Institutional Deregulation, Local Conditions, and Firm Entry: Evidence from China's Commercial System Reform

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Abstract

We study China's commercial system reform in 2014, which greatly simplified the requirements for firm registration and market entry. Using a continuous difference-in-differences design, we exploit cross-city variation in baseline rural population share across 280 Chinese prefecture-level cities to identify heterogeneous reform effects on firm entry intensity in labor-intensive industries. The research indicates that the reform had stronger effects on firm entry intensity in labor-intensive industries in cities with higher baseline rural population share, highlighting the role of local labor-market structures in shaping the response to deregulation. Mechanism analysis points to a primary labor-reallocation channel associated with per capita rural disposable income: the reform effect is stronger where the opportunity cost of reallocating rural labor is lower. A complementary market access channel further amplifies the response in cities with better access to surrounding markets. These findings show that reducing entry barriers alone does not determine the magnitude of deregulation effects; the local labor and market environments in which reforms operate are also important.

Keywords: Entry deregulation, Reform Exposure, Firm entry

1 Introduction

The economic institution would change the quality of development in an economy at a very large content. Nowadays, the macroeconomic reform always change the economy through numerous microeconomic cells with a long and complex transmission mechanism, which lastly influence the macroeconomic indexes. firm entry is the fundamental force driving market vitality and resource reallocation. (Dunne et al., 1988; Foster et al., 2001). The creation of new firms is not only conducive to job creation and the cross-industry mobility of productive resources, but also shapes industrial transformation and long-term economic development (Decker et al., 2014; Sedláček and Sterk, 2017). However, in many economies, potential entrepreneurs face huge institutional barriers to entry, including complex administrative procedures, high registration threshold costs, and restrictions on location conditions. Although many countries have implemented institutional reforms aimed at reducing barriers to entry for firms, a fundamental question remains: to what extent can the reduction in institutional entry costs translate into actual entry for firms, and how does the effectiveness of such reforms depend on local economic conditions?

A growing literature examines how the institutional environment shapes firm dynamics. A central insight in this literature is that entry decisions depend on both market opportunities and the institutional costs of starting a business. Regulatory restrictions, licensing requirements, and administrative procedures raise fixed entry costs, increase uncertainty, and deter potential entrepreneurs from entering. Specifically, entry regulations raise the institutional costs of business creation. This constrains entrepreneurial activity across countries and industries. The adverse effects are especially strong in industries where entry shapes competitive dynamics (Djankov et al., 2002; Klapper et al., 2006). Higher market entry costs will also affect

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overall economic performance. They limit the market entry of firms and the reallocation of resources in the market (Barseghyan, 2008). In addition to the differences in entry costs between countries, local restrictions on market entry will also distort the competitive environment, specifically by inhibiting firm entry, weakening competitive pressures, and slowing employment growth (Bertrand and Kramarz, 2002; Schivardi and Viviano, 2011; Fattal-Jaef, 2022). On a broader level, institutional distortions favor incumbent firms, reducing the intensity of competition, thereby limiting market entry and weakening economic vitality (Gutiérrez and Philippon, 2018, 2019).

A related strand of research explored whether reducing institutional barriers to entry through regulatory reform can stimulate entrepreneurial activity. Early evidence from specific regulatory reforms shows that simplifying business registration procedures and relaxing licensing restrictions lowers the administrative costs faced by potential entrants and thereby promotes firm entry. (Bruhn, 2011; Kaplan et al., 2011; Rostam-Afschar, 2014; Branstetter et al., 2014). The latest evidence from large-scale institutional reforms further highlights the broad impact of market entry deregulation on the dynamics of emerging market firms. In particular, China's commercial system reform, while reducing registration costs and simplifying entry procedures, not only promotes the entry of enterprises, but also increases the productivity of new entrants by intensifying competition and relaxing constraints (Barwick et al., 2025). Relatedly, removing minimum capital requirements promotes entrepreneurship by reducing financial barriers faced by potential entrepreneurs (Cui and Wei, 2021; Cheng et al., 2024). However, existing studies have primarily focused on estimating the average effects of institutional reforms on entrepreneurship and firm entry (Aghion et al., 2008; Monteiro and Assunção, 2012; Yakovlev and Zhuravskaya, 2013). Much less is known about how pre-existing regional economic structures determine the extent to which firms can respond to opportunities created by institutional reform. This problem is particularly important in developing economies, because these economies often have significant differences in market access, factor availability, and industrial composition (Amiti and Smarzynska Javorcik, 2008; Restuccia and Rogerson, 2017). With the simultaneous reduction of market entry barriers at the national level, there may be significant differences in the ability to transform institutional improvement into actual enterprise creation due to the heterogeneity of different regions. Why do some regions benefit more from the same institutional reform?

This paper studies China's commercial system reform, a large-scale institutional reform that transformed the regulatory environment for firm creation. Beginning with pilot programs between 2012 and 2014 and subsequently expanded nationwide, the reform simplified registration procedures, reduced administrative burdens, and lowered institutional barriers faced by potential entrants. This institutional setting provides an opportunity to examine not only whether institutional improvements promote entrepreneurial activities, but also why the same reform generates heterogeneous responses across regions with different economic structures. A central challenge in evaluating nationwide institutional reforms is the absence of clearly defined untreated regions. Because the reform was implemented nationwide, conventional difference-in-differences approaches based on treated and untreated comparisons cannot directly capture the heterogeneous consequences of the reform. Instead, this paper exploits predetermined differences in regional economic structures before the reform to examine how local conditions shape responses to a common institutional shock. Specifically, I construct a continuous measure based on pre-reform rural population share, capturing cross-regional differences in structural transformation potential and labor reallocation capacity. Regions with larger rural population share before the reform had greater potential for reallocating labor from rural to non-rural activities, allowing reductions in institutional entry barriers to generate stronger entrepreneurial responses.

Based on the above discussion, this paper asks a broader question: why do identical institutional reforms generate different firm entry responses across regions? Reducing entry barriers lowers the fixed costs associated with formal business creation and expands entrepreneurial opportunities, but institutional improvements alone do not guarantee that these opportunities will be realized. The extent to which regions translate reduced entry costs into actual firm creation depends on complementary local conditions, including market opportunities, access to demand, and the availability of production factors. In this sense, institutional reforms create new opportunities for economic activity, while regional economic environments determine the capacity to absorb and transform these opportunities into entrepreneurial outcomes.

To investigate this question, I combine prefecture-level data on newly registered firms of the four labor-intensive industries examined in this study with regional economic characteristics and employ a continuous treatment difference-in-differences framework. This approach (Callaway et al., 2024) exploits predetermined

cross-sectional variation in regional characteristics while controlling for city and year fixed effects, allowing the identification of differential firm entry responses associated with the nationwide reform.

Building on the mechanism highlighted by our general equilibrium model, we empirically show that lowering administrative entry barriers induces asymmetric labor-intensive firm entry depending on regional labor market conditions. Applying a continuous treatment DID design, we find that the 2014 reform significantly increased firm entry intensity, with regional responsiveness monotonically increasing in the baseline rural population share. The event-study analysis provides support for the parallel trends assumption prior to 2014 and reveals a persistent positive post-reform pattern. Consistent with our foregoing, these effects are concentrated in labor-intensive industries. In terms of economic magnitude, a one-standard-deviation higher baseline rural population share is associated with larger post-reform increases in firm entry density of 0.1205, 0.0500, and 0.1273 for low-end manufacturing, construction, and urban resident services, respectively.

We conduct a battery of robustness tests. Our findings are robust to alternative measures of reform exposure, excluding special-status cities, and restricting the sample to alternative estimation windows. We also conduct other robustness tests that further substantiate our identification strategy. Specifically, we account for differential pre-reform characteristics and conduct placebo tests, which provide no evidence of spurious treatment effects prior to the reform. Beyond estimating the average effects of the reform, we examine the conditions under which institutional deregulation generates stronger entrepreneurial responses. Specifically, we investigate whether pre-existing factor availability and market access shape the extent to which reductions in entry barriers translate into new firm creation. We find that the effects of the reform are more pronounced in regions with greater rural labor supply capacity and stronger market access, suggesting that institutional reforms are more effective when complementary local conditions facilitate resource reallocation and market expansion.

This paper makes three main contributions to the literature.

First, this paper contributes to the literature on institutional deregulation and entrepreneurship by documenting that the effects of regulatory deregulation are not uniform across industries. While existing studies have largely focused on the aggregate effects of entry deregulation, we examine how a common institutional shock affects labor-intensive industries that are particularly sensitive to entry barriers. Using a continuous difference-in-differences design that exploits cross-city variation in baseline rural population share, we show that commercial system reform generated stronger increases in firm entry intensity in labor-intensive industries where local labor supply provides complementary conditions for business formation.

Second, this paper further reveals how local economic conditions affect the effect of deregulation on firm entry, thus enriching the literature on institutional reform and firm entry. Although reducing regulatory barriers can reduce the cost of market entry, the scale of firm entry depends largely on the elasticity and cost of local labor supply. Specifically, we find that local rural disposable income plays a key role through the opportunity cost channel of labor supply. In areas with a low rural disposable income base, the opportunity cost for rural labor to abandon existing low-income economic activities is also significantly lower. This not only brings higher labor supply elasticity, but also forms a more accessible and cost-effective local labor pool, which greatly reduces recruitment friction and amplifies the stimulating effect of deregulation on firm entry. In addition, research shows that market access (MA) constitutes a crucial complementary channel: lower labor opportunity costs ensure access to economic factors of production, while The superior market access conditions enable new firms to obtain economic ties with the broader regional market. Taken together, these findings suggest that when institutional deregulation is combined with lower local labor opportunity costs and favorable market access, it can generate the strongest firm-entry response.

Third, this paper enriches the literature in the field of spatial economics and provides methodological improvements for reform evaluations using continuous DID designs. At the methodological level, this paper clearly defines the conceptual boundary between marginal response to baseline exposure and overall reform effect, thus constructing a rigorous framework for assessing continuous reform exposure. At the empirical level, by applying the framework to the spatial dimension, we reveal significant spatial nonlinear characteristics: those non-riverside hinterland cities show a much higher marginal response to benchmark labor exposure due to the lack of cross-regional population siphon effect and high-income employment substitution opportunities than historical industrial hub cities along the Yangtze River. This finding confirms that the marginal returns to regulatory liberalization are highest in geographically isolated hinterlands, where baseline rural labor faces the lowest opportunity costs and constitutes the primary lifeline for business cre-

ation.

Overall, this paper demonstrates that the economic dividends of nation-wide institutional deregulation are not automatically realized, but are fundamentally governed by the local capacity to convert administrative relief into firm creation. By identifying the critical interplay between entry deregulation, local labor opportunity costs, and market access, our findings reveal that institutional reform yields the highest marginal returns precisely where structural factor bottlenecks are most severely relieved. Consequently, evaluating institutional reform requires moving beyond uniform reform shock to account for local factor supply elasticities, providing essential insights for designing targeted regional strategies that foster broader and more spatially balanced economic dynamism.

2 Theoretical Framework

To establish the micro-foundation for our empirical identification strategy, we construct a general equilibrium firm-entry model with heterogeneous labor market frictions. The model illustrates how a institutional reform lowering administrative entry barriers interacts with regional exposure (measured by the baseline agricultural population share) to induce asymmetric firm entry across regions.

2.1 Model

Consider an economy consisting of regions $i \in \{1, \ldots, I\}$ and industries $j \in \{1, \ldots, J\}$. Time is continuous, indexed by t.

Assumption 1 (Production Technology and Firm Optimization) *In region i and industry j, prospective entering firms operate under a Cobb-Douglas production technology with labor l_{ijt} as the primary factor input:*

$$Y_{ijt} = A_j l_{ijt}^{\alpha_j} \tag{1}$$

where $A_j > 0$ represents industry-level total factor productivity, and $\alpha_j \in (0, 1)$ denotes the output elasticity of labor. The product price $P_j > 0$ is determined in the competitive national market.

Assumption 2 (Institutional Reform and Administrative Entry Barrier) *Starting a firm requires entrepreneurs to pay an administrative sunk entry costs $\tau(D_t) > 0$. The commercial system reform shock $D_t \in \{0, 1\}$ (where $D_t = 1$ indicates the post-reform period) reduces administrative entry barriers:*

$$\tau(D_t) = \tau_0 - \tau_1 D_t, \quad \text{with } \tau_0 > \tau_1 > 0 \tag{2}$$

where $\frac{d\tau(D_t)}{dD_t} = -\tau_1 < 0$ captures the policy-driven decline in administrative friction.

Assumption 3 (Regional Labor Supply and Rural Labor Share Exposure) *Non-rural labor is sourced locally from transitioning rural workers. The aggregate non-rural labor supply L_{it}^S in region i at time t is specified as:*

$$L_{it}^S = \left(\frac{w_{Rit}}{w_A}\right)^{\frac{1}{\gamma}} \cdot \exp\left(\frac{\lambda}{\gamma} D_t E_i\right) \tag{3}$$

where $E_i \in (0, 1)$ represents regional baseline reform exposure (the baseline rural population share); $w_A > 0$ denotes the exogenous rural reservation wage; $w_{Rit} > 0$ is the endogenous market-clearing wage in region i[1]; $1/\gamma > 0$ is the labor supply elasticity; and $\lambda > 0$ reflects the efficiency parameter of reform-unlocked labor mobility.

[1]We adopt the rural wage (w_R) as the relevant factor price in the firm's optimization problem for two key institutional reasons. First, urban wages (w_U) are subject to institutional constraints, such as minimum wage regulations and formal-sector wage rigidity, making them relatively sticky and insensitive to firm entry ($\frac{\partial w_U}{\partial N} \approx 0$). Second, newly registered firms, which are examine predominantly labor-intensive in our research, primarily recruit marginal workers from the rural pool. Consequently, the rural wage w_R more accurately reflects the true marginal opportunity cost of labor faced by prospective entrepreneurs at the entry stage.

2.2 Firm Profit and Labor Market Equilibrium

Solving the firm's static profit maximization problem yields the maximized operating profit:

$$\pi^*_{ijt} = \Omega_j w_{Rit}^{-\kappa_j} \tag{4}$$

where $\Omega_j \equiv (1-\alpha_j)\alpha_j^{\kappa_j}(P_j A_j)^{\frac{1}{1-\alpha_j}} > 0$ is an industry-specific productivity constant, and $\kappa_j \equiv \frac{\alpha_j}{1-\alpha_j} > 0$ denotes the profit elasticity with respect to factor costs. Local labor market clearing requires aggregate regional labor supply to equal total labor demand across all J industries ($L^S_{it} = \sum_{j=1}^{J} N^*_{ijt} l^*_{ijt}$).

Lemma 1 (Equilibrium Transition Wage) *By clearing the regional labor market, the endogenous equilibrium wage w^*_{Rit} faced by entering firms in region i is uniquely determined by:*

$$w^*_{Rit} = \bar{w} \cdot \left(\sum_{j=1}^{J} \theta_j N^*_{ijt} \right)^{\Gamma} \cdot \exp\left(-\lambda^* D_t E_i\right) \tag{5}$$

where $\bar{w} \equiv w_A^{\frac{\Gamma}{\gamma}} > 0$, $\Gamma \equiv \left(\frac{1}{\gamma} + \frac{1}{1-\tilde{\alpha}}\right)^{-1} > 0$, and $\lambda^* \equiv \frac{\lambda\Gamma}{\gamma} > 0$.

Economic Intuition of Lemma 1: The term $\sum_{j=1}^{J} \theta_j N^*_{ijt}$ captures localized wage escalation driven by market competition for labor as total firms expand. Crucially, the exponential term $\exp(-\lambda^* D_t E_i)$ illustrates the core mechanism: in regions with high baseline exposure (E_i), institutional reform ($D_t = 1$) unlocks a larger latent labor pool, thereby systematically dampening wage inflation pressure caused by firm entry.

2.3 Free-Entry General Equilibrium and Entry Intensity

Forward-looking prospective entrepreneurs enter region i and industry j until operating profits equal statutory administrative entry costs under the zero-profit condition $\frac{\pi^*_{ijt}}{r} = \tau(D_t)$, where $r > 0$ is the discount rate. Substituting the equilibrium wage from Lemma 1 yields the total equilibrium firm entry N^*_{ijt}.

To eliminate scale effects associated with regional economic size, we normalize entry scale by regional lagged GDP ($GDP_{i,t-1} > 0$), defining the aggregate **Firm Entry Intensity** as $y^*_{ijt} \equiv \frac{N^*_{ijt}}{GDP_{i,t-1}}$:

$$y^*_{ijt} \equiv \frac{N^*_{ijt}}{GDP_{i,t-1}} = \frac{\tilde{C}_j}{GDP_{i,t-1}} \cdot [\tau(D_t)]^{-\frac{1}{\Gamma\kappa_j}} \cdot \exp\left(\frac{\lambda^*}{\Gamma} D_t E_i\right) \tag{6}$$

where $\tilde{C}_j \equiv \left(\frac{\Omega_j \bar{w}^{-\kappa_j}}{r}\right)^{\frac{1}{\Gamma\kappa_j}} > 0$ captures industry-specific baseline firm entry technology.

2.4 Comparative Statics and Empirical Mapping

Differentiating the equilibrium of firm entry intensity y^*_{ijt} with respect to the reform shock D_t and regional agricultural exposure E_i establishes the theoretical core of our empirical continuous DID specification.

Proposition 1 (Reform Impact on Firm Entry Intensity) *The institutional reform aimed at facilitating market entry significantly increased firm entry intensity, measured as the number of newly registered firms per unit of lagged local GDP:*

$$\frac{\partial y^*_{ijt}}{\partial D_t} = y^*_{ijt} \left[\frac{\tau_1}{\Gamma\kappa_j \tau(D_t)} + \left(\frac{\lambda^*}{\Gamma}\right) E_i \right] > 0 \tag{7}$$

Proposition 2 (Continuous DID Hypothesis) *The reform-induced expansion in firm entry intensity y^*_{ijt} is strictly increasing in regional baseline rural population share E_i:*

$$\frac{\partial^2 y^*_{ijt}}{\partial D_t \partial E_i} > 0 \tag{8}$$

Empirical Strategy Mapping: Proposition 2 directly links our theoretical general equilibrium model to the Continuous Difference-in-Differences (DID) empirical specification using normalized entry intensity:

$$\left(\frac{N_{ijt}}{GDP_{i,t-1}}\right) = \beta_0 + \beta_1(Post_t \times Exposure_i) + X'_{it}\Gamma + \mu_i + \lambda_t + \varepsilon_{ijt} \tag{9}$$

The positive theoretical interaction derivative ($\frac{\partial^2 y^*_{ijt}}{\partial D_t \partial E_i} > 0$) mapped to firm entry intensity provides a direct testable hypothesis that $\beta_1 > 0$.

Core Hypothesis *The commercial system reform significantly promotes the emergence of labor-intensive firms by lowering institutional entry barriers and unlocking rural labor supply. Specifically, regions with greater baseline exposure to the reform are expected to experience larger increases in firm entry intensity, while the magnitude of this response varies across industries owing to heterogeneous production structures and factor dependencies.*

3 Institutional background

3.1 China's Commercial System Reform

Historically, China 's commercial registration system is characterized by significant administrative barriers to market access. (World Bank Group, 2011; Barwick et al., 2025). Before the reform, entrepreneurs must complete a number of approval procedures, obtain various types of administrative licenses, and meet strict registered capital requirements before they can legally set up businesses. [2] These requirements not only increase the fixed costs of formal market access, but also cause institutional frictions. For potential entrepreneurs who are difficult to obtain sufficient funds and administrative resources, this burden is particularly heavy (Aghion et al., 2007).

A major reform of China's commercial registration system began in the early 2010s with the objective of reducing institutional barriers to firm creation and improving the business environment. The reform represented a fundamental shift from a pre-entry administrative approval system toward a registration-based system.[3] By simplifying registration procedures and reducing administrative restrictions, the reform lowered the institutional costs associated with establishing a new business.

A key component of the reform was the replacement of the registered capital paid-in system with a capital subscription system.[4] Before the reform, firms were required to meet minimum registered capital requirements and complete capital verification procedures before obtaining a business license. The reform eliminated minimum capital thresholds, relaxed contribution requirements, and removed mandatory capital verification, thereby reducing the financial requirements for business entry.

The reform also streamlined administrative procedures by reducing the number of required approvals and allowing firms to complete certain procedures after establishment. In addition, it replaced the costly annual inspection system with an annual reporting system supported by an electronic registration platform. These changes simplified compliance procedures and improved the access of formal business registration.

3.2 Nationwide Expansion and Institutional Shock

The commercial registration reform was initially introduced through pilot programs in Guangdong Province before being expanded nationwide. The pilot began in selected Guangdong cities in 2012 and was gradually extended to other cities during 2013 and early 2014.[5] In March 2014, the reform was implemented nationwide, transforming a regional experiment into a nationwide institutional change.

The gradual introduction of the reform in Guangdong reflected an experimental approach to institutional transformation. However, after the nationwide expansion, all prefecture-level cities were exposed to the

[2] See https://policy.mofcom.gov.cn/claw/clawContent.shtml?id=42320

[3] See https://www.ln.gov.cn/web/zwgkx/lnsrmzfgb/2014n/qk/2014n_dqq/gwywj/573DE2C3C46B461793700669B615A6C3/index.shtml

[4] See https://www.gjxfj.gov.cn/gjxfj/fgwj/flfg/webinfo/2014/05/1601761496739866.htm

[5]

same reform shock. This nationwide implementation creates a challenge for conventional difference-in-differences designs, as there is no clearly untreated group after the reform.

Instead, the nationwide nature of the reform provides an opportunity to examine whether regions with different pre-existing economic structures responded differently to the same institutional change. While all regions experienced reductions in entry barriers, their ability to transform these newly created opportunities into actual entrepreneurial activity may have differed substantially.

3.3 Commercial Reform, Labor Reallocation, and Labor-Intensive Firm Entry

The economic consequences of reducing entry barriers are unlikely to be uniform across regions and industries (Han and Wu, 2024; Su et al., 2026). In particular, labor-intensive industries may respond more strongly to institutional improvements because their production processes rely relatively heavily on local labor availability and the ability of workers to transition across industries.

Before the reform, there were significant differences in the economic structure of different regions in China. In some areas, the proportion of rural population is large, so it has greater potential to reallocate labor from the agricultural sector to non-agricultural industries. As institutional barriers to business start-ups are reduced, these regions are likely to experience a significant increase in labour-intensive entrepreneurial activity, as new businesses are able to absorb labour released from the agricultural sector.

Therefore, the rural population share before the reform provides an indicator to measure the degree of regional reform. Areas with a higher rural agricultural share population face greater potential benefits due to lower entry costs, because institutional improvements can promote the transfer of labor resources to newly created non-agricultural firms.

This view emphasizes that institutional reform creates opportunities, but the regional economic structure determines the degree to which these opportunities are realized. By examining labor-intensive firm entry, this paper studies how the interaction between nationwide institutional reform and pre-existing labor market conditions shapes entrepreneurial responses across regions.

4 Data

The primary unit of analysis in this study is Chinese prefecture-level cities (hereafter, "cities"). Our sample covers 280 prefecture-level cities over the period 2008-2020. This study relies on data from multiple sources. First, the micro-level firm data encompassing province, prefecture-level city, ownership structure, primary and secondary industry classifications, and the number of newly registered firms are derived from CnOpenData. Second, city-level variables, including GDP, per capita GDP, average years of education, general public budget expenditure, year-end balances of deposits and loans are sourced from the China Statistical Yearbook, various provincial and prefecture-level statistical yearbooks, and statistical bulletins at respective administrative levels. Following Chen and Kung (2016), which exclude administrative units with superior governmental status to maintain institutional comparability, we remove municipalities directly under the central government from the prefecture-level city sample[6]. In addition, this study excludes Xizang, Xinjiang, and non-city prefecture-level administrative units (e.g., autonomous prefectures and leagues) due to differences in statistical coverage, urbanization patterns, and economic structures (Roberts et al., 2010).

4.1 Industry-Specific firm entry intensity relative to predetermined local economic capacity (FEI_{it})

To capture heterogeneous responses across labor-intensive industries, the industry-specific outcome variables are constructed separately at the city-year level using a common measurement procedure. The main four industries this paper considered are low-end manufacturing, construction, urban resident services and hospitality. Specifically, FEI_{ijt} is defined as the number of newly registered firms in industry j within city i at year t, normalized by lagged local GDP ($GDP_{i,t-1}$, per 100 million RMB). This measure captures the intensity of new firm formation relative to the city's predetermined economic capacity, rather than the absolute volume of business creation. The use of lagged GDP serves two purposes. First, it accounts for substantial

[6]This includes Beijing, Tianjin, Shanghai, and Chongqing.

differences in local economic scale across cities while avoiding the contemporaneous normalization problem that arises when GDP in the same period is potentially affected by the reform itself. Second, it aligns the outcome measure with the theoretical concept of firm entry intensity: namely, how many new firms are created given the existing economic capacity of a locality [7].

4.2 Baseline Treatment Exposure

As detailed in Section 3, the nationwide implementation of the amended Company Law in March 2014 lowered administrative entry barriers simultaneously across all Chinese municipalities. When evaluating the causal impacts of this reform, a standard binary treatment framework based on early local pilot status presents a severe identification challenge. Prior to the 2014 national rollout, commercial registration reforms were selectively piloted in economically developed coastal areas (such as Shenzhen, Dongguan, and selected cities in Zhejiang and Fujian). As documented by Wang and Yang (2025), local policy pilots in China are subject to non-random selection, as regions with higher economic development levels, stronger fiscal capacity, and superior transport access are preferentially chosen as pilot sites. Evaluating the reform using early pilot groupings would therefore confound the true policy effect with pre-existing regional economic advantages. To fully bypass localized pilot selection bias, we utilize the nationwide implementation of the reform in March 2014 as a uniform, exogenous temporal shock ($Post_t = 1$ for $t \geq 2014$). Following the continuous difference-in-differences framework pioneered by Duflo (2001) and Nunn and Qian (2011), we interact this uniform temporal shock with a predetermined measure of structural exposure: the baseline rural population share ($RuralShare_{i,base}$). Specifically, the continuous treatment exposure is defined as:

$$Treatment_{it} = RuralShare_{i,base} \times Post_t \tag{10}$$

$$RuralShare_{i,base} = \frac{1}{6} \sum_{t=2008}^{2013} RuralShare_{it} \tag{11}$$

where $RuralShare_{i,base}$ measures city i's baseline capacity to provide a low-cost labor buffer prior to the reform. Because baseline rural population shares are predetermined prior to the policy shock, this continuous DID formulation isolates cross-city variation in structural responsiveness while holding national policy timing constant, effectively eliminating selection bias associated with early reform pilots. where

4.3 Regional Characteristics

GDP per Capita (pgdp). GDP per capita could capture local market size and economic development. Higher GDP per capita may stimulate firm entry by expanding consumer demand, improving market opportunities, and enhancing local business conditions. Hence, following from Klapper and Love (2011) and Aidis et al. (2012), this study controls for GDP per capita, measured in logarithmic form.

Financial Development Level (fin). We measure regional financial development using the combined ratio of outstanding deposits and loans to GDP. The loan-to-GDP ratio captures credit availability and access to external finance, while the deposit-to-GDP ratio reflects savings mobilization capacity and local liquidity conditions. Given that new and small firms often face substantial initial capital constraints and depend on external financing, greater financial development is expected to facilitate firm creation (Beck et al., 2007; Hasan et al., 2015).

Average Years of Education (edu). Following recent studies , we control for regional human capital (average

[7] The normalization by lagged GDP is motivated by the objective of measuring firm entry intensity relative to local economic capacity. Local economic conditions, including the availability of customers, suppliers, labor, and agglomeration-related advantages, are closely related to the spatial distribution of new firm formation (e.g., Glaeser and Kerr (2009); Duranton and Puga (2004)). Accordingly, scaling new firm registrations by local economic scale allows the analysis to distinguish the intensity of business formation from differences in the absolute size of local economies. We use lagged rather than contemporaneous GDP to avoid normalizing current-period firm entry by an economic aggregate that may itself respond to the reform during the same period. The resulting measure therefore captures new firm formation relative to the economic capacity inherited from the preceding period. Although lagged GDP may gradually incorporate post-reform economic responses in later periods, this does not imply that the normalization itself constitutes a post-treatment control; rather, it determines the economic scale relative to which firm entry intensity is measured. The potential concern is instead whether reform-induced changes in economic scale differ systematically across exposure groups. We therefore assess the robustness of the results using non-normalized firm entry and alternative measures of local economic scale.

years of education) to account for local resource availability and entry quality. General human capital lowers barriers to opportunity recognition and resource mobilization (Marvel et al., 2016), while also boosting entering firms' initial size and productivity through knowledge spillovers (Queiró, 2022). Controlling for this variable isolates the confounding effects of local absorptive capacity and initial firm quality on firm entry.

Foreign Direct Investment (fdi). Measured as the natural logarithm of actual utilized FDI, controlling for FDI accounts for regional economic openness, foreign capital agglomeration, and market competition(Lu et al., 2017). not only expands the demand for intermediate goods and services from domestic suppliers (Smarzynska Javorcik, 2004), but also affects local entrepreneurial dynamics. De Backer and Sleuwaegen (2003) investigate the net effect of FDI on domestic entrepreneurship and firm entry, showing that FDI promotes the creation of domestic firms in the long run through demonstration effects and knowledge spillovers.

General public expenditure-to-GDP ratio (exp_gdp). Following related research, we control for government size, measured by general public expenditure as a percentage of GDP—to account for local fiscal capacity and state-provided public goods. Public expenditure serves as a productivity-enhancing input and provides essential infrastructure, which lowers operational risks, mitigates the negative wealth effect, and directly crowds in new firm entry (Deng et al., 2023; Lewis and Winkler, 2017). Controlling for this variable isolates regional baseline fiscal support and public resource availability from startup dynamics.

5 Empirical Strategy and Identification

To estimate the causal impact of the commercial system reform on the entry of labor-intensive firms, This study exploits the reform's nationwide implementation as a plausibly exogenous shock. We argue that the reform's marginal dividend varies across cities according to baseline rural labor endowments. Because labor-intensive firm formation relies heavily on local labor availability, regions with greater pre-reform rural population share exhibit higher baseline exposure and potential labor elasticity. Thus, we adopt a continuous exposure DID framework, interacting the post-reform indicator with the baseline rural population share to identify the reform-induced treatment heterogeneity. The first this study constructs the continuous DID regression model is as follows:

$$FEI_{it}^{j} = \alpha_0 + \beta^{j}(RuralShare_{i,base} \times Post_t) + \mathbf{X}_{it}'\boldsymbol{\theta} + \mu_i + \lambda_t + \varepsilon_{it}^{j} \tag{12}$$

where FEI_{it} denotes the firm entry intensity for industry j in city i and year t. The key explanatory variable is the interaction term $RuralShare_{i,base} \times Post_t$, where $Post_t$ is a dummy variable set to 1 for years $t \geq 2014$ and 0 otherwise, and $RuralShare_{i,base}$ measures the pre-reform baseline rural population share capturing local treatment exposure. X_{it} represents a vector of time-varying city-level control variables. μ_i and λ_t represent city fixed effects and year fixed effects, respectively, which subsume the main effects of $RuralShare_{i,base}$ and $Post_t$. ε_{it}^{j} is the idiosyncratic error term clustered at the city level. The key parameter of interest, β^{j}, captures how the impact of the nationwide reform varies with baseline rural labor endowments for industry j.

The causal interpretation of the key parameter β_1 relies on two core identification assumptions: the exogeneity of the baseline treatment exposure ($RuralShare_{i,base}$) and the parallel trends assumption. This study elaborates on the justification of exposure measure in Section 5.1, and outline the empirical verification of parallel trends, which is formally tested in Section 5.2.

5.1 Identification Strategy and Treatment Exposure Validity

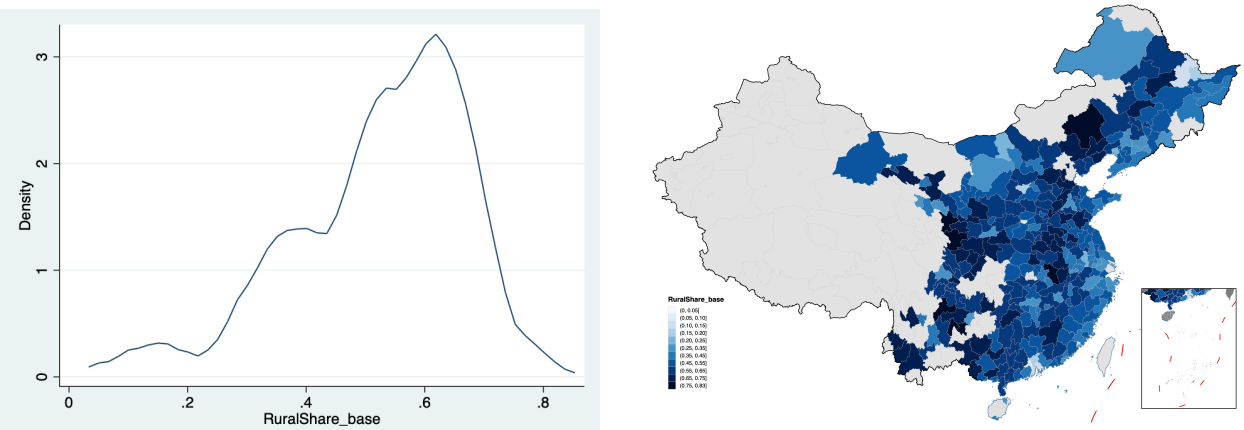
By lowering institutional barriers to firm entry, the reform was expected to stimulate new firm formation (Djankov et al., 2002; Klapper et al., 2006). Following recent developments in continuous treatment DID and exposure-based identification designs, we exploit predetermined cross-city variation in baseline rural population share interacted with a common institutional shock to identify heterogeneous reform effects (Goldsmith-Pinkham et al., 2020; Callaway et al., 2024). The interaction between the reform indicator and baseline rural population share captures heterogeneous exposure to the commercial system reform rather than merely differences in initial economic development. Conceptually, baseline rural population share captures cities' pre-reform potential for structural transformation, reflecting the scope for reallocating labor from agriculture to non-agricultural industries (LEWIS, 1954; McMillan and Rodrik, 2011). The choice of

baseline rural population share is motivated by the potential role of initial labor market structure in shaping local responses to a common reduction in institutional entry barriers. Cities with different rural population share may differ in their potential for labor reallocation and in the availability of labor for industries that rely relatively heavily on less-skilled workers. Baseline rural population share therefore provides a predetermined proxy for one dimension of cities' differential exposure to the reform, rather than a direct measure of the reform itself. Then, $RuralShare_{i,\text{base}}$ is constructed as the pre-reform average of rural population share from 2008 to 2013, freezing city-level exposure prior to the 2014 commercial system reform. Because the reform was a top-down, nationwide initiative mandated by the central authorities, its timing and scope were exogenous to individual cities (Lu et al., 2019). This temporal ordering mitigates concerns that post-reform firm entry outcomes could have directly influenced the baseline exposure measure. However, predetermination alone does not rule out anticipatory responses or confounding pre-existing trends. In addition, the average share of rural population from 2008 to 2013 can not only capture the persistent structural differences in local labor endowments, but also smooth the impact of short-term shock. Figure 1 plots the kernel density and geographic pattern of this measure, illustrating its continuous distribution and substantial variation across cities. This variation provides the empirical basis for our continuous treatment design, in which cities are exposed to the common reform shock to different exposure rather than classified into treated and untreated groups. Meanwhile, a key empirical concern is whether baseline exposure captures differential reform effects or merely proxies for pre-existing city-level trends. We explicitly consider the fact that the initial proportion of the rural population is not randomly distributed, because this indicator may reflect broader urban characteristics, such as income levels, financial development, industrial structure and others. In order to avoid these initial endowments lead to different development trajectories over time, we introduce the interaction between the initial characteristics of the city and the linear time trend in the empirical model. This flexible model setting allows cities with different initial characteristics to follow their own evolution trends, thus ensuring that our continuous DID estimator captures the impact of reforms rather than the structural differences that already exist (Angrist and Pischke, 2008; Freyaldenhoven et al., 2019). Finally, the effectiveness of our continuous DID design is based on the parallel trend hypothesis and a clear dose-response relationship. We provide intuitive and transparent reduced-form evidence for these recognition features in Fig 2 and Fig 3. In terms of spatial distribution, Figure 2 draws a spatio-temporal evolution map of the firm entry intensity (FEI) of low-end manufacturing industry between 2008 and 2019 (calculated on a three-year cycle) based on a representative major industry. The map shows that the firm entry intensity has maintained significant stability during the pre-reform period (2008-2013); after 2014, the FEI increased markedly and persistently, which provided descriptive evidence of changes in firm entry intensity following the reform. Finally, the figure 3 shows the distribution of four labor-intensive industries in the post-reform firm entry intensity growth (ΔFEI_{ij}), and is divided by the quartile of the degree of treatment ($Q1$ to $Q4$). Across the first three industries (Panels 3a–3c), the box plots display a clear, monotonic upward gradient, confirming that higher reform exposure translates into larger firm entry gains. In contrast, Panel 3d shows a muted pattern across exposure quartiles for the fourth industry, capturing meaningful cross-industry heterogeneity in reform responsiveness that we further explore in mechanism analysis. These reduced-form patterns, alongside dynamic event-study estimations in Section 5.2, substantiate our identifying assumptions.

5.2 Event Study Analysis

To assess the validity of the parallel trends assumption and examine the dynamic effects of the commercial system reform, we estimate an event-study specification that allows the differential effect of reform exposure to vary over time. Following the event-study framework commonly used in DID designs (Autor, 2003; Sun and Abraham, 2021), we replace the single post-reform interaction with year-specific interactions between baseline treatment exposure and year indicators.

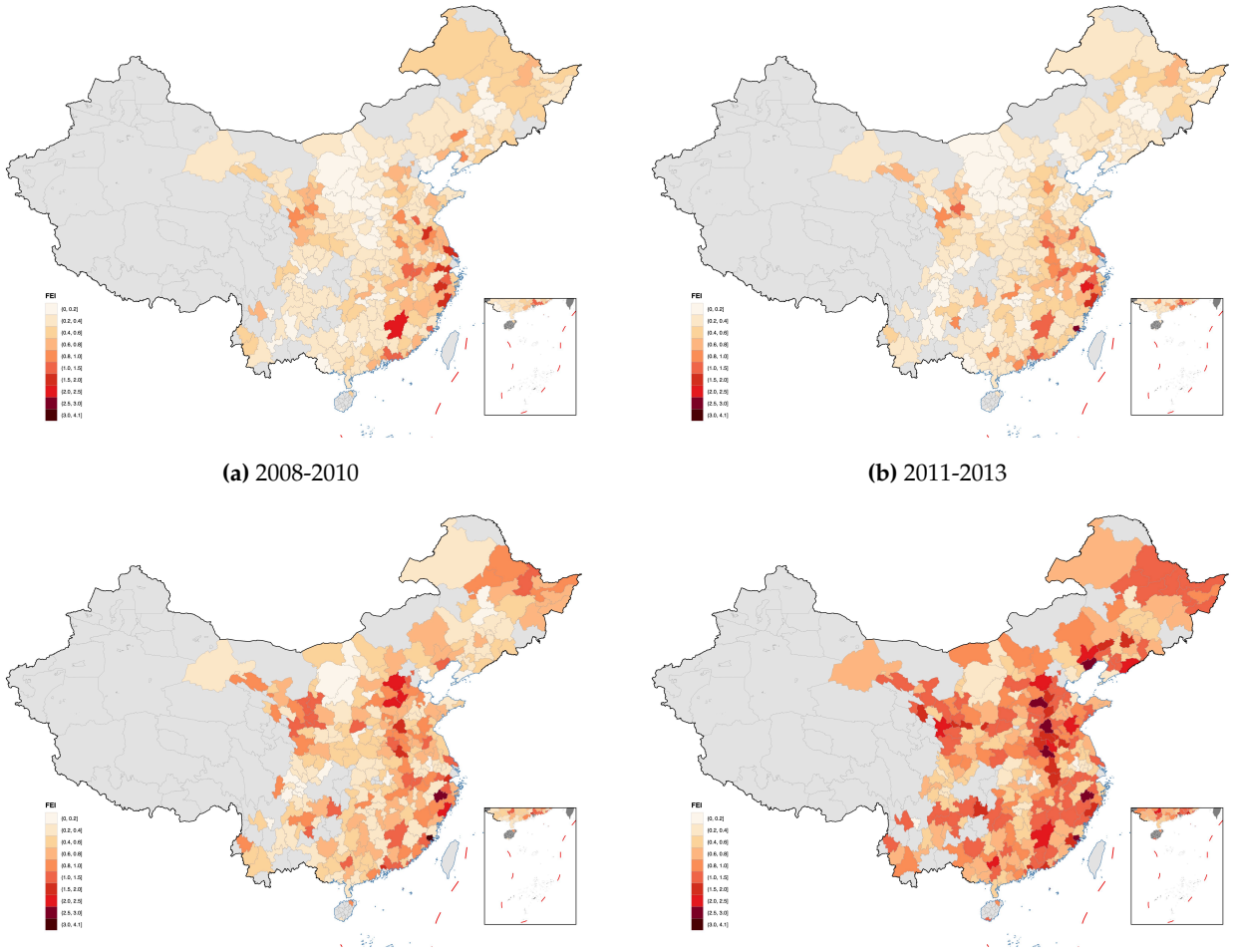
Unlike conventional DID designs that compare treated and untreated groups, our continuous DID framework exploits cross-sectional variation in predetermined treatment exposure interacted with a common institutional shock (Adão et al., 2019; Callaway et al., 2024). Therefore, the key identifying assumption is that cities with different levels of baseline rural population shares would have followed similar trends in the absence of the reform. The event-study specification allows us to directly examine this assumption by estimating whether differential trends existed before the reform and how the outcome evolved after the reform.



(a) Kernel Distribution Estimation

(b) Geographic Distribution

Figure 1: Kernel Density and Geographic Pattern of the Baseline Rural Population Share (Baseline Exposure). *Notes:* This figure illustrates the kernel distribution and geographic pattern of the baseline rural population share (averaged over 2008–2013) across Chinese cities. **Panel (a)** This figure presents the Epanechnikov kernel density estimation of the baseline rural population share (averaged over 2008–2013) across sample cities. The distribution spans continuously from 0.05 to 0.85, with a primary concentration centered between 0.50 and 0.70 (peak around 0.62) and a secondary shoulder around 0.40. This wide, smooth dispersion confirms robust continuous variation across cities without discrete truncation. **Panel (b)** maps the geographic distribution of baseline exposure (averaged over 2008–2013) across Chinese cities, categorized into discrete numerical intervals with darker shades representing higher baseline treatment exposure. The spatial pattern reveals substantial within-region and cross-city variation. Gray shaded areas represent regions excluded from the research sample. Data sources: National Bureau of Statistics of China and local statistical bureaus.



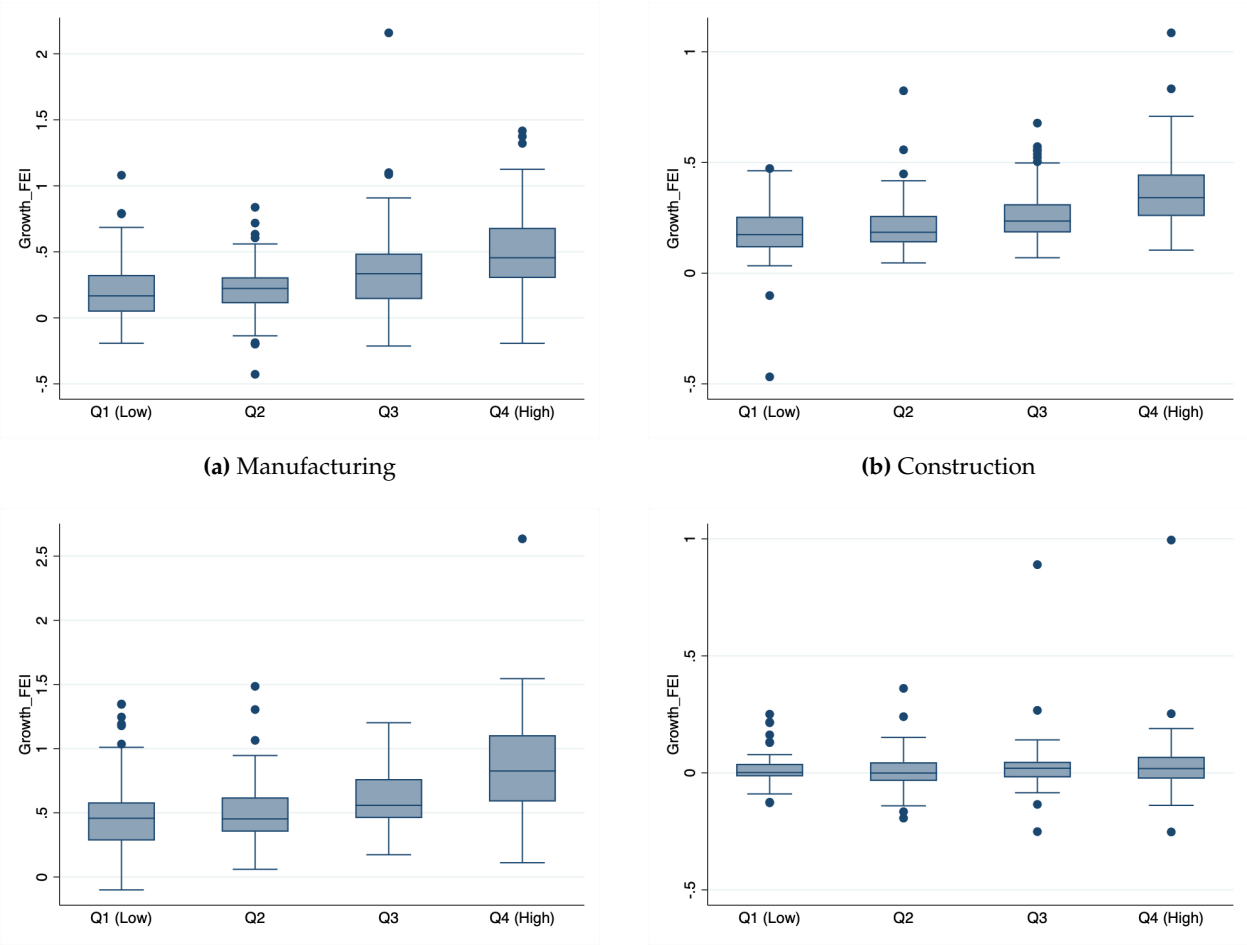
(a) 2008-2010

(b) 2011-2013

(c) 2014-2016

(d) 2017-2019

Figure 2: Geographic Distribution of Firm Entry Intensity. *Notes:* This figure maps the geographic distribution of the entry intensity of low-end manufacturing firms across Chinese cities, averaged over three-year intervals from 2008 to 2019, where the 2008–2013 period corresponds to the pre-reform baseline. Prior to the 2014 reform, firm entry intensity remained relatively stable across time while exhibiting substantial spatial heterogeneity across regions. Following the implementation of the reform in 2014, firm entry increased markedly but heterogeneously across cities during 2014–2016, with the increase persisting through 2017–2019. Data sources: National Bureau of Statistics of China and CnOpenData.



(a) Manufacturing

(b) Construction

(c) Urban Resident Services

(d) Hospitality

Figure 3: Reduced-form Dose-Response Distribution across Four Labor-Intensive Industries. *Notes:* Panels (a)–(d) present box plots of post-reform firm entry growth ($\text{Growth_FEI}_{ij} = \overline{FEI}_{ij,\text{post}} - \overline{FEI}_{ij,\text{pre}}$) across baseline treatment exposure quartiles ($Q1$ to $Q4$) for four labor-intensive industries, respectively. $\overline{FEI}_{ij,\text{pre}}$ and $\overline{FEI}_{ij,\text{post}}$ denote the average firm entry intensity for industry j in city i during 2008–2013 and 2014–2019, respectively. In each box plot, the horizontal line within the box indicates the median, the lower and upper hinges represent the 25th and 75th percentiles, the whiskers extend to $1.5 \times$ IQR, and solid dots mark outlying observations. Data sources: National Bureau of Statistics of China and CnOpenData.

Specifically, we estimate the following model:

$$FEI_{it}^{j} = \alpha + \beta_{k}^{j} \sum_{k=-6, k\neq -1}^{6} (RuralShare_{i,base} \times \mathbf{I}(t = t^{*} + k)) + \mathbf{X}_{it}'\boldsymbol{\theta} + \mu_i + \lambda_t + \varepsilon_{it}^{j} \tag{13}$$

where FEI_{it}^{j} denotes the firm entry intensity in industry j for city i in year t. $RuralShare_{i,base}$ measures the pre-reform baseline rural population share capturing local treatment exposure. $\mathbf{I}(t = t^{*} + k))$ is an indicator function that equals one if year t is k years relative to the reform year t^{*}, and zero otherwise. The coefficient β_{k}^{j} captures the differential change in firm entry intensity associated with a one-unit increase in baseline treatment exposure ($RuralShare_{i,base}$) at event time k for industry j. X_{it} represents a vector of time-varying city-level control variables. μ_i and λ_t denote city fixed effects and year fixed effects, respectively. ε_{it} is the idiosyncratic error term clustered at the city level.

Figure 4 presents the event-study estimates of the effect of the commercial system reform. The four subfigures correspond to the four labor-intensive industries examined in the analysis. Across all four industries, the pre-reform coefficients are small and statistically indistinguishable from zero, providing no evidence of differential pre-trends in firm entry intensity across cities with different levels of baseline rural population share. This pattern is consistent with the parallel-trends assumption underlying our identification strategy. Following the implementation of the reform, the coefficients become positive and statistically significant for the first three industries (Figures 4a–4c), indicating that cities with higher baseline exposure experienced larger increases in firm entry intensity in these industries. In contrast, the post-reform coefficients for the fourth industry (Figure 4d) remain statistically indistinguishable from zero, providing little evidence of a reform-induced increase in firm entry intensity in this industry.

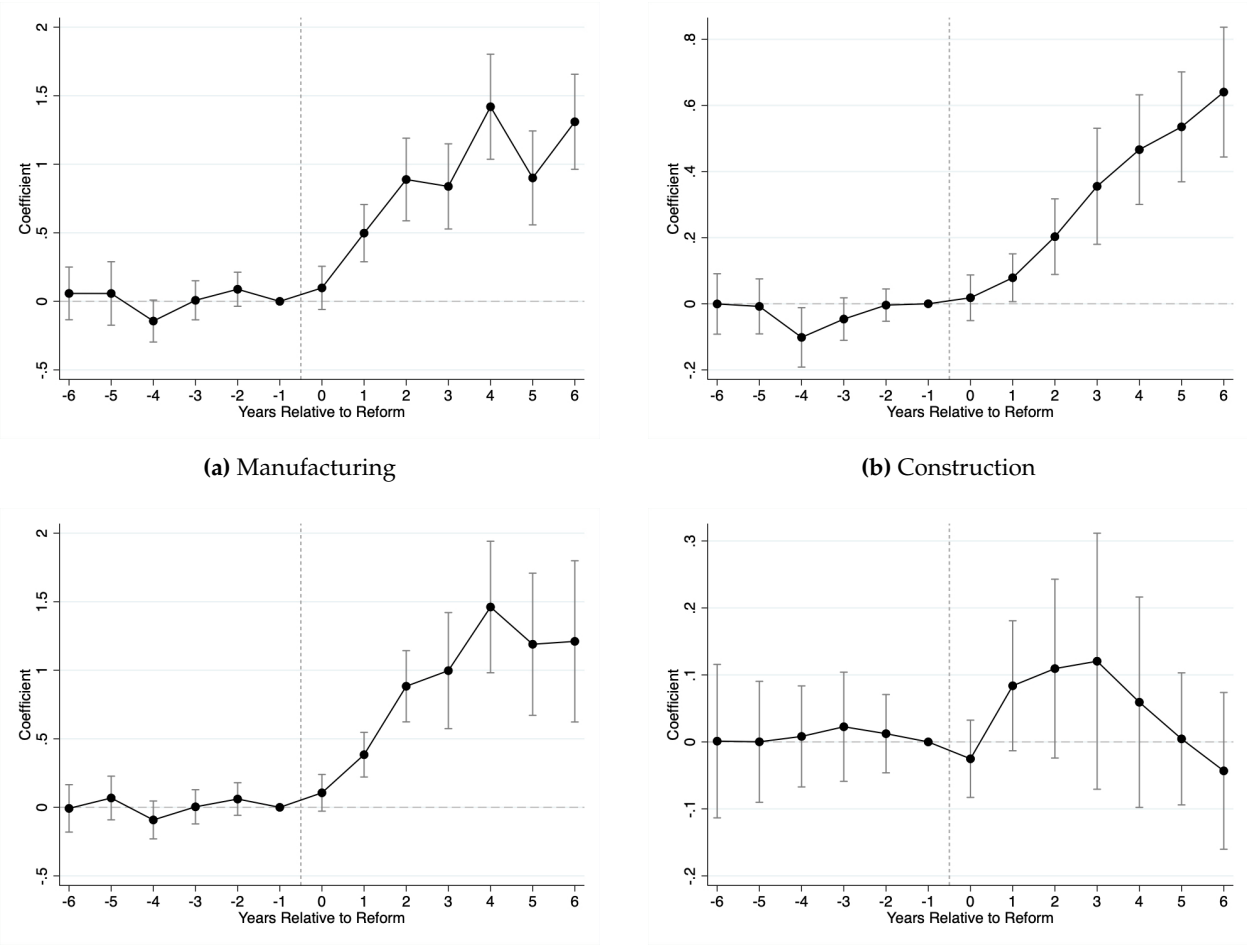
6 Results

6.1 Baseline

Table 1 collects the baseline results. We examine the effects of commercial system reform on firm entry intensity across four industries: low-end manufacturing, construction, urban resident services, and hospitality. First, columns (1)–(2), (5)–(6), (9)–(10), and (13)–(14) reveal a consistent pattern across all four industries. Reassuringly, the pre-reform interaction coefficients remain close to zero and statistically indistinguishable from zero across specifications, regardless of whether control variables are included. These results provide no evidence of differential pre-trends across cities with different levels of baseline exposure, consistent with the parallel-trends assumption. Following the reform, the dynamic effects differ across industries. For the first three industries, the post-reform coefficients become positive and statistically significant and increase in magnitude over time, indicating that the effect of the reform strengthens over time. In contrast, the coefficients for the fourth industry remain statistically indistinguishable from zero throughout the post-reform period.

Second, The coefficients for 3 industries (low-end manufacturing, construction and urban resident services) in column (4), (8) and (12) are statistically significant at the 1% level. The estimated coefficients of the interaction terms between pre-reform treatment exposure and the post-reform indicator are 0.816, 0.341, and 0.862 for the first three industries, respectively. These estimates suggest that the commercial system reform generated larger increases in firm entry intensity in cities with greater pre-reform rural population share exposure. Given that the standard deviation of pre-reform rural population share is 0.1477, a one-standard-deviation higher level of exposure is associated with an additional post-reform increase in firm entry intensity of 0.1205, 0.050, and 0.1273 units, respectively. These industry-specific estimates and their 95% confidence intervals are graphically presented in Figure **??**. Moreover, additional increases are equivalent to approximately 19.73%, 17.43%, and 16.46% of the corresponding sample means of firm entry intensity, indicating sizable economic effects of the reform across the first three industries. The dynamic treatment effects estimated from the event-study specifications further reveal how the effects evolved over time.

Specifically, for models (2), (6), and (10), a one-standard-deviation increase in treatment intensity is associated with additional increases in FEI of 0.0734, 0.0117, and 0.0567 in 2015, respectively. These effects cor-



(a) Manufacturing

(b) Construction

(c) Urban Resident Services

(d) Hospitality

Figure 4: Event-study estimates of the effect of the commercial system reform on firm entry intensity. *Notes:* Panels (a)–(d) report the estimates for low-end manufacturing, construction, urban resident services and hospitality, respectively. The figure reports coefficients from the continuous DID event-study specification, where baseline rural population shares are interacted with year indicators to capture differential exposure to the reform. The omitted category is 2013. Vertical bars denote 95% confidence intervals based on standard errors clustered at the city level. All specifications include city and year fixed effects.

respond to approximately 12.02%, 4.07%, and 7.33% of the respective sample means of firm entry intensity. In 2017, the corresponding effects increase in FEI of 0.1238, 0.0524, and 0.1473, equivalent to approximately 20.27%, 18.28%, and 19.04% of the respective sample means. The larger post-reform effects observed in 2017 suggest that the positive impact of the reform strengthened over time.

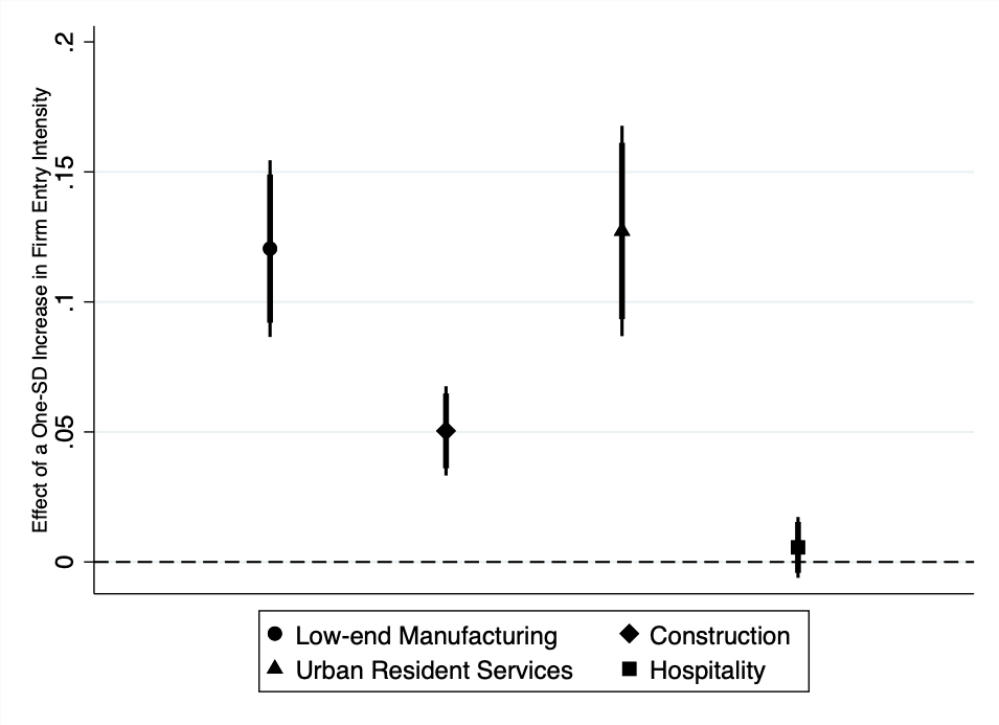


Figure 5: Effect of a One-Standard-Deviation Increase in Baseline Exposure on Firm Entry Intensity. *Notes:* This figure presents the estimated effects of a one-standard-deviation increase in baseline rural population share on firm entry intensity across the four industries. Points show point estimates; thick and thin vertical bars denote 90% and 95% confidence intervals, respectively. Estimates are based on the baseline continuous DID specification with city and year fixed effects and standard errors clustered by city.

6.2 Robustness

6.2.1 Controlling for Trends in Pre-Reform Characteristics

To address concerns that cities with different initial conditions may have followed different development trends prior to the commercial registration reform, we augment the baseline continuous DID specification by interacting standardized pre-reform municipal characteristics with a linear time trend. The estimated specification is as follows:

$$FEI_{it}^{j} = \alpha + \beta^{j}(RuralShare_{i,base} \times Post_t) + \sum_{m=1}^{M} \theta_m(Z_{im,0}^{std} \times Trend_t) + \mathbf{X}_{it}'\boldsymbol{\theta} + \mu_i + \mu_i + \lambda_j + \varepsilon_{it}^{j} \tag{14}$$

The notation follows that of the baseline specification. The linear time trend is defined as the sample year relative to 2008, and the additional interaction terms capture differential trends associated with pre-reform city-level characteristics. Specifically, $Z_{im,pre}^{std}$ denotes the standardized pre-reform average of the m-th city-level characteristic over 2008–2013, including baseline GDP per capita, average education years, FDI, general government expenditure-to-GDP ratio, and financial development. These interaction terms allow cities with different initial characteristics to follow heterogeneous linear trends over time. Including these differential-trend controls provides a more stringent test of the baseline identification strategy by accounting for po-

Table 1: Baseline Results: Commercial System Reform and Firm Entry Intensity

	Industry 1 (Low-end Manufacturing)				**Industry 2 (Construction)**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{RuralShare}_{\text{base}} \times \text{Post}$			0.730***	0.816***			0.407***	0.341***
			(0.118)	(0.117)			(0.060)	(0.059)
$\text{RuralShare}_{\text{base}} \times 2009$	-.021	0.057			-0.059	-0.008		
	(0.102)	(0.118)			(0.037)	(0.042)		
$\text{RuralShare}_{\text{base}} \times 2011$	-0.094	0.008			-0.057	-0.046		
	(0.067)	(0.072)			(0.032)	(0.033)		
$\text{RuralShare}_{\text{base}} \times 2015$	0.351***	0.497***			0.111***	0 .079**		
	(0.096)	(0.106)			(0.031)	(0.037)		
$\text{RuralShare}_{\text{base}} \times 2017$	0.688***	0.838***			0.404***	0.355***		
	(0.150)	(0.158)			(0.089)	(0.089)		
	Industry 3 (Urban Resident Services)				**Industry 4 (Hospitality)**			
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
$\text{RuralShare}_{\text{base}} \times \text{Post}$			0.804***	0.862***			0.061	0.038
			(0.147)	(0.139)			(0.043)	(0.040)
$\text{RuralShare}_{\text{base}} \times 2009$	-0.075	0.068			-0.062	0.0001		
	(0.068)	(0.081)			(0.037)	(0.046)		
$\text{RuralShare}_{\text{base}} \times 2011$	-0.110**	0.004			0.017	0.023		
	(0.053)	(0.063)			(0.039)	(0.041)		
$\text{RuralShare}_{\text{base}} \times 2015$	0.216***	0.384***			0.097**	0.084*		
	(0.061)	(0.083)			(0.048)	(0.049)		
$\text{RuralShare}_{\text{base}} \times 2017$	0.844***	0.997***			0.125	0.120		
	(0.214)	(0.215)			(0.094)	(0.097)		
Controls	No	Yes	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3,640	3,540	3,640	3,540	3,640	3,540	3,640	3,540

Notes: This table reports results from estimating the effect of agricultural population exposure on firm entry intensity following the reform. The regressions include the interaction between the agricultural population share and the post-reform indicator, along with city and year fixed effects. The 2013 served as the base year. Each sector is reported with two columns, presenting the results without and with controls, respectively. The models control for GDP per capita, foreign direct investment, financial development, average years of education and general public expenditure-to-GDP ratio. Standard errors clustered at the city level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

tential pre-existing differences in city-level development trends. The coefficient β^j therefore captures the differential effect of reform exposure after accounting for observable differences in pre-reform trends. All specifications include city fixed effects (μ_i) and year fixed effects (λ_t), which absorb time-invariant city characteristics and common time shock, respectively.

Table 1 reports the baseline estimates, while Table 2 presents the results after controlling for these differential trends. The coefficients of interest (0.324, 0.120, 0.303), capturing the interaction between reform exposure and the post-reform indicator, remain positive and statistically significant for industries 1–3 after accounting for potential differences in pre-existing trends. Although the estimated coefficients are smaller than those reported in the baseline specification in Table 2, they remain positive and statistically significant for industries 1–3. This attenuation is consistent with the fact that the augmented specification absorbs part of the variation associated with heterogeneous development trajectories across cities, suggesting that the baseline findings are not solely driven by pre-existing differences in municipal trends. The industry-specific pattern remains consistent with the baseline findings. In particular, the effects remain statistically insignificant for industry 4, suggesting that firm entry intensity in this industry was relatively less responsive to the reform shock. The corresponding coefficient estimates and their 90% and 95% confidence intervals are presented graphically in Figure 6.

Overall, these results indicate that the estimated reform effects cannot be fully explained by differential development trajectories associated with pre-reform municipal characteristics. After allowing cities with different initial conditions to follow distinct trends, the positive effect of reform exposure on firm entry intensity remains robust.

Table 2: Robustness I: Controlling for Differential Pre-Reform Trends

	Ind. 1 (1)	Ind. 2 (2)	Ind. 3 (3)	Ind. 4 (4)
$\text{RuralShare}_{\text{base}} \times \text{Post}$	0.324^{**}	0.120^{*}	0.303^{**}	0.062
	(0.126)	(0.067)	(0.131)	(0.043)
Pre-Reform Characteristics × Trend	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	3,540	3,540	3,540	3,540

Notes: This table presents the results from the continuous DID specification after controlling for differential trends associated with pre-reform city characteristics. All specifications include the full set of baseline control variables, as well as city and year fixed effects. The four columns groups correspond to the four industries; "Ind." denotes industry. Robust standard errors clustered at the city level are reported in parentheses. *** $p < 0.01$,** $p < 0.05$, * $p < 0.1$.

6.2.2 Placebo Test

Although the specification controlling for differential pre-reform trends alleviates concerns that cities with different initial characteristics may have followed different trends, the estimated effects could still potentially reflect spurious correlations between reform exposure and firm entry outcomes. To further assess the validity of the identification strategy, we conduct a permutation placebo test by randomly reallocating treatment exposure across cities while keeping the reform timing unchanged.

In terms of methodology, the method used in this study is based on the framework of randomized inference and permutation test initiated by Abadie et al. (2010). Under this framework, the distribution of the test statistics is derived by repeatedly randomly reassigning the processing states among the sample units. As emphasized by Roth et al. (2023) in a recent review of the difference-in-differences (DID) literature, conducting a placebo test is a key step to ensure that the baseline estimation results are not derived from unobservable confounding factors or mechanical human effects. Specifically, we follow the graphical implementation scheme and replacement strategy proposed by Hansen et al. (2017), randomly disrupt the

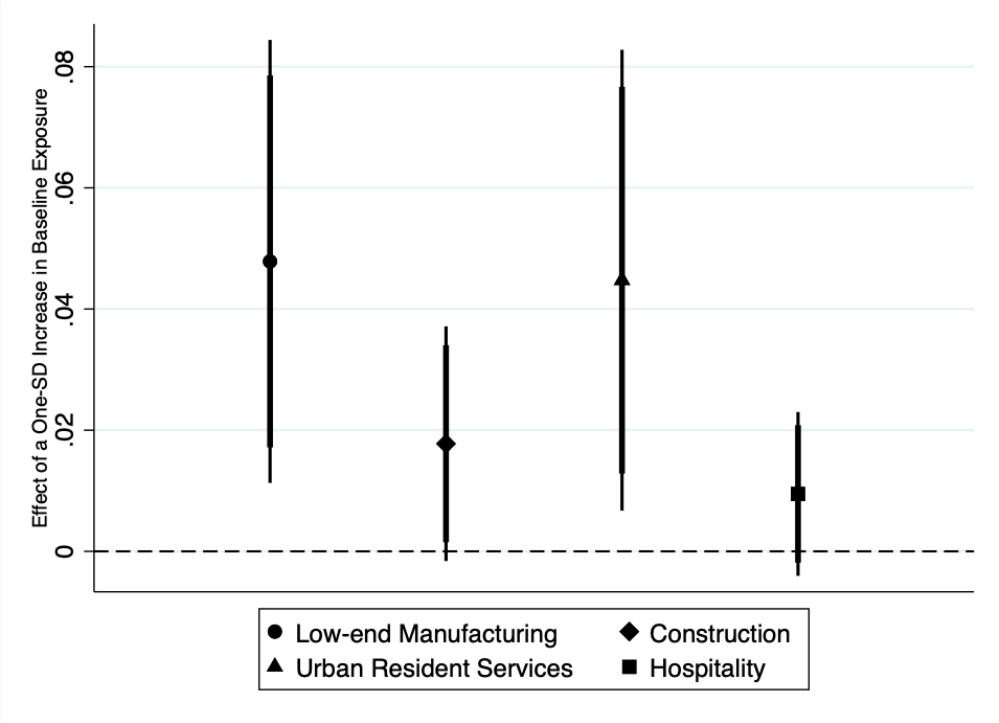


Figure 6: Effects of a One-Standard-Deviation Increase in Baseline Exposure after Controlling for Differential Pre-Reform Trends. *Notes:* This figure presents the estimated effects of a one-standard-deviation increase in baseline rural population share on firm entry intensity across the four industries, after controlling for differential trends associated with pre-reform city characteristics. Points denote point estimates, while thick and thin vertical bars represent 90% and 95% confidence intervals, respectively. Estimates are obtained from the continuous DID specification with city and year fixed effects and controls for interactions between pre-reform characteristics and a linear time trend. Standard errors are clustered at the city level.

processing state among cities, and perform 500 repeated estimates on the continuous DID model. This process not only retains the time node of panel data structure and business system reform, but also breaks the original relevance between treatment exposure and city-specific characteristics, allowing us to benchmark our true estimate against an empirical distribution generated under the null hypothesis. Table 3 presents the distribution of placebo estimates for industry 1-4. Overall, the placebo coefficients for the first three industries are centered around zero, with mean values of –0.0018, –0.0023, and 0.0047, and standard deviations of 0.1178, 0.0587, and 0.1066, respectively. These distributions are also illustrated in Figure 7. In contrast, the baseline estimates for these industries (0.816, 0.341, and 0.862) lie far outside the corresponding simulated placebo distributions. In these three industries, the estimated values of 500 placebos did not exceed the magnitude of the benchmark coefficient, and the resulting empirical p-value was less than 0.002. These results show that the positive effects of the first three industries are unlikely to be driven by the random correlation between reform exposure and firm entry intensity. For the fourth industry, although the placebo coefficient is also concentrated near zero, the benchmark estimate (0.038) falls within the range of simulated distribution. This finding is consistent with the benchmark regression results, that is, in the industry, the effect of reform exposure is not statistically significant. This model does not mean that the placebo test fails, but shows that there is no systematic difference between the reaction of the fourth industry to the reform exposure and the expectation in the case of random distribution, which further confirms the conclusion that the industry is less sensitive to business system reform.

Overall, the permutation placebo test provides further support for the validity of the identification strategy. The significant effects observed in the first three industries are unlikely to be driven by random exposure assignments, while the insignificant response in industry 4 remains consistent with the absence of a systematic reform effect.

Table 3: Robustness II: Placebo Test Results Across Different Industries

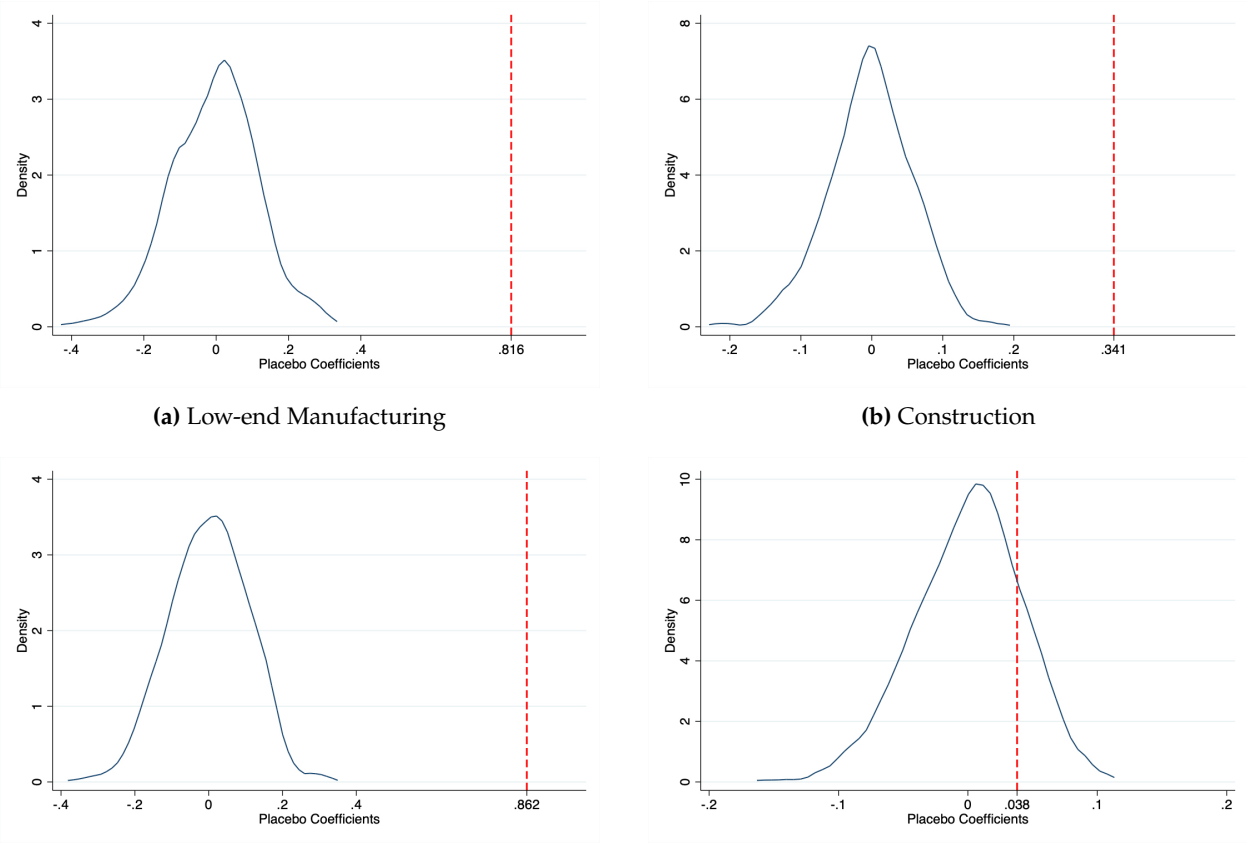
Industry / Sample	Obs	Mean	Std. Dev.	Min	Max	True β	Emp. p-value
Ind. 1 (Low-Skill Manufacturing)	500	-.0042	0.1178	-0.3983	0.3040	0.816	< 0.002
Ind. 2 (Construction)	500	-0.0023	0.0587	-0.2146	0.1801	0.341	< 0.002
Ind. 3 (Urban Resident Services)	500	0.005	0.1066	-0.3537	0.3219	0.862	< 0.002
Ind. 4 (Hospitality)	500	0.0001	0.0414	-0.1526	0.1027	0.038	$= 0.364$

Note: This table reports the summary statistics of the placebo tests (500 iterations) by randomly shuffling the exposure variable. "Emp. p-value" represents the proportion of placebo coefficients whose absolute values exceed or equal the absolute value of the true baseline coefficient. Min and Max denote the minimum and maximum values of the placebo coefficients, respectively.

6.2.3 Alternative the Exposure

Although the baseline exposure measure is constructed based on the original city-level exposure intensity, the estimated treatment effect may potentially be sensitive to the distributional characteristics and scaling of the exposure variable. In particular, extreme observations, differences in measurement units, and nonlinear transformations may affect the magnitude and precision of the estimated coefficients. Therefore, this section conducts a series of robustness checks by applying alternative transformations to the baseline exposure measure while preserving its underlying economic meaning.

First, we winsorize the exposure measure at the 1st and 99th percentiles to mitigate the potential influence of extreme observations. This transformation reduces the possibility that the estimated treatment effects are driven by a small number of cities with exceptionally high or low exposure intensity. Second, we replace the original exposure measure with its percentile rank. This approach preserves the relative ordering of cities according to their exposure intensity while eliminating concerns arising from differences in measurement scale and distributional dispersion. Third, we standardize the exposure variable with mean zero and unit variance. The standardized specification allows the coefficient to be interpreted in terms of the effect associated with a one-standard-deviation increase in exposure intensity and ensures that the results are not driven by the original measurement scale. Finally, we apply a logarithmic transformation to the exposure measure to account for potential nonlinear effects and reduce skewness in the distribution. This



(a) Low-end Manufacturing

(b) Construction

(c) Urban Resident Services

(d) Hospitality

Figure 7: Robustness II: Permutation placebo test. *Notes:* This figure presents the empirical distribution of placebo coefficient estimates obtained from repeated random permutations of baseline exposure across cities. The smooth curve represents the kernel density of the placebo estimates, while the red vertical line indicates the coefficient estimated from the actual baseline specification.

specification examines whether the baseline findings remain robust when the exposure intensity enters the model in a multiplicative form [8].

Table 4 reports the estimated effects of reform exposure under alternative transformations of the exposure measure. The specifications with the full set of control variables are emphasized in the following discussion, as they are comparable to the baseline model. For industry 1, industry 2, and industry 3, the estimated coefficients of the interaction term between reform exposure and the post-reform indicator remain statistically significant across all four alternative exposure specifications. The signs and magnitudes of the coefficients are broadly consistent with those obtained in the baseline regressions, suggesting that the estimated impact of the reform on firm entry in these industries is not sensitive to the functional form or scaling of the exposure measure. For industry 4, however, the coefficients remain statistically insignificant under all alternative exposure transformations. This result indicates that the lack of statistically significant evidence for the effect of reform exposure on firm entry in this sector is not driven by the original measurement or scaling of the exposure variable.

Table 4: Robustness III: Alternative Exposure Measures Across Four Industries

	Ind. 1		**Ind. 2**		**Ind. 3**		**Ind. 4**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winsor.RuralShare$_{base}$ × Post	0.734***	0.819***	0.409***	0.342***	0.809***	0.865***	0.061	0.037
	(0.119)	(0.117)	(0.061)	(0.059)	(0.148)	(0.140)	(0.043)	(0.040)
Rank.RuralShare$_{base}$ × Post	0.434***	0.482***	0.236***	0.205***	0.477***	0.501***	0.040	0.027
	(0.061)	(0.061)	(0.031)	(0.030)	(0.070)	(0.071)	(0.025)	(0.023)
Std.RuralShare$_{base}$ × Post	0.009***	0.121***	0 .061***	0.051***	0.119***	0.128***	0.009	0.006
	(0.006)	(0.017)	(0.009)	(0.009)	(0.022)	(0.021)	(0.006)	(0.006)
ln.RuralShare$_{base}$ × Post	1.006***	1.003***	0.560***	0.532***	1.100***	1.025***	0.080	0.037
	(0.173)	(0.160)	(0.088)	(0.082)	(0.217)	(0.179)	(0.060)	(0.054)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,640	3,540	3,640	3,540	3,640	3,540	3,640	3,540

Notes: This table reports robustness estimates using four alternative transformations of the reform exposure measure across four industries. For each sector, both specifications without control variables and specifications with the full set of baseline control variables are reported. The alternative exposure specifications are based on four transformations of the original exposure measure: 1% winsorization, percentile-rank transformation, standardization, and logarithmic transformation. Specifically, The percentile-rank specification transforms the original exposure measure into a normalized ranking variable ranging from 0 to 1, where higher values indicate a higher relative position in the exposure distribution. Therefore, the coefficient on the interaction term between the post-reform indicator and the rank-based exposure measure captures the difference in the outcome variable between cities at the highest and lowest exposure ranks after the reform. For example, a coefficient of 0.482 for industry 1 indicates that, following the reform, cities at the top of the exposure distribution experienced an increase of approximately 0.482 units in firm entry intensity compared with cities at the bottom of the distribution. In other words, a 10-percentage-point increase in the relative baseline exposure rank ($\Delta rank = 0.1$) is associated with an average change of 0.0482 ($0.1 \times \beta$) units in the marginal effect of the reform. Interpretations for industry 2 and industry 3 are similar to those for industry 1 and are omitted for brevity. Robust standard errors adjusted for clustering at the city level are reported in parentheses. *** $p < 0.01$,** $p < 0.05$, * $p < 0.1$.

6.2.4 Province-by-Year Fixed Effects

Although the baseline specification includes city fixed effects and year fixed effects, cities within the same province may still be exposed to common province-level shock that vary over time, such as regional development policies, provincial economic fluctuations, and other institutional changes. To address this concern, we further include province-by-year fixed effects, which allow each province to have its own time-specific shock and absorb unobserved province-level factors that evolve over time.

The results with province-by-year fixed effects are reported in Panel A of Table 5. The estimated coefficients of the interaction term between the post-reform indicator and treatment exposure of industry 1-3

[8]Here we introduce the concrete transformation methods of the exposure measure, also shown in Table 4 as the variables' name. The detailed calculate method see the Appendix.

remain stable in terms of both magnitude and statistical significance after controlling for these additional regional shocks. This finding suggests that the baseline results are unlikely to be driven by time-varying province-level confounding factors.

6.2.5 Alternative Sample Period and Excluding Cities with Special Administrative or Policy Status

We further examine whether the baseline findings are sensitive to the sample composition by conducting a series of sample restriction exercises. Specifically, we consider an alternative sample period and exclude cities with special administrative or policy characteristics that may differ systematically from ordinary prefecture-level cities.

First, the baseline sample covers the full observation period, which may capture various long-term structural changes occurring alongside the reform. To mitigate concerns that the estimated effects may be confounded by broader secular trends or other macroeconomic transformations, we restrict the sample to a narrower period surrounding the reform, specifically 2011–2017. As reported in Panel B of Table 5, across industries 1–3 the estimated coefficients remain consistent with the baseline results under the restricted sample period. This suggests that the documented effects are not driven by long-run trends or observations far from the reform implementation period.

Second, some cities in China have unique administrative or policy characteristics, which may lead to systematic differences in their economic operation compared with ordinary prefecture-level cities. For example, provincial capitals are often regional administrative and economic centers, while special economic zones and cities with separate plans enjoy relatively greater policy autonomy and preferential institutional arrangements. These characteristics may influence both reform exposure and firm entry outcomes. To address this concern, we re-estimate the baseline model after excluding provincial capital cities, special economic zones, and cities with independent planning status. The results remain qualitatively unchanged, indicating that the baseline findings are not driven by cities with exceptional administrative status or policy advantages

As reported in Panel C of Table 5, across industries 1–3 the estimated coefficients remain consistent with the baseline results under the restricted sample period. This suggests that the documented effects are not driven by long-run trends or observations far from the reform implementation period.

In contrast, the estimated coefficients for sector 4 remain statistically insignificant across these alternative specifications. This result suggests that the lack of statistically significant evidence in the baseline specification is not attributable to the inclusion of specific provinces, particular sample periods, or cities with special administrative or policy characteristics.

7 Mechanism I: Labor Reallocation Costs and the Reform Effect

The effect of the reform should depend not only on the institutional costs of firm entry but also on conditions in local factor markets, particularly the cost of reallocating labor across industries. Although this reform directly reduces the administrative, time and compliance costs of starting a business, the decision-making of entrepreneurs before entering the market ultimately depends on the expected production costs and factor prices. (Hopenhayn, 1992; Melitz, 2003; Bilbiie et al., 2012). In labor-intensive industries, the opportunity cost of drawing workers away from existing income-generating activities may therefore play a central role in determining how strongly lower entry barriers translate into new firm formation. Among these production overheads, labor availability and wage costs represent primary considerations prior to business registration (Poschke, 2013). Grounded in classic dual-economy frameworks (LEWIS, 1954; Ranis and Fei, 1961) and modern structural transformation literature (Gollin et al., 2014; Bryan and Morten, 2019; Lagakos, 2020; Pellegrina and Sotelo, 2021),rural per capita disposable income serves as an empirical proxy for the opportunity cost of rural labor transition. Economically, a lower baseline rural income reflects a lower reservation wage for rural workers, widening the shadow wage gap between farming and urban non-agricultural employment. This lower reservation wage reduces the migration barrier for rural labor, creating a more elastic supply of surplus labor for low-end manufacturing, construction and urban resident service industries. From the perspective of pre-decision-making, this feature of the labor market determines the operating environment expected by new entrants. In regions with lower baseline rural disposable incomes, prospective firms foresee lower recruitment costs, diminished labor-search frictions, and less upward

Table 5: Robustness IV: Province-by-Year Fixed Effects and Sample Specification

	Industry 1 Low-end Mfg.	**Industry 2 Construction**	**Industry 3 Urban Resident Services**	**Industry 4 Hospitality**
	(1)	(2)	(3)	(4)
Panel A				
RuralShare × Post	0.580***	0.196***	0.640***	0.017
	(0.101)	(0.054)	(0.133)	(0.031)
Province × Year	Yes	Yes	Yes	Yes
N	3,527	3,527	3,527	3,527
Panel B				
RuralShare × Post	0.527***	0.153***	0.517***	0.050
	(0.101)	(0.048)	(0.082)	(0.043)
N	1,932	1,932	1,932	1,932
Panel C				
RuralShare × Post	0.868***	0.416***	0.875***	0.079
	(0.158)	(0.072)	(0.185)	0.048
N	3,150	3,150	3,150	3,150
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: This table reports additional robustness checks based on alternative fixed-effect specifications and sample restrictions. Panel A incorporates province-by-year fixed effects, allowing each province to have its own time-specific shock and controlling for unobserved province-level factors that vary over time. Panel B restricts the sample period to 2011–2017 to examine whether the baseline results are sensitive to the choice of estimation window. Panel C excludes cities with special administrative or policy characteristics, including 25 provincial capital cities and 6 cities with either independent planning status or special economic zone status. These sample restrictions are implemented to assess whether the baseline findings are driven by specific groups of cities with distinctive administrative or policy characteristics. Robust standard errors adjusted for clustering at the city level are reported in parentheses. *** $p < 0.01$,** $p < 0.05$, * $p < 0.1$.

wage pressure. Consequently, a vital strategic complementarity arises between regulatory easing and labor supply elasticity: while commercial reform eliminates institutional entry barriers, a lower labor opportunity cost ensures that newly established firms can cost-effectively recruit workers, optimize factor allocation, and sustain post-entry profitability. We therefore hypothesize that the entry-promoting impact of the reform is more pronounced in cities with lower baseline rural disposable incomes.

To formally explore the underlying mechanism, we build upon our baseline design by introducing a triple interaction term that incorporates a moderating variable, $MA_{i,ctr}$. Specifically, we estimate the following specification:

$$\begin{aligned} FEI_{it}^{j} = \beta_0 &+ \beta_1^j (RuralShare_{i,base} \times Post_t \times MC_{i,ctr}) \\ &+ \beta_2^j (RuralShare_{i,base} \times Post_t) + \beta_3^j (Post_t \times MC_{i,ctr}) \\ &+ \mathbf{X}_{it}' \boldsymbol{\theta} + \mu_i + \lambda_t + \varepsilon_{it}^j. \end{aligned} \tag{15}$$

where $MC_{i,ctr}$ represents the mean-centered average per capita disposable income of rural residents in city i over the pre-reform period 2008–2013. The definitions of the remaining variables follow those given above. The key coefficient β_1 on the triple interaction term ($RuralShare_{i,base} \times Post_t \times MC_{i,ctr}$) captures how the marginal effect of baseline rural labor exposure on post-reform firm entry varies with the opportunity cost of rural labor reallocation, proxied by mean-centered rural residents' per capita disposable income. The specification also includes the constituent two-way interactions.

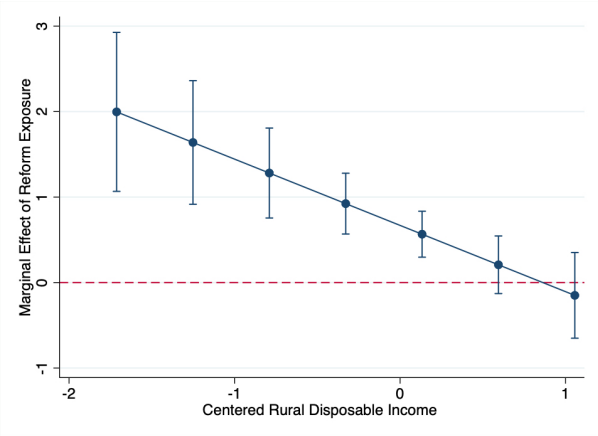
The estimation results for the internal labor market mechanism are presented in Table 6. Columns (1)–(4) report the estimates across different industries specifications. Crucially, the coefficients on the triple interaction term are negative and statistically significant at the 1% level across main specifications ($\beta_1^1 = -0.775, p < 0.01$; $\beta_1^2 = -0.498, p < 0.01$; $\beta_1^3 = -0.618, p < 0.01$), while reaching a smaller point estimate in Column (4) ($\beta_1^4 = -0.172$). Because rural disposable income is mean-centered at its sample mean, the negative triple-interaction coefficient has a straightforward economic interpretation: lower baseline rural income strengthens the positive effect of reform exposure on firm entry intensity. Quantitatively, taking the estimated triple interaction coefficient -0.775 as an example, the baseline of rural disposable income decreased by 1 unit relative to the sample mean, which was related to the increase of 0.775 units in the marginal effect of reform exposure on firm entry intensity. In order to intuitively capture this dynamic across the entire rural income range, the figure 8 maps the conditional marginal effects of baseline exposure of rural disposable income centered on the mean. As shown in the figure, the estimated marginal effect shows a clear downward trend, which directly reflects the negative triple interaction coefficient. When the baseline rural income level is low ($MC_{ctr} < 0.5$), the reform effect is strictly positive and statistically significant, because its 95% confidence interval is completely above the horizontal zero line. However, with the increase of rural income, the dividend of promoting market entry is monotonically decreasing. Once the rural disposable income exceeds the threshold of about 0.5 (centralized unit), the confidence interval will cross the horizontal zero line, resulting in no statistically significant difference between the marginal effect and zero.

From the perspective of pre-decision-making, this confirms that potential entrepreneurs expect to face lower wage costs and recruitment frictions when entering the market in areas with lower agricultural opportunity costs and retention wages. Therefore, in an environment with a large elasticity of surplus labor supply, the market access dividend brought by institutional deregulation is the most significant, which indicates that the internal labor market situation is the key transmission channel to promote this process.

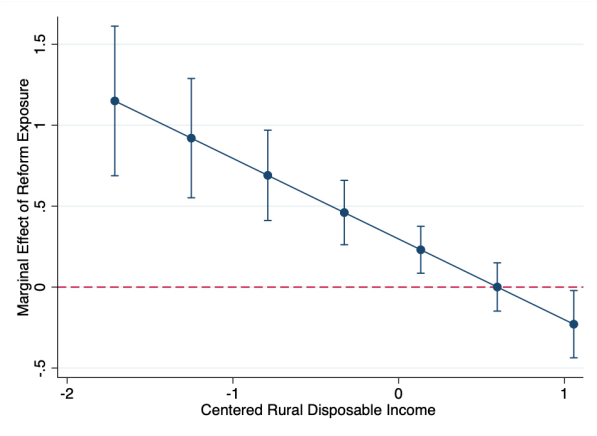
8 Mechanism II: The Market Access and the Reform Effects

In addition to the internal constraints brought about by labor reallocation costs, the effectiveness of institutional reforms may also depend on external market conditions. Although the reform of the business system has reduced the administrative barriers to enterprise access, newly registered enterprises still need to overcome space frictions, because these frictions restrict their ability to obtain markets, customers and business opportunities. Therefore, the difference in the benchmark level of market access may affect the degree to which the reduction of barriers to entry is translated into the creation of actual enterprises.

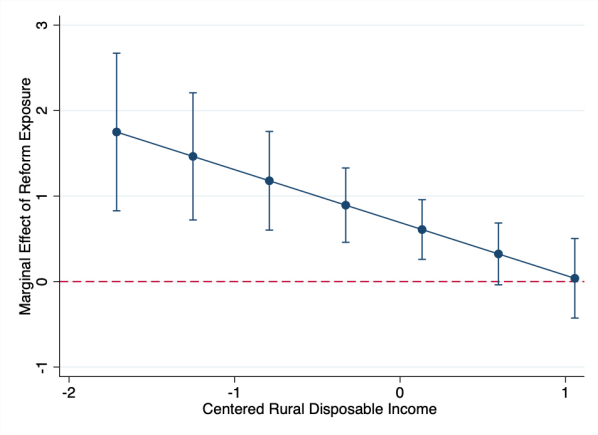
Drawing on insights from the new economic geography literature, we argue that market access serves as an important boundary condition determining the extent to which institutional reforms translate into



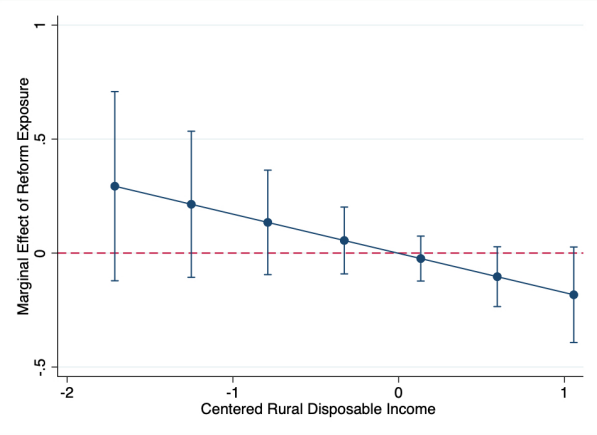
(a) Manufacturing



(b) Construction



(c) Urban Resident Services



(d) Hospitality

Figure 8: Industry-Specific Marginal Effects of Reform Exposure across Levels of Rural Residents' Disposable Income. *Notes:* This figure presents the marginal effects of reform exposure on firm entry intensity across the four labor-intensive industries evaluated at different levels of mean-centered pre-reform per capita disposable income of rural residents. The horizontal axis denotes the mean-centered income measure, and the vertical axis reports the estimated marginal effect of reform exposure. Dots indicate point estimates, and vertical lines represent 95 percent confidence intervals. The horizontal axis reports the corresponding values of the mean-centered income measure.

Table 6: Mechanism: Labor Reallocation Costs and the Reform Effect

	(1) Industry 1	(2) Industry 2	(3) Industry 3	(4) Industry 4
$\text{RuralShare}_{\text{base}} \times \text{Post} \times \text{MC}_{\text{ctr}}$	-0.775***	-0.498***	-0.618***	-0.172
	(0.242)	(0.110)	(0.216)	(0.110)
$\text{RuralShare}_{\text{base}} \times \text{Post}$	0.669***	0.297***	0.691***	-0.001
	(0.143)	(0.080)	(0.186)	(0.054)
$\text{MC}_{\text{ctr}} \times \text{Post}$	0.226	0.182***	0.142	0.046
	(0.143)	(0.068)	(0.135)	(0.055)
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	3,540	3,540	3,540	3,540

*Notes:*All specifications include the full set of baseline controls, city and year fixed effects. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

firm entry. Theoretically, greater market access expands firms' effective market size and raises the expected returns to entry by reducing spatial barriers to reaching consumers and suppliers (Redding and Venables, 2004; Donaldson and Hornbeck, 2016). Consistent with this view, recent empirical evidence shows that improvements in market access can stimulate local economic activity by strengthening firms' connections to external markets (Brooks and Donovan, 2020). Cities with better market access are more closely integrated with larger economic centers and face lower transaction costs, allowing newly registered firms to access broader markets and capture greater entry opportunities. Thus, market access complements institutional reforms by enabling reductions in entry barriers to translate more effectively into realized entrepreneurial activity. Accordingly, we expect the positive effect of the commercial system reform on firm entry to be stronger in cities with higher baseline market access.

To formally examine this spatial complementarity, we extend our baseline DID framework by incorporating pre-reform market access (MA) and estimate a triple-interaction specification. Following the market potential approach introduced by Harris (1954), we construct city-level market access as the sum of economic mass weighted by an exponential distance-decay function:

$$MA_i = \sum_{j \neq i} GDP_j \times \exp(-\beta d_{ij}),$$

where GDP_j captures the economic mass of surrounding cities and d_{ij} denotes the transportation distance between cities i and j[9]. β is the distance-decay parameter capturing the attenuation of market access with geographic distance. To mitigate concerns that contemporaneous economic changes may affect the measured access index, we construct (MA) based on pre-reform economic conditions and use the mean-centered baseline measure in the estimation.

Table 7 presents the estimates from the triple-interaction specification, which examines whether market access shapes the heterogeneous effects of the commercial system reform on firm entry intensity. The key triple interaction term, captures whether the reform-induced increase in firm entry intensity varies with pre-reform market access across cities with different levels of reform exposure. The results show that the coefficient of the triple interaction term is positive and statistically significant for industry 1 ($p < 0.01$) and industry 3 ($p < 0.1$). This finding indicates that market access strengthens the positive effect of commercial system reform on firm entry in the two industries. Taking Industry 1 as an example, the coefficient on the two-way interaction term ($\beta_2 = 0.817$, $p < 0.01$) captures the effect of reform exposure at the sample mean of market access ($MA_{ctr} = 0$). Thus, at the mean level of market access, a one-unit increase in baseline rural labor exposure is associated with a 0.817-unit increase in firm entry intensity following the reform. Then, the coefficient on the triple interaction term ($\beta_1 = 0.320$, $p < 0.01$) captures how baseline MA conditions the reform effect. A one-unit increase in baseline MA above its sample mean is associated with a 0.320-unit

[9]The geographical distance matrix data of prefecture-level cities are from PPData database.

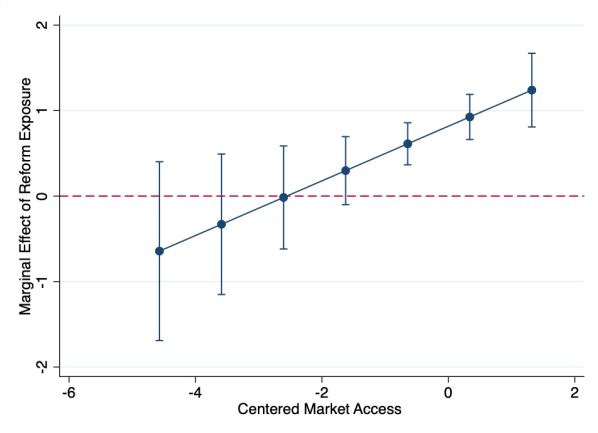
increase in the marginal effect of baseline exposure on firm entry intensity following the reform. The positive and statistically significant estimate indicates that greater market access reinforces the reform-induced response of firm entry intensity to local rural labor exposure. Economically, the positive β_1 indicates that a more favorable baseline MA strengthens the responsiveness of high-exposure cities to the reform. While the reform reduces institutional barriers to market entry, the extent to which these institutional gains translate into new firm entry depends on the surrounding market environment. Regions with better market access are characterized by larger potential demand, lower transaction costs, and stronger connections to external markets, allowing newly registered firms to more effectively exploit the opportunities created by the reform. Overall, the evidence suggests that there is a complementary relationship between institutional improvement and spatial advantage : better market access conditions amplify the impact of reform on local entrepreneurial activities.

The figure 9 shows the conditional marginal effect of reform exposure under different benchmark market access levels (MA_{ctr}). In the model setting for each industry, there is a positive adjustment mode, but there is also significant inter-industry heterogeneity: for industry 1, the triple interaction coefficient is highly significant ($p < 0.01$); for industry 3, the coefficient is marginally significant ($p < 0.10$); for industry 2 and industry 4, the coefficient is not statistically significant. As shown in industry 1 in Figure 9a, the upward sloping curve shows that market access significantly enhances the positive impact of business system reform on firm entry intensity. Specifically, the marginal effect of the estimation increases monotonically with market access, confirming the positive point estimation of the triple interaction term. The confidence interval reveals an obvious threshold pattern. When the market access level is low (about $MA_{ctr} < -1.6$), the estimated marginal effect is statistically indistinguishable from zero, with a 95% confidence interval of zero. Beyond this threshold, the marginal effect becomes statistically positive and further increases with market access. This pattern is consistent with a complementarity between institutional deregulation and market access. Although the reform lowers administrative barriers to market entry, these institutional changes may have limited effects where firms face substantial constraints in accessing markets. In contrast, cities with greater market access provide more favorable conditions for firms to capitalize on the opportunities created by deregulation, including access to downstream markets and production networks. Thus, the effectiveness of regulatory easing appears to depend not only on the reduction of institutional barriers but also on the market environment in which new firms operate.

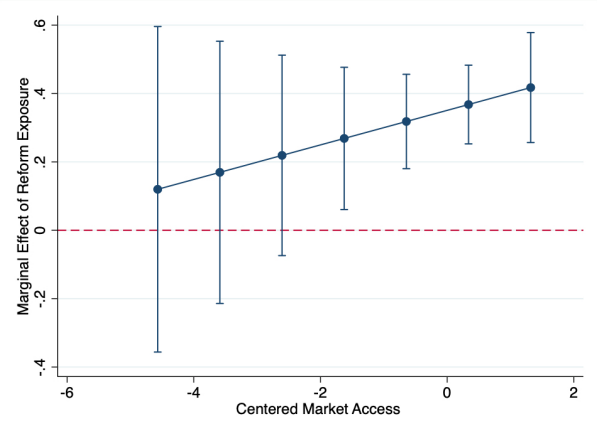
Table 7: The Mechanism II: he Market Access and the Reform Effects

	(1) Industry 1	(2) Industry 2	(3) Industry 3	(4) Industry 4
$\text{RuralShare}_{\text{base}} \times \text{Post} \times \text{MA}_{\text{ctr}}$	0.320***	0.051	0.216*	-0.067
	(0.120)	(0.050)	(0.117)	(0.078)
$\text{RuralShare}_{\text{base}} \times \text{Post}$	0.817***	0.351***	0.851***	0.040
	(0.119)	(0.059)	(0.139)	(0.038)
$\text{MA}_{\text{ctr}} \times \text{Post}$	-0.146**	0.010	-0.110*	0.020
	(0.062)	(0.026)	(0.062)	(0.035)
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	3,527	3,527	3,527	3,527

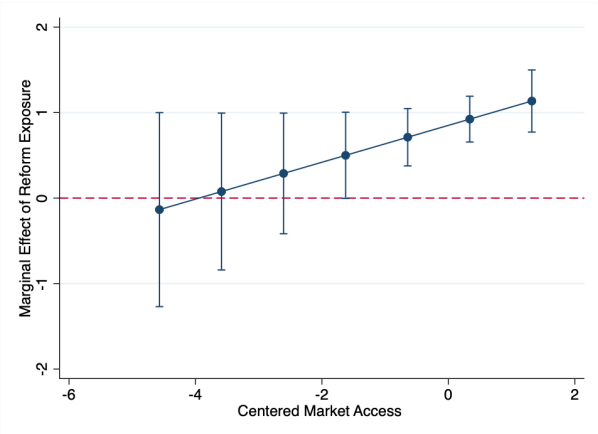
Notes: Industry-Specific Marginal Effects of Reform Exposure across market access. *Notes:* This table reports estimates from the continuous DID specification including the triple interaction ($\text{RuralShare}_{\text{base}} \times \text{Post} \times \text{MA}_{\text{ctr}}$), together with the constituent two-way interactions. MA_{ctr} denotes the mean-centered baseline market access measure constructed using pre-reform economic conditions. All specifications include the full set of baseline controls, city and year fixed effects. Standard errors, clustered at the city level, are reported in parentheses. Robust Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.



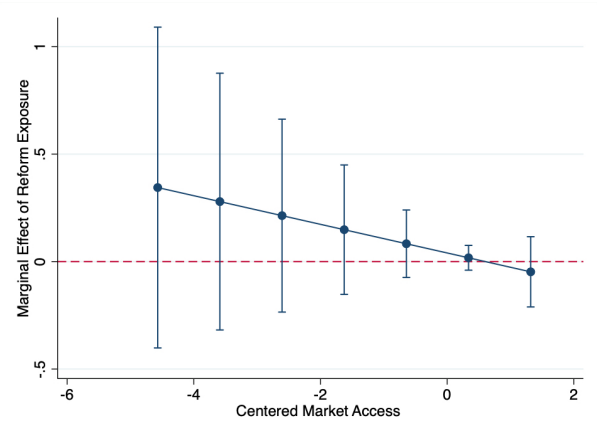
(a) Manufacturing



(b) Construction



(c) Urban Resident Services



(d) Hospitality

Figure 9: Industry-Specific Marginal Effects of Reform Exposure across Levels of Market Access. *Notes* The vertical axis reports the marginal effect of reform exposure on firm entry intensity, evaluated at different levels of mean-centered market access. The horizontal axis represents centered market access. The vertical lines indicate 95% confidence intervals.

9 Heterogeneity across Regional and Spatial Contexts

Although baseline estimates demonstrate that the commercial system reform generally promotes firm entry intensity across three major industries (The reform has a positive but statistically insignificant effect on the hospitality industry), the magnitude of the reform effect may vary geographically due to regional disparities in resource endowments and market development. Given that each industry features a distinct industrial structure, we perform sub-sample analyses within each industry to explore spatial heterogeneity. To rigorously evaluate whether regional variations are statistically significant, we further conduct Wald tests for the equality of coefficients across sub-samples. To maintain methodological consistency across industries and spatial definitions, we apply a unified event-study framework to examine spatial heterogeneity across three regional dimensions: (i) Eastern, Central, and Western regions; (ii) Coastal vs. Inland regions; and (iii) the Yangtze River Economic Belt (YEB) vs. Other regions (Non-YEB regions). Specifically, we estimate the following model:

$$\begin{aligned} \mathrm{FEI}_{it}^{j} = \alpha_0^{g_m} + \sum_{k\neq -1} \gamma_k^{g_m} (\mathrm{RuralShare}_{i,base} \times \mathbb{I}(t - t^* = k)) \\ + \mathbf{X}_{it}'\boldsymbol{\theta} + \mu_i + \lambda_t + \varepsilon_{it} \\ (g_1 \in \{\text{East, Central, West}\}, g_2 \in \{\text{Coastal, Inland }\}, g_3 \in \{\text{YEB, Others}\}, m \in \{1,2,3\}) \end{aligned} \tag{16}$$

where the index g_m represents the regional sub-sample under the m-th classification scheme, where $g_1 \in$ {East, Central, West}, $g_2 \in$ {Coastal, Inland}, and $g_3 \in$ {YEB, Others}. Equation 16 is estimated separately for each sub-sample within each industry. The coefficient of primary interest, $\gamma_k^{g_m}$, captures the dynamic treatment effect of the reform in region group g_m at event relative time k.

To quantify the overall average heterogeneous treatment effects and formally test the statistical differences across regions within a fully interacted framework, we further estimate the following pooled DID model for each industry j:

$$\mathrm{FEI}_{it}^{j} = \sum_{r\in R_m} \beta_r^{j} \left(\mathrm{RuralShare}_{i,base} \times \mathrm{Post}_t \times \mathrm{Region}_{ir}\right) + \mathbf{X}_{it}'\boldsymbol{\theta} + \mu_i + \lambda_t + \varepsilon_{it} \tag{17}$$

where Region_{ir} is a regional indicator dummy that equals one if city i belongs to regional group r, and zero otherwise. The set R_m represents the corresponding regional classification scheme ($R_1 \in$ {East, Central, West}, $R_2 \in$ {Coastal, Inland}, and $R_3 \in$ {YEB, Others}). The coefficients of interest, β_r^j, capture the industry-specific heterogeneous treatment effects of the reform in region r.

9.1 Eastern, Central, and Western Regions

To explore the regional variation in the impact of the reform, Figure 10 presents the event-study estimates across the Eastern, Central, and Western regions for low-end manufacturing, construction, and resident services, respectively. Across all three industry specifications, the pre-reform coefficients for all three geographical regions fluctuate around zero without distinct pre-trends, confirming the plausible validity of the parallel trends assumption across regional subsamples. Table 8 further quantifies these regional responses. Across these industries, the estimated effects of the continuous reform exposure are positive and statistically significant in the Eastern, Central, and Western regions, indicating that the reform generated broadly positive firm entry intensity effects across geographical areas. Nevertheless, formal tests of coefficient equality indicate that the magnitude of the reform effect varies across regions in some industries. We therefore conduct pairwise tests of coefficient differences between the Eastern and Central, Eastern and Western, and Central and Western regions within each industry.

In low-end manufacturing industry, the coefficient on the interaction between baseline exposure (rural population share) and the post-reform indicator is significantly larger in the Eastern region than in both the Central and Western regions ($p < 0.10$ for both comparisons), while the difference between the Central and Western regions is statistically insignificant ($p > 0.10$). This pattern indicates that the marginal effect of baseline exposure on post-reform firm entry intensity is stronger in the Eastern region than in the Central and Western regions. In construction industry, none of the pairwise differences in the estimated

coefficients across the Eastern, Central, and Western regions is statistically significant ($p > 0.10$ for all three comparisons). The estimated marginal effects of baseline exposure are therefore statistically indistinguishable across regions, reflecting the high spatial mobility of construction labor where project-based workers are mobilized across city boundaries rather than relying on static local labor pools. In urban resident services industry, the regional pattern differs. The estimated coefficient in the Western region is significantly larger than that in the Central region ($p < 0.01$), whereas the differences between the Eastern and Central regions and between the Eastern and Western regions are statistically insignificant ($p > 0.10$). Thus, regional heterogeneity in resident services is concentrated in the Central–Western comparison, where less-industrialized Western cities absorb transferring rural labor predominantly into low-barrier local service industries, significantly lowering operating costs and driving business formation.

A potential concern in interpreting the regional estimates is that baseline exposure may differ systematically across regions, with Eastern cities potentially exhibiting lower exposure (lower baseline rural population share) than their Central and Western counterparts. This distinction is crucial because in the continuous DID framework, the coefficient on the interaction between baseline exposure and the post-reform indicator captures the marginal effect of exposure on post-reform firm entry intensity, rather than the aggregate effect of the reform for a city. Accordingly, cross-regional differences in the estimated coefficients should be interpreted as differences in the responsiveness of firm entry intensity to baseline exposure, rather than as differences in the overall magnitude of the reform shock.

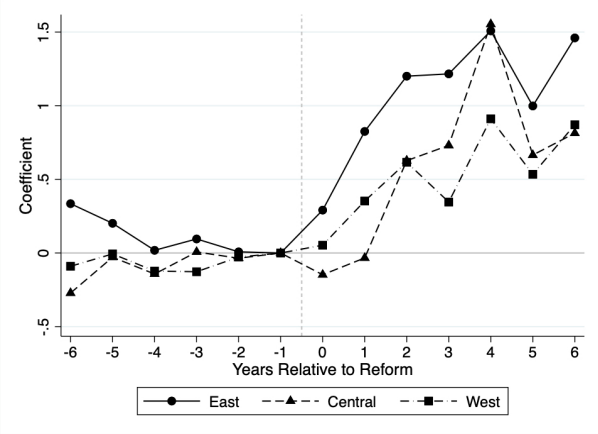
Low-end manufacturing provides a useful illustration. The estimated marginal effect of baseline exposure is 0.186 larger in the Eastern region than in the Central region ($p < 0.10$) and 0.164 larger than in the Western region ($p < 0.10$), while the Central–Western difference is only -0.022 and statistically insignificant ($p > 0.10$). Thus, for a given increase in baseline exposure, the post-reform increase in firm entry intensity in low-end manufacturing is estimated to be 0.186 and 0.164 larger in Eastern cities than in Central and Western cities, respectively. These coefficient differences therefore indicate stronger marginal responsiveness to reform exposure in the Eastern region, rather than a larger aggregate treatment effect. This stronger marginal responsiveness in Eastern low-end manufacturing is highly consistent with complementarities between local factor availability and favorable market conditions. Eastern cities feature denser supplier networks, superior market access, and abundant capital, but face acute labor scarcity and high wage pressures. A higher baseline rural population share in an Eastern city implies a larger pool of potential low-cost labor, easing recruitment frictions and enabling firms to more fully capitalize on institutional deregulation. On the contrary, in the central and western regions, the expansion of low-end manufacturing may be constrained by capital stock or market demand, resulting in a diminishing marginal return on local labor supply. These interpretations are only enlightening and not directly confirmed by the comparison of regional coefficients.

9.2 Coastal and Inland Areas

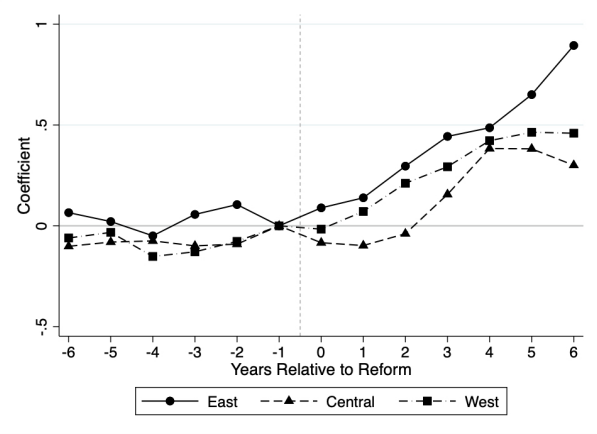
We next examine whether coastal location is associated with differential reform responsiveness by dividing the sample into coastal and inland cities. This spatial distinction captures differences in broader market integration, transport logistics, and international trade access under which firms respond to reductions in entry barriers. Comparing the two groups allows us to assess whether the marginal effect of baseline exposure on post-reform firm entry differs systematically across geographic locations.

The event-study estimates provide broadly similar dynamic patterns across coastal and inland cities (see Figure 11). For low-end manufacturing, construction, and resident services, the estimated coefficients in both groups show no systematic evidence of differential pre-trends prior to the reform, while the post-reform coefficients become positive and increase steadily.

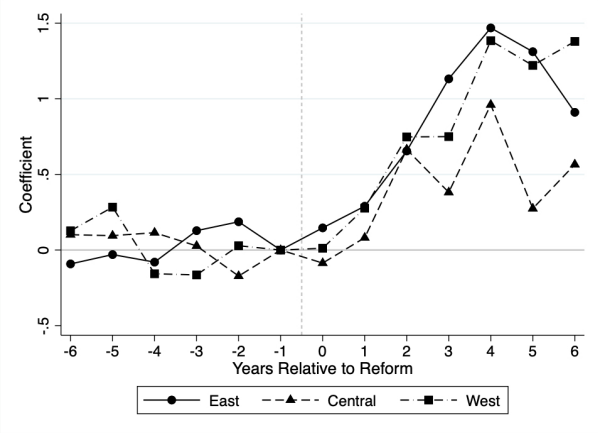
Table 9 presents the corresponding continuous DID estimation results and cross-group coefficient equality tests. For low-end manufacturing, construction, and urban resident services, the estimated treatment effects are positive and statistically significant in both coastal and inland cities. However, coefficient-equality tests reveal statistically significant coastal–inland heterogeneity exclusively in low-end manufacturing. The estimated marginal effects of baseline exposure on low-end manufacturing firm entry intensity are 0.936 in coastal cities and 0.780 in inland cities, with the difference of 0.156 being statistically significant at the 5% level ($p < 0.05$). Thus, for a given increase in baseline reform exposure, the associated post-reform increase in low-end manufacturing firm entry intensity is estimated to be 0.156 larger in coastal than in inland cities. By contrast, the coastal–inland coefficient differences in construction and urban resident services are



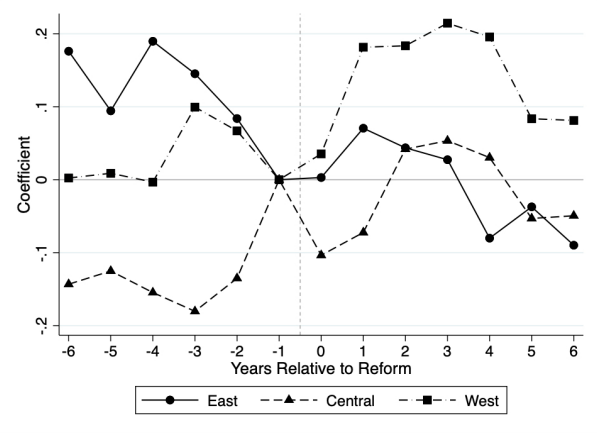
(a) Low-end Manufacturing



(b) Construction



(c) Urban Resident Services



(d) Hospitality

Figure 10: Industry-Specific Dynamic Effects of Reform: East vs. Central vs. West Heterogeneity. Notes: This figure presents the dynamic treatment effects of the commercial system reform across Eastern, Central, and Western regions. Panels (a) through (d) report the event-study results for the four major industries: Low-end Manufacturing, Construction, Urban Resident Services, and Hospitality, respectively. The solid dots depict the point estimates ($\gamma_k^{g_1}$). All regressions control for city fixed effects, year fixed effects, and a comprehensive set of time-varying city characteristics, as specified in Equation 16. The period immediately preceding the reform ($k = -1$) is the baseline reference category.

Table 8: Regional Heterogeneity and Tests of Treatment Effect Differences

	Panel A: Industry 1			**Panel B: Industry 2**			**Panel C: Industry 3**			**Panel D: Industry 4**		
	East	Central	West	East	Central	West	East	Central	West	East	Central	West
Core Interaction	0.974***	0.788***	0. .810***	0.357***	0.307***	0.357***	0.827***	0.734***	0.931***	-0.028	0.013	0.059*
	(0.165)	(0.125)	(0.113)	(0.068)	(0.060)	(0.061)	(0.176)	(0.139)	(0.136)	(0.037)	(0.041)	(0.043)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,248	1,298	994	1,248	1,298	994	1,248	1,298	994	1,248	1,298	994

Notes: Panels (a) through (d) (Industries 1–4) correspond to low-end manufacturing, construction, urban resident services, and hospitality, respectively. All regressions control for city fixed effects, year fixed effects, and a comprehensive set of time-varying city characteristics (per capita GDP, financial level, FDI, education level, and government expenditure ratio-to GDP). Robust standard errors clustered at the city level are in parentheses. $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

Difference in DID Effects (p-values):

Panel A: East = Central (Diff = -0.186; $t = -1.89$; $p = 0.060$), East = West (Diff = -0.164; $t = -1.69$; $p = 0.093$), Central = West (Diff = -0.021; $t = 0.13$; $p = 0.7222$);
Panel B: East = Central (Diff = -0.050; $t = -1.40$; $p = 0.164$), East = West (Diff = 0.0003; $t = 0.01$; $p = 0.994$), Central = West (Diff =-0.050; $t = -1.71$; $p = 0.192$);
Panel C: East = Central (Diff = -0.093; $t = -1.13$; $p = 0.261$), East = West (Diff = 0.104; $t = 1.16$; $p = 0.246$), Central = West (Diff = -0.197; $t = -2.84$; $p = 0.005$).

statistically insignificant ($p > 0.10$ in both industries), indicating that the marginal responsiveness to baseline exposure is statistically similar across coastal and inland cities in these non-tradable and project-based industries.

As emphasized in the regional analysis, this statistically significant coefficient difference in low-end manufacturing reflects the difference in the marginal response of firm entry intensity to benchmark risk exposures, rather than the larger overall reform shock in coastal areas. Although the rural population share in coastal cities is usually low, these cities are deeply integrated into the global supply chain and face urgent export delivery deadlines, which makes the availability of local labor a key constraint. Therefore, a modest increase in the supply of local rural labor in coastal cities can significantly reduce the friction between recruitment and search, enable manufacturing companies to respond more flexibly to administrative approval simplification measures, and rapidly expand their business scale. In contrast, the manufacturing industry in inland areas is limited by higher transportation logistics costs and narrower regional market coverage, and its enterprise entry rate has a lower marginal elasticity relative to the local labor supply.

9.3 Yangtze River Economic Belt

We further examine whether reform effects differ between cities within and outside the Yangtze River Economic Belt. This regional classification reflects the differences in economic development and market integration between cities inside and outside the Yangtze River Economic Belt, and provides a further dimension for testing whether there are regional differences in the marginal response of enterprises to reform risks.

The event-study estimates exhibit broadly similar dynamic patterns across the two groups (see Figure 12). For low-end manufacturing, construction, and urban resident services, neither Yangtze River cities nor non-Yangtze cities show systematic evidence of differential pre-trends prior to the reform, while the estimated post-reform coefficients become positive and increase steadily.

Table 10 presents the corresponding continuous DID estimates and formal cross-group coefficient equality tests. For low-end manufacturing, construction, and urban resident services, the continuous DID estimates are positive and statistically significant in both subsamples. However, in sharp contrast to the coastal–inland comparison, the coefficient-equality tests reveal statistically significant spatial heterogeneity across all three industries at the 1% level ($p < 0.01$). In each industry, the estimated marginal effect of baseline exposure is consistently and significantly larger in non-YEB cities than in YEB cities. Thus, for a given increase in baseline rural population share, the post-reform increase in firm entry intensity is greater in non-YEB cities across all three active industries.

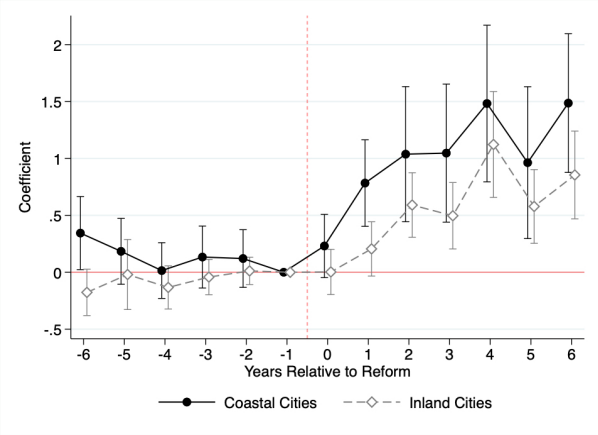
To plausibly interpret this unanimous pattern, we emphasize that the continuous DID estimates reflect differences in the marginal responsiveness of firm entry intensity to local baseline exposure, rather than indicating that the reform was less effective in overall magnitude along the Yangtze River Economic Belt. This lower marginal responsiveness of cities along the Yangtze River stems from two possible structural mechanisms:

First, the cities along the Yangtze River Corridor have always been modern industrial and logistics hubs, with capital-intensive infrastructure and a mature waterway transportation system. In these capital-intensive economic environments, the existing capital stock and superior logistics infrastructure have formed a strong alternative to the original human labor, thus weakening the marginal returns brought by the introduction of more local rural labor.

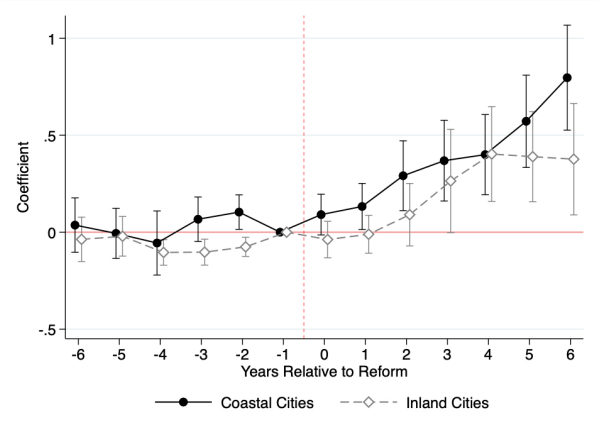
Second, as the magnetic pole of the regional economy, the cities along the Yangtze River have a strong population siphon effect, which can attract migrant workers from the wider hinterland of the province. Therefore, the establishment of enterprises in cities along the Yangtze River does not strictly depend on the proportion of rural population in their cities. In contrast, non-riverside hinterland cities are difficult to attract external labor inflows due to their relatively closed geographical location.

10 Conclusion

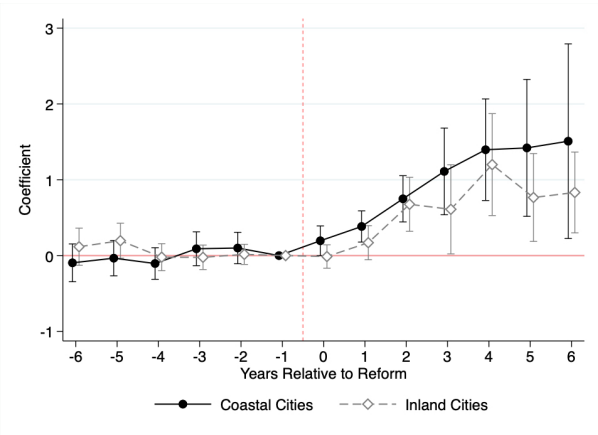
How does institutional deregulation affect the firm entry, and to what extent does its impact depend on firms' local economic environment? We study these questions in the context of China's commercial system reform, which substantially reduced administrative barriers to firm entry and was implemented nationwide



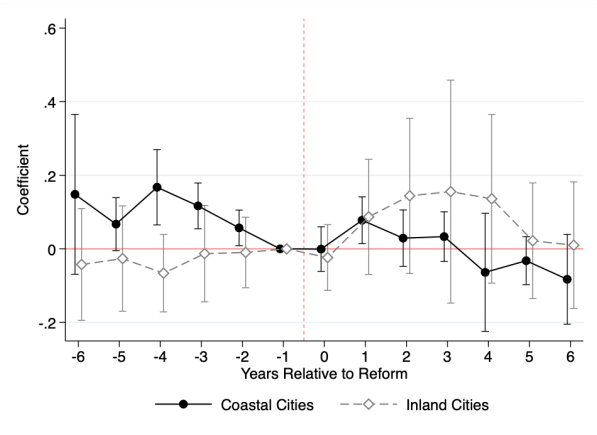
(a) Low-end Manufacturing



(b) Construction



(c) Urban Resident Services



(d) Hospitality

Figure 11: Industry-Specific Dynamic Effects of Reform: Coastal vs. Inland Heterogeneity. Notes: This figure presents the dynamic treatment effects of the commercial system reform across Coastal and Inland regions, estimated based on Equation (16). Panels (a) through (d) report the event-study results for the four major industries. The solid dots and hollow dots depict the point estimates ($\gamma_k^{g_2}$) for Coastal and Inland regions, respectively. The solid (black) vertical lines and dashed (gray) vertical lines denote the corresponding 95% confidence intervals derived from standard errors clustered at the city level. All regressions control for city fixed effects, year fixed effects, and a comprehensive set of time-varying city characteristics, as specified in Equation (16). The period immediately preceding the reform ($k = -1$) is omitted as the baseline reference category.

Table 9: Coastal–Inland Heterogeneity and Tests of Treatment Effect Differences

	Panel A: Sector 1		**Panel B: Sector 2**		**Panel C: Sector 3**		**Panel D: Sector 4**	
	Coastal	Inland	Coastal	Inland	Coastal	Inland	Coastal	Inland
Core Interaction	0.936 ***	0.780 ***	0.344***	0.339***	0.958***	0.845***	-0.0000	0.048
	(0.113)	(0.159)	(0.066)	(0.059)	(0.168)	(0.135)	(0.038)	(0.041)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,407	2,107	1,407	2,107	1,407	2,107	1,407	2,107

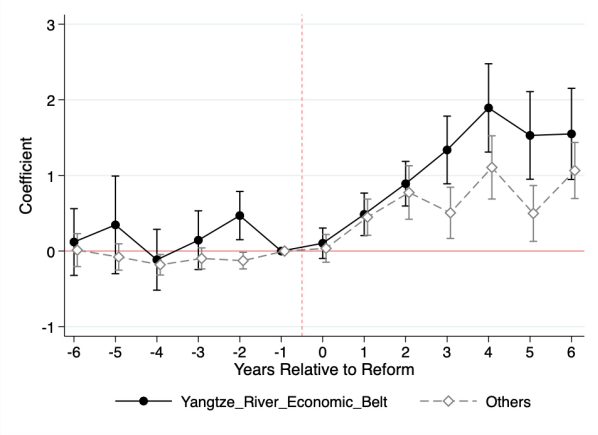
Notes: Robust standard errors clustered at the city level are in parentheses. $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

Difference in DID Effects(p-values):

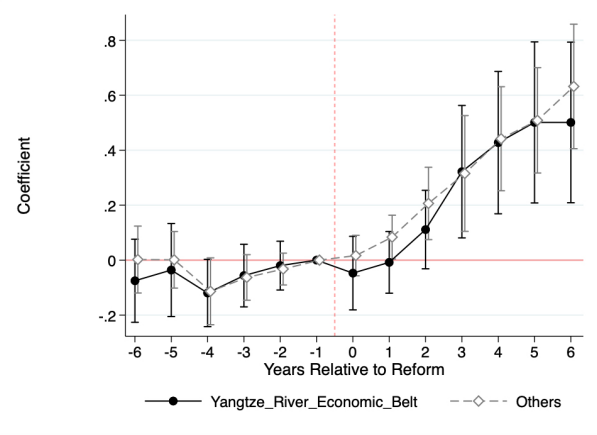
Panel A: Coastal = Inland (Diff = 0.156;$t = 1.99$; $p = 0.048$);

Panel B: Coastal = Inland (Diff = 0.005;$t = 0.15$; $p = 0.883$);

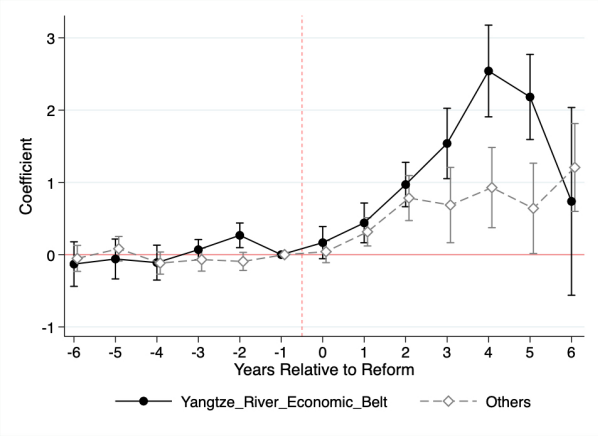
Panel C: Coastal = Inland (Diff = 0.113;$t = 1.56$; $p = 0.121$).



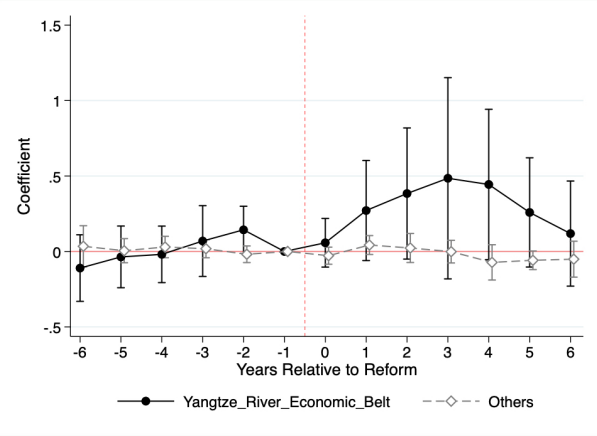
(a) Manufacturing



(b) Construction



(c) Urban Resident Services



(d) Hospitality

Figure 12: Industry-Specific Dynamic Effects of Reform: Yangzte River Economic Belt(YEB) vs. Others(Non-YEB) Heterogeneity. Notes: This figure presents the dynamic treatment effects of the commercial system reform across the Yangtze River Corridor and Other regions, estimated based on Equation (16). Panels (a) through (d) report the event-study results for the four major industries, respectively. The solid dots and hollow dots depict the point estimates ($\gamma_k^{g_3}$) for the Yangtze River Economic Belt and Other regions, respectively. The solid (black) vertical lines and dashed (gray) vertical lines denote the corresponding 95% confidence intervals derived from standard errors clustered at the city level. All regressions control for city fixed effects, year fixed effects, and a comprehensive set of time-varying city characteristics, as specified in Equation (16). The period immediately preceding the reform ($k = -1$) is omitted as the baseline reference category.

Table 10: Yangtze River Economic Belt Heterogeneity and Tests of Treatment Effect Differences

	Panel A: Industry 1		**Panel B: Industry 2**		**Panel C: Industry 3**		**Panel D: Industry 4**	
	YrCities	Other	YrCities	Other	YrCities	Other	YrCities	Other
Core Interaction	0.550 ***	0.877***	0.257***	0.364***	0.678 ***	0.916 ***	0.062	0.034
	(0.109)	(0.114)	(0.061)	(0.057)	(0.137)	(0.139)	(0.050)	(0.037)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,355	2,159	1,355	2,159	1,355	2,159	1,355	2,159

Notes: Robust standard errors clustered at the city level are in parentheses. $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

Wald Tests for Group Differences (p-values):

Panel A: YangzeRiver = Other (Diff= -0.326; $t = -6.39$; $p = 0.000$);

Panel B: YangzeRiver = Other (Diff=-0.107;$t = -3.52$; $p = 0.000$)

Panel C: YangzeRiver = Other (Diff=-0.238; $t = -4.24$; $p = 0.000$).

in 2014. Exploiting cross-city variation in pre-reform exposure to the reform, we use a continuous difference-in-differences design to examine how the marginal effect of reform exposure varies across labor-intensity industries and local conditions.

Our continuous DID estimates show that cities with a higher baseline rural population share experienced larger post-reform increases in firm entry intensity in low-end manufacturing, construction and urban resident services. In contrast, we find no statistically significant effect on firm entry intensity in hospitality. The estimated effects are robust to alternative measures of reform exposure, sample restrictions and alternative specifications. The results therefore suggest that reducing institutional barriers can generate substantial firm entry responses, particularly in labor-intensive industries. The heterogeneity analysis shows that the marginal response to baseline exposure is not geographically uniform. Across alternative geographic classifications, we find systematic differences in the estimated exposure coefficients, although the pattern varies across industries. In particular, the regional comparisons indicate stronger responses in some cases for Eastern cities, while the coastal–inland comparison reveals significant heterogeneity primarily in low-end manufacturing. The comparison between Yangtze River Economic Belt (YEB) cities and non-YEB cities shows a more systematic pattern, with the marginal effect of baseline exposure significantly larger in non-YEB cities in the three affected industries. The mechanism-oriented analysis further suggests that the effectiveness of institutional deregulation depends on pre-existing local economic conditions. The marginal effect of reform exposure varies systematically with market access and rural per capita disposable income, indicating that the response to lower entry barriers is stronger where complementary economic conditions are more favorable. Taken together, these results point to complementarities between institutional deregulation and the local environment in shaping firm entry. More broadly, our findings suggest that the effects of firm entry reform should not be assessed solely on the basis of average effect. The same institutional reform, depending on the economic environment in which the firm is located, may have a very different marginal response in different industries and regions. Therefore, the interaction between institutional barriers and local economic fundamentals may be an important factor in determining whether deregulation can translate into sustained growth in firm entry.

Future research can be further expanded in three directions. First, further examine the construction of firm entry indicators and their economic implications. This paper measures the firm entry intensity by the ratio of the number of newly registered firms to lagged regional GDP. This indicator reflects the registration activities of new firms relative to the existing economic scale, but it is not directly equivalent to the expansion of actual business activities. Second, future research can analyze other industries, and construct reform exposure indicators suitable for their characteristics according to the production technology, factor input structure, capital demand and market dependence of each industry, so as to identify the different effects of different industries under the same institutional reform, and further reveal how relaxation of regulation influence firm entry through different industrial development conditions. Third, it extends from the firm entry reaction to the final economic and social welfare effects of the reform. The growth of firm registration activities is not the ultimate goal of institutional reform. Future research can further examine whether the reform has brought about sustained real economic output growth, job creation, productivity improvement, income improvement and resource allocation efficiency, and analyze how these benefits are distributed among different regions, industries and groups.

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Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this manuscript, the author used ChatGPT (OpenAI) and Gemini (Google) to assist with grammar checking, language polishing, and stylistic refinement of selected passages. The author reviewed and revised the suggested changes as appropriate and takes full responsibility for the content of the published article.

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Appendix A: Mathematical Theoretical Derivations and Proofs

This appendix provides complete step-by-step mathematical proofs, log-linearization procedures, and comparative statics derivations for the theoretical framework presented in Section **??**.

A.1 Micro-Level Firm Optimization and Operating Profit

A representative firm in industry j, region i at time t solves the static profit maximization problem:

$$\max_{l_{ijt}} \quad \pi_{ijt} = P_j A_j l_{ijt}^{\alpha_j} - w_{Rit} l_{ijt} \tag{18}$$

Taking the first-order condition (FOC) with respect to l_{ijt}:

$$\frac{\partial \pi_{ijt}}{\partial l_{ijt}} = \alpha_j P_j A_j l_{ijt}^{\alpha_j - 1} - w_{Rit} = 0 \tag{19}$$

Solving explicitly for the optimal labor demand l_{ijt}^*:

$$l_{ijt}^* = \left(\frac{\alpha_j P_j A_j}{w_{Rit}} \right)^{\frac{1}{1-\alpha_j}} = \theta_j w_{Rit}^{-\frac{1}{1-\alpha_j}} \tag{20}$$

where $\theta_j \equiv (\alpha_j P_j A_j)^{\frac{1}{1-\alpha_j}} > 0$.

Substituting l_{ijt}^* back into the objective function yields maximized operating profit:

$$\begin{aligned} \pi_{ijt}^* &= P_j A_j \left(\frac{\alpha_j P_j A_j}{w_{Rit}} \right)^{\frac{\alpha_j}{1-\alpha_j}} - w_{Rit} \left(\frac{\alpha_j P_j A_j}{w_{Rit}} \right)^{\frac{1}{1-\alpha_j}} \\ &= (1-\alpha_j) \alpha_j^{\frac{\alpha_j}{1-\alpha_j}} (P_j A_j)^{\frac{1}{1-\alpha_j}} w_{Rit}^{-\frac{\alpha_j}{1-\alpha_j}} \\ &\equiv \Omega_j w_{Rit}^{-\kappa_j} \end{aligned} \tag{21}$$

where $\Omega_j \equiv (1-\alpha_j)\alpha_j^{\kappa_j} (P_j A_j)^{\frac{1}{1-\alpha_j}} > 0$ and $\kappa_j \equiv \frac{\alpha_j}{1-\alpha_j} > 0$.

A.2 Regional Labor Market Clearing and Proof of Lemma 1

Aggregate regional labor demand L_{it}^D across all J industries is given by:

$$L_{it}^D = \sum_{j=1}^{J} N_{ijt}^* l_{ijt}^* = \sum_{j=1}^{J} N_{ijt}^* \theta_j w_{Rit}^{-\frac{1}{1-\alpha_j}} \tag{22}$$

To evaluate the summation without loss of generality, we perform a log-linearization around the average regional labor output elasticity $\bar{\alpha} \in (0,1)$. Expanding $\frac{1}{1-\alpha_j}$ around $\bar{\alpha}$ via a first-order Taylor series yields:

$$\frac{1}{1-\alpha_j} \approx \frac{1}{1-\bar{\alpha}} + \frac{1}{(1-\bar{\alpha})^2}(\alpha_j - \bar{\alpha}) \tag{23}$$

Thus, the wage term can be approximated as:

$$w_{Rit}^{-\frac{1}{1-\alpha_j}} \approx w_{Rit}^{-\frac{1}{1-\bar{\alpha}}} \left[1 - \frac{\ln w_{Rit}}{(1-\bar{\alpha})^2}(\alpha_j - \bar{\alpha}) \right] \tag{24}$$

Substituting this back into aggregate labor demand:

$$L_{it}^{D} \approx w_{Rit}^{-\frac{1}{1-\bar{\alpha}}} \sum_{j=1}^{J} \theta_k N_{ijt}^{*} - w_{Rit}^{-\frac{1}{1-\bar{\alpha}}} \frac{\ln w_{Rit}}{(1-\bar{\alpha})^2} \sum_{j=1}^{J} \theta_j N_{ijt}^{*} (\alpha_j - \bar{\alpha}) \tag{25}$$

By definition of the weighted average $\bar{\alpha} \equiv \frac{\sum_{j=1}^{J} \theta_j N_{ijt}^{*} \alpha_j}{\sum_{j=1}^{J} \theta_j N_{ijt}^{*}}$, the first-order deviation term sums identically to zero: $\sum_{j=1}^{J} \theta_j N_{ijt}^{*} (\alpha_j - \bar{\alpha}) \equiv 0$. Therefore:

$$L_{it}^{D} \approx w_{Rit}^{-\frac{1}{1-\bar{\alpha}}} \cdot \sum_{j=1}^{J} \theta_j N_{ijt}^{*} \tag{26}$$

Equating labor supply L_{it}^{S} (Assumption 3) with labor demand L_{it}^{D}:

$$\left(\frac{w_{Rit}}{w_A}\right)^{\frac{1}{\gamma}} \cdot \exp\left(\frac{\lambda}{\gamma} D_t E_i\right) = w_{Rit}^{-\frac{1}{1-\bar{\alpha}}} \cdot \sum_{k=1}^{J} \theta_k N_{ikt}^{*} \tag{27}$$

Rearranging terms to isolate w_{Rit}:

$$w_{Rit}^{\frac{1}{\gamma}+\frac{1}{1-\bar{\alpha}}} = w_A^{\frac{1}{\gamma}} \cdot \left(\sum_{k=1}^{J} \theta_k N_{ikt}^{*}\right) \cdot \exp\left(-\frac{\lambda}{\gamma} D_t E_i\right) \tag{28}$$

Defining $\Gamma \equiv \left(\frac{1}{\gamma} + \frac{1}{1-\bar{\alpha}}\right)^{-1} > 0$, $\bar{w} \equiv w_A^{\frac{\Gamma}{\gamma}} > 0$, and $\lambda^{*} \equiv \frac{\lambda \Gamma}{\gamma} > 0$, and taking the Γ-th power on both sides yields the closed-form equilibrium wage:

$$w_{Rit}^{*} = \bar{w} \cdot \left(\sum_{k=1}^{J} \theta_k N_{ikt}^{*}\right)^{\Gamma} \cdot \exp\left(-\lambda^{*} D_t E_i\right) \tag{29}$$

This completes the proof of Lemma 1. ■

A.3 General Equilibrium Entry Intensity Derivation

Imposing the dynamic zero-profit free-entry condition $\frac{\pi_{ijt}^{*}}{r} = \tau(D_t)$ using equilibrium profits $\pi_{ijt}^{*} = \Omega_j (w_{Rit}^{*})^{-\kappa_j}$:

$$\frac{1}{r} \Omega_j \bar{w}^{-\kappa_j} \left(\sum_{k=1}^{J} \theta_k N_{ikt}^{*}\right)^{-\Gamma \kappa_j} \exp\left(\kappa_j \lambda^{*} D_t E_i\right) = \tau(D_t) \tag{30}$$

Solving for total aggregate equilibrium entry N_{ijt}^{*}:

$$N_{ijt}^{*} = \tilde{C}_j \cdot [\tau(D_t)]^{-\frac{1}{\Gamma \kappa_j}} \cdot \exp\left(\frac{\lambda^{*}}{\Gamma} D_t E_i\right) \tag{31}$$

where $\tilde{C}_j \equiv \left(\frac{\Omega_j \bar{w}^{-\kappa_j}}{r}\right)^{\frac{1}{\Gamma \kappa_j}} > 0$. Dividng both sides by regional economic scale $GDP_{it} > 0$ yields the normalized enterprise entry intensity $y_{ijt}^{*} \equiv \frac{N_{ijt}^{*}}{GDP_{it}}$:

$$y_{ijt}^{*} = \frac{\tilde{C}_j}{GDP_{it}} \cdot [\tau(D_t)]^{-\frac{1}{\Gamma \kappa_j}} \cdot \exp\left(\frac{\lambda^{*}}{\Gamma} D_t E_i\right) \tag{32}$$

A.4 Proofs of Propositions 1 and 2 for Entry Intensity

Proof 1 (Proof of Proposition 1) *Differentiating y^*_{ijt} with respect to reform status D_t:*

$$\begin{aligned}\frac{\partial y^*_{ijt}}{\partial D_t} &= \frac{\tilde{C}_j}{GDP_{it}}\left[-\frac{1}{\Gamma\kappa_j}\tau(D_t)^{-\frac{1}{\Gamma\kappa_j}-1}(-\tau_1)\right]\exp\left(\frac{\lambda^*}{\Gamma}D_tE_i\right)+\frac{\tilde{C}_j}{GDP_{it}}\tau(D_t)^{-\frac{1}{\Gamma\kappa_j}}\exp\left(\frac{\lambda^*}{\Gamma}D_tE_i\right)\left(\frac{\lambda^*}{\Gamma}E_i\right)\\ &= y^*_{ijt}\left[\frac{\tau_1}{\Gamma\kappa_j\tau(D_t)}+\left(\frac{\lambda^*}{\Gamma}\right)E_i\right] \qquad (33)\end{aligned}$$

Since $y^*_{ijt}, \tau_1, \tau(D_t), \Gamma, \kappa_j, \lambda^*, E_i > 0$, $\frac{\partial y^*_{ijt}}{\partial D_t} > 0$ strictly holds.

Proof 2 (Proof of Proposition 2) *Differentiating $\frac{\partial y^*_{ijt}}{\partial D_t}$ with respect to regional exposure E_i:*

$$\begin{aligned}\frac{\partial^2 y^*_{ijt}}{\partial D_t\partial E_i} &= \frac{\partial y^*_{ijt}}{\partial E_i}\left[\frac{\tau_1}{\Gamma\kappa_j\tau(D_t)}+\left(\frac{\lambda^*}{\Gamma}\right)E_i\right]+y^*_{ijt}\cdot\frac{\partial}{\partial E_i}\left[\frac{\tau_1}{\Gamma\kappa_j\tau(D_t)}+\left(\frac{\lambda^*}{\Gamma}\right)E_i\right]\\ &= \left(y^*_{ijt}\cdot\frac{\lambda^*}{\Gamma}D_t\right)\left[\frac{\tau_1}{\Gamma\kappa_j\tau(D_t)}+\left(\frac{\lambda^*}{\Gamma}\right)E_i\right]+y^*_{ijt}\cdot\left(\frac{\lambda^*}{\Gamma}\right) \qquad (34)\end{aligned}$$

Evaluating at post-reform status $D_t = 1$:

$$\left.\frac{\partial^2 y^*_{ijt}}{\partial D_t\partial E_i}\right|_{D_t=1} = y^*_{ijt}\left(\frac{\lambda^*}{\Gamma}\right)\left[\frac{\tau_1}{\Gamma\kappa_j\tau(1)}+\left(\frac{\lambda^*}{\Gamma}\right)E_i+1\right] > 0 \qquad (35)$$

Since $GDP_{it} > 0$, entry intensity $y^*_{ijt} > 0$, and all structural parameters are positive, the cross-partial derivative of entry intensity is strictly positive.

Appendix B

B.1 The Descriptive Statistics of The Main Variables

Table 11: Descriptive Statistics

Variables	N	Mean	SD	Min	Max
$\text{RuralShare}_{\text{base}}$	3,640	0.5229475	0.1481853	0.05825	0.8281834
Low-end Manufacturing	3,640	0.6134513	0.4907143	0.0334087	5.256682
Construction	3,640	0.2853469	0.2466917	0.0136732	2.204464
Urban Resident Services	3,640	0.7765072	0.6020352	0.0752839	6.931434
Hospitality	3,640	0.1417637	0.1515348	0.0011763	3.084485
pgdp	3,640	10.53727	0.6363161	8.189245	12.45635
exp_gdp	3,640	0.1958688	0.1053479	0.0437108	1.026784
fdi	3,612	0.017597	0.0188142	0.0000535	0.1294008
fin	3,640	2.297426	1.119757	0.5878793	21.30146
edu	3,554	9.064047	0.7600165	6.55	11.54

Notes: This table reports the descriptive statistics of the main variables used in the empirical analysis.

B.2 The Average FEI Across the Four Industries

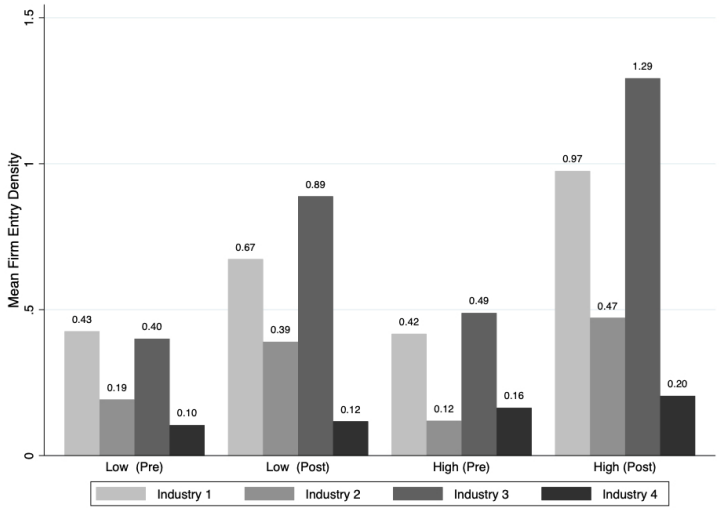


Figure 13: Industry-Specific Average Firm Entry Intensity by Baseline Exposure: Low VS. High Exposure Cities. *Notes:* This figure presents average firm entry intensity (FEI) before and after the commercial system reform across cities with different levels of baseline exposure. To provide a clearer visualization of the exposure gradient, cities are divided into terciles according to their baseline exposure. We focus on the bottom and top terciles, which capture cities with relatively low and high exposure, respectively, while excluding the middle tercile from the comparison. Pre-reform averages are calculated over 2008–2013, while post-reform averages are calculated over 2014–2020. FEI is defined as the number of newly registered firms normalized by lagged local GDP. Different colors indicate the four labor-intensive industries examined in the analysis.

B.3 The Aggregate Firm Entry Intensity by Province: Low-end Manufacturing



Figure 14: Aggregate Firm Entry Intensity in the Low-end Manufacturing by Province over Time. *Notes:* This figure presents annual aggregate firm entry intensity (FEI) at the provincial level. The horizontal axis represents years, and the vertical axis reports the aggregate FEI, defined as the sum of city-level FEI values across all prefecture-level cities within each province-year observation. The figure covers 25 provincial-level units during the sample period of 2008–2020.

Appendix C

C.1 Construction of the Transformation Exposure Measure for Robustness Check III

1. Winsor.RuralShare is defined as Winsorize(RuralShare, $1\%, 99\%$);
2. Rank.RuralShare is defined as $\frac{\text{Rank}(\text{RuralShare}_{i,\text{base}}) - \min(\text{Rank}(\text{RuralShare}_{i,\text{base}}))}{\max(\text{Rank}(\text{RuralShare}_{i,\text{base}})) - \min(\text{Rank}(\text{RuralShare}_{i,\text{base}}))}$;
3. Normalized.RuralShare is defined as $\frac{\text{RuralShare}_{i,\text{base}} - \overline{\text{RuralShare}_{i,\text{base}}}}{\sigma(\text{RuralShare}_{i,\text{base}})}$;
4. ln.RuralShare is defined as $\ln(\text{RuralShare}_{i,\text{base}} + 1)$.

C.2 Robustness III: Rank the exposure

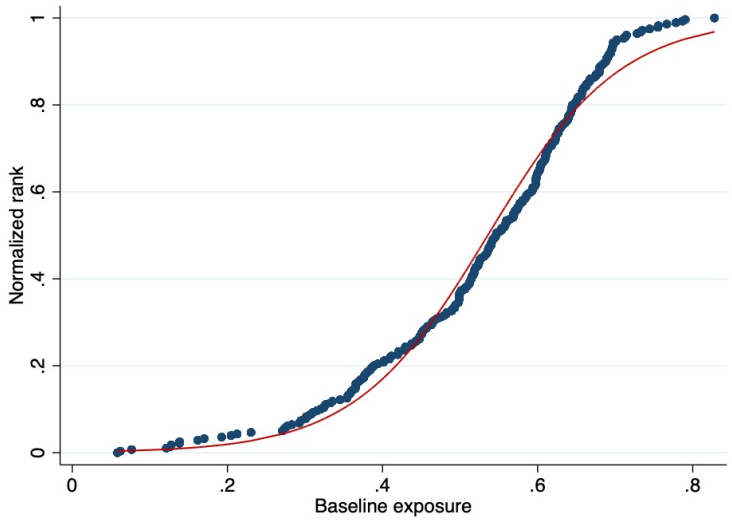


Figure 15: Mapping between Baseline Exposure and Its Normalized Rank Transformation. *Notes:* This figure illustrates the transformation from the baseline exposure measure to its normalized rank counterpart. The horizontal axis groups the original exposure measure into intervals of 0.2 units, and the vertical axis reports the corresponding normalized rank values.

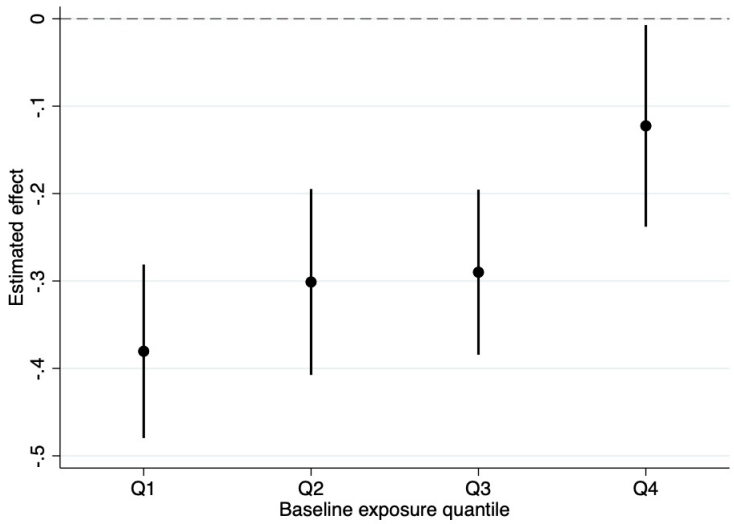


Figure 16: Reform Effects Relative to the Highest Baseline Exposure Quantile (Q5). *Notes:* The baseline exposure measure is ranked and divided into five quantiles, with Q5 representing the highest exposure group and serving as the reference category. Each coefficient captures the difference-in-differences estimate for a given exposure quantile relative to Q5, measuring the differential change in firm entry intensity after the reform. Vertical bars represent 95% confidence intervals.