treatment period–and examine when the post-treatment effects would become invalid. Appendix Figure E.2 shows that the effects generally remain significantly different from zero, even when the violation in the post-treatment period is as large as the maximum of that in the pre-treatment period. This ensures the statistical power of the pretrend tests.

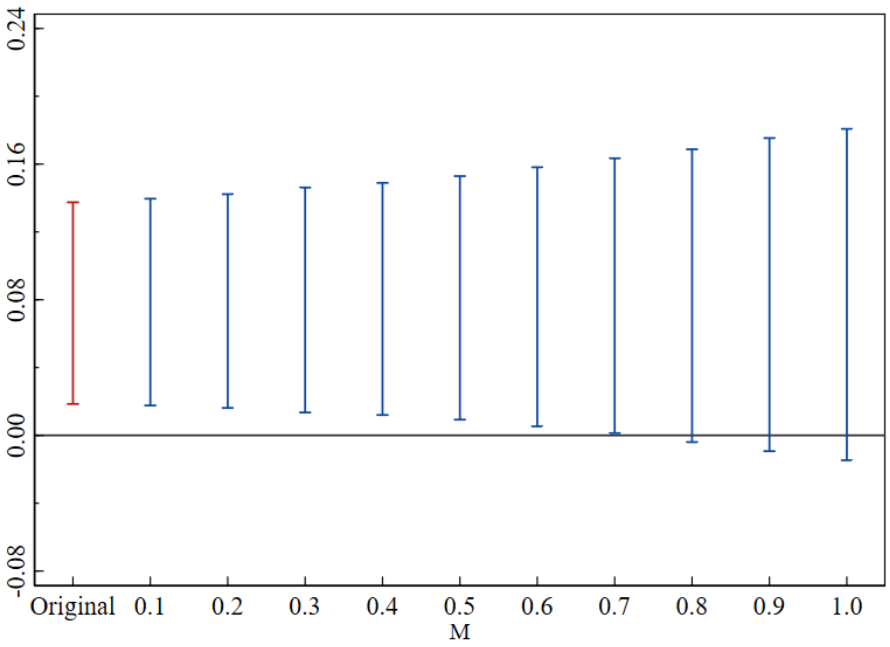


Figure E.2: Sensitivity check for pretrends

Note: To assess to how sensitive the pretrend test is towards the potential violation of parallel trend assumption, this figure reports the test purposed by Rambachan and Roth (2023) for Figure E.1. The underlying assumption is that the violation of parallel trend assumption before the treatment will not be too different from that after the treatment. The original 95% confidence intervals (red) are adjusted by M times the largest violation of parallel trend assumption in the pre-treatment period to calculate the counterfactual intervals (blue).

E.2 Alternative robust DD estimators

Some latest econometrics literature raises concerns regarding the interpretability of two-way fixed effect (TWFE) DD estimator used in the baseline regression. These concerns primarily stem from the heterogeneity of treatment effects across time and groups. To alleviate such concerns, I employ robust DD estimators developed by Borusyak et al. (2024); Sun and Abraham (2021); Callaway and Sant'Anna (2021), and present the estimated effects in Appendix Figure E.3. The coefficients from these robust DD estimators exhibit similar patterns as those of TWFE DD estimator, thereby supporting the parallel trend assumption and indicating a statistically similar effects.

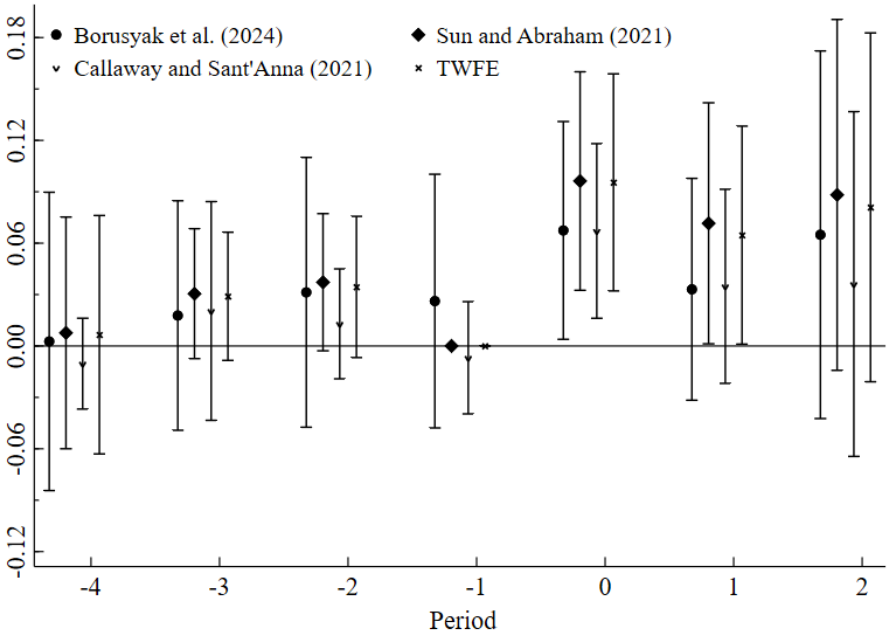


Figure E.3: Pretrend tests with alternative DD estimators

Note: This figure presents the dynamic effects of TJAL on household total income using four different estimators from Borusyak et al. (2024) (circle), Sun and Abraham (2021) (diamonds), Callaway and Sant'Anna (2021) (squares) and TWFE ("x"). The level of observation is the household-year. I include year fixed effects, household fixed effects, household-level controls, and city-level controls, as described in Equation (1). Robust standard errors are in parentheses and are clustered at the prefecture level. The 95% confidence intervals are calculated based on standard errors.

E.3 Test for the selection issue of TJAL

Selective implementation of TJAL is a potential concern for causal interpretation. Herein, I conduct a balance test to assess the selection issue of TJAL. The dependent variable is a dummy=1 if a prefecture implements TJAL during the sample period. The explanatory variables include the logarithm of GDP per capita, the logarithm of government expenditure, the logarithm of population, and the GDP share of secondary industry. Appendix Table E.1 reveals that most coefficients are statistically insignificant between 2008 and 2012. These suggest that reformed prefectures are not statistically different from those unreformed at least five years before the TJAL reform, thereby supporting the absence of the selection issue on these observables.

Table E.1: Do observables drive the implementation of TJAL?

Dependent Variable:	Dummy=1 if a implements TJAL				
Sample year	(1) 2012	(2) 2011	(3) 2010	(4) 2009	(5) 2008
ln(GDP per capita)	-0.117	-0.059	0.045	0.055	-0.043
	(0.088)	(0.084)	(0.105)	(0.100)	(0.099)
ln(Government expenditure)	-0.094	-0.029	-0.129	-0.052	0.026
	(0.110)	(0.096)	(0.137)	(0.137)	(0.118)
ln(Population)	0.133	0.088	0.211*	0.126	0.077
	(0.095)	(0.081)	(0.123)	(0.121)	(0.115)
GDP share of secondary industry	0.005	0.002	-0.001	-0.002	0.000
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	235	235	235	235	235
Adjusted R^2	0.041	0.447	0.445	0.449	0.458

Note: This table reports the balance test of TJAL. The unit of observation is prefecture. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

E.4 Test for the endogeneity of TJAL's reform timing

Another identification challenge is the endogenous reform timing of TJAL. By utilizing the city-year level Panel, I define the dependent variable as a dummy=1 if the year is TJAL's reform year, otherwise zero. The explanatory variables are the same as those included in section E.3. The results, presented in Appendix Table E.2, indicate no significant correlation between TJAL's reform year and key socio-economic characteristics, even after controlling for year and prefecture fixed effects.

Table E.2: Do observables determine TJAL's reform timing?

Dep. Var.	Dummy=1 if year= TJAL's reform year	
	(1)	(2)
ln(GDP per capita)	-0.009	0.142
	(0.009)	(0.124)
ln(Government expenditure)	-0.008	0.032
	(0.011)	(0.052)
ln(Population)	0.011	0.122
	(0.012)	(0.122)
GDP share of secondary	0.000	-0.009*
industry	(0.001)	(0.005)
Year FE	Yes	Yes
Prefecture FE	No	Yes
Observations	1800	1800
Adjusted R^2	0.069	0.177

Note: This table reports the balance test of TJAL. The unit of observation is city-year. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

E.5 Encompassing test for other judicial reforms

This section examines the confounding effects of other contemporaneous judicial reforms, whose contemporaneity could impact outcomes and bias the estimates. To mitigate these concerns, I evaluate four reforms: interprovincial circuit tribunals, court management centralization, judge quota reform, and anti-corruption patrols. As shown in Appendix Table E.3, adding indicators for these reforms reveals no significant changes. Collectively, these reforms account for less than 5% of the estimated effects, suggesting minimal bias in the original analysis.

Table E.3: Encompassing test for other judicial reforms

Dep. Var.	Total inc				
	(1) Baseline	(2)	(3)	(4)	(5)
TJAL	0.073**	0.070**	0.071**	0.070**	0.070**
	(0.029)	(0.030)	(0.030)	(0.030)	(0.030)
Interprovincial circuit tribunal	No	Yes	Yes	Yes	Yes
Court management centralization	No	No	Yes	Yes	Yes
Judge quota	No	No	No	Yes	Yes
Anti-corruption patrol	No	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
MDV	11.139	11.139	11.139	11.139	11.139
Observations	149,042	149,042	149,042	149,042	149,042
Adjusted R^2	0.717	0.717	0.717	0.717	0.717

Note: This table examines the potential confounding effects of other judicial reforms. The level of observation is the household-year. Column (1) replicates the results shown in Column (1) of Table E.4. Column (2) includes the indicator of interprovincial circuit tribunal reform. Column (3) includes the indicator of judge quota reform. Column (4) includes the indicator of anti-corruption patrol. I include year fixed effects, household fixed effects, household-level controls, and city-level controls, as described in Equation (1). MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

E.6 Impacts on rural household total income: Sequentially adding controls

Estimates remain stable as controls are sequentially added in Column (1)–(3) of Appendix Table E.4, indicating robustness to alternative covariate sets. Because land expropriation risk likely reflects village characteristics (e.g., proximity to prefectures; Ma and Mu (2020)), I replace household fixed effects with village fixed effects and re-estimate Equation (1). Column (4) shows a similar 6.4% increase in total household income.

Table E.4: The impact of TJAL on rural household total income

Dep. Var.	Total inc			
	(1)	(2)	(3)	(4)
TJAL	0.064**	0.064**	0.073**	0.064**
	(0.029)	(0.029)	(0.029)	(0.028)
Year FE	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	No
Household controls	No	Yes	Yes	Yes
Prefecture controls	No	No	Yes	Yes
Village FE	No	No	No	Yes
MDV	11.139	11.139	11.139	11.139
Observations	149,042	149,042	149,042	149,042
Adjusted R^2	0.672	0.672	0.717	0.263

Note: This table reports the DD estimates of the impact of TJAL on household total income. The level of observation is the household-year. The dependent variable is the IHS transformation of household's total income. Column (1) includes year fixed effects and household fixed effects. Column (2) adds household-level controls, including a dummy variable that equals one if the household head is a party member, a dummy variable that equals one if the household has religion beliefs, and a dummy variable that equals one if the household is of minority. Column (3) adds city-level controls, including the logarithm of GDP per capital, the logarithm of government expenditure, the logarithm of population, and the GDP share of secondary industry. Column (4) replaces household fixed effects with village fixed effects. MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

E.7 Test for the unit issue of dependent variable

Some recent econometrics studies point out that the arbitrary selection of dependent variable's unit (e.g., thousand and million) may affect the size and intepretability of estimates (Chen and Roth, 2024). One solution is to utilize the Poisson estimation, which statistically account for both the extensive margin (i.e., from 0 to 1) and intensive margin of dependent variable. This is presented in Appendix Table E.5. The Poisson estimation indicates a 11.2% increase in household total income, significant at 1% level. This is consistent with the baseline results (7.3%).

Table E.5: Unit issue of dependent variable

Dep. Var.	Total inc	
	(1) Baseline	(2) Poisson
Estimate	0.073**	0.112**
	(0.029)	(0.053)
Predicted effects		5815.273**
		(2746.759)
Year FE	Yes	Yes
Household FE	Yes	Yes
Controls	Yes	Yes
MDV	11.139	51461.47
Observations	149,042	149,042
Adjusted R^2	0.717	0.717

Note: This table examines the potential unit issue of dependent variable for TJAL's effects on household total income. The level of observation is the household-year. Column (1) replicates the results shown in Column (1) of Table E.4. Column (2) utilizes a Poisson estimation using total income as the dependent variable. I include year fixed effects, household fixed effects, household-level controls, and city-level controls, as described in Equation (1). MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

E.8 Alternative levels of cluster

Some recent studies demonstrates that the choice of clustering level can significantly impact standard error of estimates (de Chaisemartin and Ramirez-Cuellar, 2024; Abadie et al., 2023). To test the robustness of my findings, I re-estimate the standard errors by allowing them to be autocorrelated within each village or within each prefecture. Appendix Table E.6 presents the results, showing that the estimates remain consistent regardless of the clustering level employed. This robustness check ensures that the estimates are not sensitive to the choice of clustering level.

Table E.6: Alternative levels of cluster

Dep. Var.	Tot inc		
	(1) Prefecture	(2) Village	(3) Household
Estimate	0.073**	0.073***	0.073***
	(0.029)	(0.009)	(0.009)
Year FE	Yes	Yes	Yes
Household FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
MDV	11.139	11.139	11.139
Observations	149,042	149,042	149,042
Adjusted R^2	0.717	0.717	0.717

Note: This table examines alternative cluster levels for TJAL's effects on household total income. The level of observation is the household-year. Column (1) replicates the results shown in Column (1) of Table E.4. I include year fixed effects, household fixed effects, household-level controls, and city-level controls, as described in Equation (1). MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level for Column (1), village level for Column (2), and household level for Column (3). *** p<0.01, ** p<0.05, * p<0.1.

E.9 Test for SUTVA

While the impacts of TJAL on village-to-city migration are discussed before, migration itself could lead to a violation of SUTVA. This could bias the estimates, depending on how TJAL influences the composition of migration (Carlitz et al., 2024; Ma and Mu, 2020; de Janvry et al., 2015).

Table E.7: Whether TJAL leads to attrition?

Dep. Var.	Dummy=1 if a household is not a complete Panel structure	
	(1)	(2)
TJAL	0.002	0.002
	(0.003)	(0.003)
Year FE	Yes	Yes
Village FE	Yes	Yes
Controls	No	Yes
Observations	149,042	149,042
Adjusted R^2	0.698	0.707

Note: This table examines whether TJAL leads to attrition. I include year fixed effects, village fixed effects in Column (1), and add household-level controls, and city-level controls, as described in Equation (1), in Column (2). Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

Fortunately, the NFPS structure ensures that over 90% of households maintain a Panel structure throughout the sampling periods. This enables me to conduct two tests for SUTVA assumption. First, I define a dummy=1 if a household does not have a complete Panel structure and re-estimate Equation (1). The estimates of TJAL indicator shown in Appendix Table E.7 are insignificant and close to zero, suggesting that TJAL does not affect the attrition of NFPS households. Second, I restrict the sample to households with complete panels to 2015 and rerun Equation (1). Appendix Table E.8 demonstrates that estimated effects remain virtually unchanged before (0.072951) and after restricting the sample (0.072997), lending robust support to the main analysis.[34]

[34]The minor impacts of migration selection are expected due to the unique clan culture in China. The average annual migration rate is merely 1.28%, with a median of 0.6%.

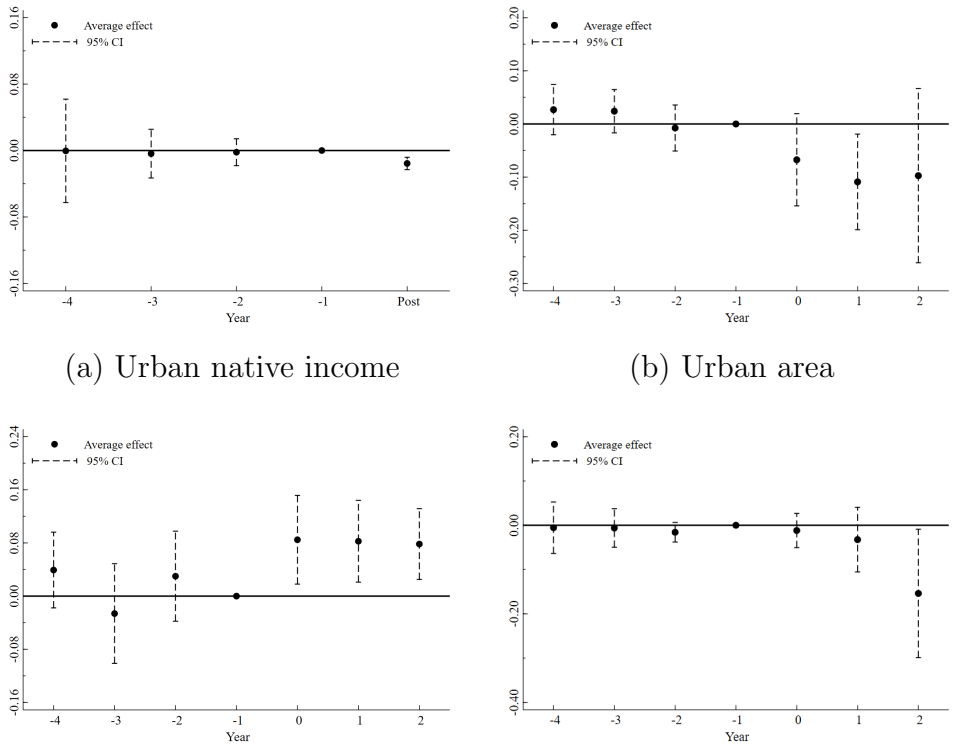
Table E.8: Test for SUTVA

Dep. Var.	Total inc	
	(1) Baseline	(2) Panel
TJAL	0.072951** (0.029)	0.072997** (0.028)
Year FE	Yes	Yes
Household FE	Yes	Yes
Controls	Yes	Yes
MDV	11.139	11.139
Observations	149,042	136,449
Adjusted R^2	0.717	0.717

Note: This table examines the potential violation of SUTVA. The level of observation is the household-year. Column (1) replicates the results shown in Column (1) of Table E.4. Column (2) restricts the sample to households that remain in the sample until 2015. I include year fixed effects, household fixed effects, household-level controls, and city-level controls, as described in Equation (1). MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

F Additional results for urban responses

F.1 Dynamics of TJAL's impacts on urban outcomes



(a) Urban native income

(b) Urban area

(c) Land price

(d) Industrial output

Figure F.1: Parallel trend test for urban outcomes

Note: This figure presents the dynamic effects of TJAL on urban outcomes. The level of observation is the household-year for Panel F.1a; city-year for Panels F.1b and F.1d; the city-usage-year-month for Panel F.1c. I include year fixed effects, household fixed effects, and household- and prefecture-level controls (as described in Equation (1)) for Panel F.1a; year fixed effects, prefecture fixed effects, and the IHS transformation of GDP per capita in 2012 interacted with year fixed effects for Panels F.1b and F.1d; usage-year fixed effects, usage-city fixed effects, and prefecture-level controls for Pane F.1c. Robust standard errors are in parentheses and are clustered at the prefecture level except for Panel F.1a at the household level due to the small number of clusters. The 95% confidence intervals are calculated based on standard errors.

F.2 TJAL's impacts on the number of service firms

Table F.1: The impact of TJAL on the number of service firms

Dep. Var.	(1) Firm entry	(2) Firm exit	(3) Annual change	(4) Firm number
TJAL	0.077***	0.182***	0.056**	0.048***
	(0.020)	(0.032)	(0.023)	(0.013)
Controls	Yes	Yes	Yes	Yes
AVMDV	417.387	67.840	349.547	1869.246
MDV	3.167	1.586	3.021	4.723
Observations	240,618	240,618	240,618	240,618
Adjusted R^2	0.779	0.763	0.749	0.800

Note: This table reports the DD estimates of TJAL's impacts on the firm number of services. The level of observation is the industry-prefecture-year. The specification follows: $Y_{i,p,t} = \alpha_0 + \alpha_1 TJAL_{p,t} + \zeta_{i,t} + \eta_{i,p} + GDPPC_{p,2012} \times \zeta_t + \epsilon_{i,p,t}$, where $Y_{i,p,t}$ denotes the outcome variables for industry i of prefecture p in year t; $\zeta_{i,t}$ and $\eta_{i,p}$ are industry-year and industry-city fixed effects; $GDPPC_{p,2012} \times \zeta_t$ denotes the IHS transformation of GDP per capita interacted with year fixed effects; $\epsilon_{i,p,t}$ is the error term; the rest terms are defined the same as in Equation (1). The dependent variables are the number of firm entries Column (1); the number of firm exits for Column (2); the annual change of firm number (i.e., firm entry–firm exit) for Column (3); the number of active firms for Column (4), all in IHS transformations. Controls are defined accordingly in the specifications. AVMDV = the absolute value of the mean of the dependent variable.MDV = mean of the dependent variable. Robust standard errors are in parentheses and are clustered at the prefecture level. *** p<0.01, ** p<0.05, * p<0.1.

G Additional information and results for general equilibrium analysis

G.1 Deriviation of labor wage in urban sector

Given the optimal labor wage level, take the factor inputs ($L^* = \bar{\theta}N + N_u$; $H^* = hN_u$; $T^* = c + \mu\bar{\theta}N$) inside, we have:

$$w_l^* = \gamma(\bar{\theta}N + N_u)^{\gamma-1}(hN_u)^{\eta}(c + \mu\bar{\theta}N)^{1-\gamma-\eta}$$

Given euqation 2, the labor wage level in urban sector is implicitly determined by:

$$\frac{w_l^*}{(\frac{N}{1-\alpha+\mu\alpha}w_l^* + N_u)^{\gamma-1}(c + \mu\frac{N}{1-\alpha+\mu\alpha}w_l^*)^{1-\gamma-\eta}} = \gamma(hN_u)^{\eta}$$

G.2 Average production of farming rural households

Conditioning on participating in farm work, that is, $\theta \in U[\bar{\theta}, 1]$ and $f(\theta) = \frac{1}{1-\bar{\theta}}$. There are a total of $N(1-\mu\bar{\theta})$ units of land for farm work. To maximize total output, land should be allocated efficiently. Consider the following maximization problem:

$$\max_{t_i} Y_{agri} = (1-\bar{\theta})N \int_{\bar{\theta}}^{1} \theta t(\theta)^{\alpha} f(\theta) d\theta$$

where $\int_{\bar{\theta}}^{1} \theta t(\theta)^{\alpha} f(\theta) d\theta$ is the average output of farming households. This is subject to the land resource constraints:

$$(1-\bar{\theta})N \int_{\bar{\theta}}^{1} t(\theta) f(\theta) d\theta = N(1-\mu\bar{\theta})$$

Define the Lagrangian as:

$$\mathcal{L} = N\{\int_{\bar{\theta}}^{1} \theta t(\theta)^{\alpha} d\theta - \lambda[\int_{\bar{\theta}}^{1} t(\theta) d\theta - (1-\mu\bar{\theta})]\}$$

Take the first-order conditions w.r.t. $t(\theta)$:

$$\frac{\partial \mathcal{L}}{\partial t(\theta)} = \alpha\theta t(\theta)^{\alpha-1} - \lambda = 0$$

This can be simplified to:

$$t(\theta) = \frac{\lambda}{\alpha}^{\frac{1}{\alpha-1}} \theta^{\frac{1}{1-\alpha}}$$

By taking this into the the land constraint, I have:

$$(\frac{\lambda}{\alpha})^{\frac{1}{\alpha-1}} \int_{\bar{\theta}}^{1} \theta^{\frac{1}{1-\alpha}} d\theta = (1-\mu\bar{\theta})$$

which is equivalent to:

$$(\frac{\lambda}{\alpha})^{\frac{1}{\alpha-1}} = \frac{(1-\mu\bar{\theta})}{\int_{\bar{\theta}}^{1} \theta^{\frac{1}{1-\alpha}} d\theta}$$

Then, the aggregate production follows:

$$Y_{agri} = N(\frac{\lambda}{\alpha})^{\frac{\alpha}{\alpha-1}} \int_{\bar{\theta}}^{1} \theta^{\frac{1}{1-\alpha}} d\theta$$

$$Y_{agri} = \frac{N(1-\mu\bar{\theta})^{\alpha}}{(\int_{\bar{\theta}}^{1} \theta^{\frac{1}{1-\alpha}} d\theta)^{\alpha-1}}$$

Hence, the averge output of farming households follow:

$$y_{agri} = \frac{Y_{agri}}{(1-\bar{\theta})N} = \frac{(1-\mu\bar{\theta})^{\alpha}}{(1-\bar{\theta})(\int_{\bar{\theta}}^{1} \theta^{\frac{1}{1-\alpha}} d\theta)^{\alpha-1}}$$

G.3 Parameterization of urban production function

Table G.1: Estimates of urban production function

Dep. Var.	ln(GDP per capita)				
Sample year	(1) 2008	(2) 2009	(3) 2010	(4) 2011	(5) 2012
ln(Average years of schooling)	0.671**	0.615**	0.604**	0.628**	0.587**
	(0.282)	(0.283)	(0.299)	(0.133)	(0.134)
ln(Prefecture area per capita)	0.216***	0.265***	0.266**	0.159**	0.146**
	(0.051)	(0.058)	(0.051)	(0.024)	(0.021)
Observations	280	280	280	280	280
Adjusted R^2	0.137	0.126	0.139	0.041	0.041

Note: This table reports the estimates for model parameterization. The level of observation is the prefecture. The dependent variable is the logarithm of GDP per capita. Land lost is definied as those not driven by returning farmland to forest or by rent. I include the logarithm of average years of schooling and the logarithm of prefecture area per capita as independent variables. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

G.4 Sensitivity check

Table G.2: Sensitivity check of model setup

	(1) –20%	(2) –10%	(3) Baseline	(4) +10%	(5) +20%
Panel A. Off-farm work paricipation					
α	0.013	0.017	0.022	0.028	0.035
γ	0.015	0.018	0.022	0.028	0.037
η	0.012	0.016	0.022	0.030	0.039
κ	0.022	0.022	0.022	0.022	0.022
Panel B. Income gap					
α	0.08	0.09	0.10	0.11	0.12
γ	0.09	0.09	0.10	0.10	0.10
η	0.08	0.09	0.10	0.10	0.10
κ	0.10	0.10	0.10	0.09	0.09

Note: This table reports the sensitivity check for simulated results. Each parameter is changed by a certain percent relative to the baseline, and re-simulates the impacts when expropriation risk decreases from 0.15 (baseline) to zero.