The Centralization Paradox: How Authoritarian Reforms Reproduce Patronage through Spatial Capture [*†]

Siyuan Fan [‡] Wenyi Lu [§] Shangkun Xie [¶]

Abstract

Despite formidable enforcement capacity, authoritarian regimes frequently find their centralization reforms yield mixed results. This article theorizes a *centralization paradox*: reforms intended to curb corruption and strengthen control instead shift power upward and reproduce patronage at higher administrative levels. We advance a novel mechanism of *spatial capture*, in which politically reliant firms adapt to the loss of local protection by reallocating investment to rebuild patronage ties under newly centralized authority. We locate our theory in China's 2014 judicial reform, which centralized court authority at the provincial level. Combining a difference-in-differences specification with a shift–share design, we show that politically reliant firms withdrew from reformed jurisdictions and reinvested in provincial capitals, where concentrated authority revitalized patronage. The findings reveal an endogenous limit on centralization reforms: with weak institutions, reforms meant to purge local collusion instead relocate it, reproducing patronage in new political geographies.

Keywords: Patronage networks; Centralization reform; State-business relationship.

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[‡]Department of Political Science, UC San Diego, USA; email: sif004@ucsd.edu.

[§]Wilf Family Department of Politics, New York University, USA; email: wenyilu@nyu.edu.

[¶]School of Applied Economics, Renmin University of China, China; email: shangkunecon@gmail.com.

1 Introduction

Departing from the conventional image of extractive autocracy, modern autocrats increasingly pursue top-down institutional reforms to address governance challenges (Malesky, Nguyen and Tran, 2014; Pepinsky, 2014). Centralization reforms, which shift power authority upward within the political hierarchy, have emerged as a distinctive art of autocratic rule (Treisman, 2007; Pepinsky, 2014; Carothers, 2022). However, despite these ambitions to consolidate political control, curb local capture, and promote economic growth, such reforms often produce mixed outcomes. In Russia, politically connected firms thrived under the 2000s judicial modernization as oligarchic networks evaded enforcement (Gel'Man, 2015). Likewise, China's post-2012 anti-corruption campaign curbed collusion between officials and private entrepreneurs but redirected political favor toward state-owned enterprises (Ang, 2020; Fang and Zhang, 2025).

This pattern raises a central puzzle: why do centralization reforms designed to purge local collusion paradoxically reproduce patronage, despite strong implementation capacity for these autocrats? While existing research largely portrays authoritarian reform as a *top-down*, state-driven process,[1] we argue that its outcomes also hinge on *bottom-up* strategic adaptation by society. Firms embedded in patronage networks adjust to institutional change by repositioning influence toward the newly empowered authority, which sustains resilient patronage networks and undermines reform effectiveness.

We conceptualize reform outcomes not as one-sided consequences of institutional design but as co-evolutionary equilibria arising from strategic interactions between autocrats and firms (Grossman and Lewis, 2014). We theorize a mechanism of *spatial capture*: while top–down control may effectively alleviate collusion at lower levels, concentrated authority at higher levels creates new space for rent-seeking. As the potential targets and casualties of centralization reforms, politically reliant firms then adapt to the shifting authority by reallocating investment toward the newly empowered elites, rebuilding patronage networks in higher concentrated authority. As a result, centralization produces a dual failure: it can neither boost market prosperity in the short run due

1. For instance, Finkel and Gehlbach (2020) attributes reform failures to weak state capacity, where reliance on local implementation leads to relative deprivation.

to disinvestment by politically reliant firms, nor promote market institutionalization in the long run, as firms sustain patronage through the spatial reallocation of influence (Bisbee and You, 2024).

To support this argument, we turn to China's judicial reform, one of the prominent examples of authoritarian centralization in the world's largest authoritarian state (Zhang and Ginsburg, 2019). In 2014, Communist Party of China (CPC) initiated a sweeping judicial centralization reform as part of Xi's broader campaign to consolidate power and re-centralize authoritarian control. By shifting the authority over court personnel, assets, and budgets from local governments to provincial leaders, the reform sought to purge the long-standing patron–client ties and entrenched collusion between local officials and the judiciary. However, despite the CPC's Leninist capacity for top–down enforcement, we challenge the optimistic view that centralization reforms invariably curb government-business collusion (Lei and Li, 2025). We argue that politically reliant firms, deprived of their political leverage over judicial outcomes by the centralization reform, adapt through spatial capture by strategically reallocating investment to rebuild patronage, thereby undermining the reform's effort to purge collusion and build a more institutionalized market.

Empirically, we construct a panel dataset at the firm-(investment location)-year level for the period 2012–2019 to examine how firms respond to judicial reforms through investment decisions. We measure firms' political reliance index (PRI) using interaction transcripts between corporate leaders and institutional investors, complemented by data on revolving-door officials, party building, and government procurement. We combine two parallel strategies for identification. First, we employ a difference-in-differences (DiD) approach to estimate the reform's effect on firms' investment in local prefectures across different PRIs.[2] We find that a 10 percentage point (p.p.) increase in the PRI reduces post-reform investment by 8.5% of the mean investment level. Second, to capture firms' strategic investment relocation toward provincial capitals, we use a Bartik (shift-share) design to measure firm's exposure to the reform in local prefectures (*Reform exposure*). The results show that politically reliant firms, as potential victims of the centralization reform, increase their investments in provincial capitals during the reform period: a 10 p.p. increase in PRI raises the likelihood of investing in provincial capitals by 0.7 p.p. Taken together, these results provide evi-

2. We use "local prefectures" to represent all cities except for provincial capitals.

dence for *spatial capture*, a bottom-up strategic adaptation in which politically reliant firms reallocate investments to reconstitute patronage ties. These withdrawals mark not the end of patronage but its relocation toward provincial capitals to regain political favor.

Consistent with this mechanism, we find weaker spatial-capture incentives when local leaders retain provincial standing connections and among firms less dependent on local ties—such as centrally owned SOEs or those with limited pre-reform litigation exposure. In contrast, investment shifts among politically reliant firms are more pronounced in local prefectures with better post-reform market institutions or stronger pre-reform patronage dependence. Politically reliant firms are also more likely to redirect their investments toward provincial capitals characterized by higher corruption, favoritism, and stronger political orientation. Additionally, firms were more willing to pay higher remuneration to revolving-door officials who had previously served in provincial-level positions. This implies that these firms engaged in spatial capture, reallocating investment to reconstitute patronage networks.

Our framework speaks to ongoing debates on the effectiveness of centralization reforms. Classic accounts emphasize that centralization can strengthen governance capacity and curb corruption (Slater, 2010; Malesky et al., 2014; Boffa et al., 2016; Carothers, 2022). Yet recent research shows that top–down initiatives are often undermined by local capture (Olken, 2007; Reuter and Robertson, 2012; Ang, 2020; Trinh and Lu, 2023). In China, for instance, anti-corruption efforts have been constrained by factional ties (Lorentzen and Lu, 2018), elite conflicts (Zhu and Zhang, 2017), and leadership motives (Xi, Yao and Zhang, 2025).[3] We make two contributions to this literature. First, we move beyond the state-centered perspective to highlight societal adaptation, identifying a mechanism of *spatial capture*, whereby politically reliant firms reallocate investments across space to rebuild patronage ties. Second, unlike accounts that focus on how local capture impedes reform in weak state (Finkel and Gehlbach, 2020; Bardhan and Mookherjee, 2000; Olken, 2007), we show that even high-capacity authoritarian reforms that successfully purge local patronage can provoke an upward, strategic form of capture, producing new institutional failures.[4]

3. Studies on judicial reform also reach mixed conclusions: some show improved fairness (Lei and Li, 2025), reduced patronage (Cai, Chen, Chen and Zhang, 2025), increased investment (Liu, Lu, Peng and Wang, 2022), and enhanced accountability (Cao, Liu and Zhou, 2023), while others find negligible effects (Zhou, Liu, He and Cheng, 2021).

4. Wang (2018) show that firms differ in their capacity to capture different levels of government. Our findings reveal

Specifically, our findings shed light on judicial reform under autocracy by shifting attention from autocrats' motives (Pereira, 2005; Hilbink, 2007; Ginsburg and Moustafa, 2008; Wang, 2015) to the endogenous limits of legal institutionalization. Under weak institutions, judicial reforms are prone to spatial capture by resilient patronage networks. This perspective challenges the view that enhanced rule of law necessarily foster economic prosperity (Rajah, 2012; Acemoglu and Robinson, 2013; Chemin, 2020), but join recent research that emphasizes private actors' strategic responses to institutional change (Gans-Morse, 2017; Beazer and Blake, 2018; Hou, 2019). Notably, we offer the first evidence that judicial institutionalization could trigger patronage centralization.

Our study highlights the adaptive resilience of patronage networks under weak institutions. Existing research documented patronage in bureaucratic hierarchies, both its efficiency costs (Do, Nguyen and Tran, 2017; Xu, 2018; Colonnelli, Prem and Teso, 2020) and benefits (Jiang, 2018; Toral, 2024; **?**). We move this debate beyond the public sector, showing that patronage is not merely a bureaucratic pathology but a co-evolutionary equilibrium that market actors can reconstruct strategically when formal reforms threaten their survival. Focusing on state-business collusion in China, prior studies focus on either its social costs (Fisman and Wang, 2015; Jia and Nie, 2017) or firms' responses to political turnover (Jiang and Mei, 2020; Shi, Xi, Zhang and Zhang, 2021; Nian and Wang, 2023). This study turns to firms' strategic adaptations to reforms, which contribute to the persistence of patronage and clientelism, and meanwhile hinder reform effectiveness.

More broadly, we contribute to the scholarship on state-society relations in authoritarian states. While classic works have explored how authoritarian governments manage civil society for their own survival (Teets, 2014; Mattingly, 2019; Zeng et al., 2023), recent work turns to societal adaptation, highlighting the agency of firms in adapting to state policies while pursuing their own agendas (Hou, 2019; Lin, 2025; Li and Lu, 2025; Lu, Fan and Chen, 2025). Building on this perspective, we conceptualize reform outcomes as co-evolutionary equilibria between the state and firms: rather than eliminating local patronage, centralization reconfigures and reproduces capture in new forms that often diverge from reformers' intent (Markus, 2012; Gans-Morse, 2017), thereby revealing how the concentration of political power reshapes market dynamics.

the dynamics of capture: when local capture is curtailed as institutions change, new capture can emerge at higher levels, which also aligns with Wang (2016) showing the prevalence and effectiveness of high-level capture in China.

2 Theory

2.1 Reform with Control: The Centralization Paradox in Autocracies

Conventional wisdom depicts autocrats as predatory rulers fixated on rent extraction and repression (Olson, 1993; Wintrobe, 2000; Acemoglu and Robinson, 2013). Yet durable autocracies often exhibit striking adaptive capacity, undertaking institutional reforms to bolster state capacity, revive economic performance, and secure regime survival (Nathan, 2003; Svolik, 2012; Pepinsky, 2014; Carothers, 2022). Such reform, however, confronts a Tocquevillian dilemma: rulers must adjust institutions to sustain legitimacy but cannot cede political control, lest reform ignite pressures for democratization. To reconcile adaptation with control, many resort to *centralizing reforms* that reassert authority while redefining governance. These efforts span regimes and decades: from Chiang Kai-shek's *Reconstruction* in 1950s Taiwan, Mao's *Three and Five Antis* campaign in China, and Park Chung-hee's *General Administrative Reform* in 1970s Korea, to Xi Jinping's consolidation under China's anti-corruption campaign, all grounded in power concentration and top-down discipline to restrict collusion from below.

While such centralization reforms help maintain authoritarian rule, they also contain an inherent tension-the *centralization paradox*: the success of centralization hinges on how agents wield newly concentrated authority. Some firms embrace reduced local collusion and improved market conditions brought by the reform, while other firms that have long thrived on patronage would become the core resistance to reform. The re-concentration of political authority, rather than eliminating their influence, creates new strategic space for them to adapt. With weak institutions, discretionary power tends to be captured by informal coalitions and embedded within patronage networks (Geddes, 1994; Grzymala-Busse, 2008; Ang, 2020), and the concentrated power can turn to new haven for patronage. As authority shifts upward, old local collusion may be curtailed, but new opportunities for rent extraction emerge at higher tiers of the hierarchy. Hence, reforms designed to discipline lower-level corruption often reproduce it in more centralized forms.

Comparative evidence illustrates this dynamic. In Russia, gubernatorial appointment reforms

alleviated local defection but entrenched patron–client dependence between regional executives and the Kremlin (Reuter and Robertson, 2012). In Indonesia, centralized anti-corruption bureaus curtailed petty bribery but generated selective enforcement (Olken, 2007). China's anti-corruption campaign since 2012 also exemplifies similar dynamics (Ang, 2020). Together, centralization reforms that curbed local corruption often reproduced it at higher tiers, highlighting the persistent dilemma facing authoritarian regimes.

2.2 Following Power: Strategic Adaptation and the Mechanism of Spatial Capture

We argue that centralizing reforms in authoritarian regimes trigger bottom-up strategic adaptation that can offset their intended effects. Rather than treating reform outcomes as one-sided consequences of state design, we conceptualize them as co-evolutionary equilibria emerging from the strategic interactions between state (autocrats) and society (firms). As reforms reshape the distribution of authority, societal actors respond by reconfiguring their modes of capture, generating new institutional equilibria that diverge from reformers' intent.

Heterogeneous Preferences and Selective Exit. Conventional wisdom assumes that firms universally prefer cleaner markets and impartial institutions, which reduce policy uncertainty and enforcement risk (Henisz, 2000; Jensen, 2008). Accordingly, top–down centralization reforms that aim to weaken local government–business collusion are expected to enhance economic performance. Yet this modernization view overlooks the heterogeneity of firms' political embeddedness. In weak-institutional settings, where the enforcement of formal rules is inconsistent, many firms rely on informal mechanisms, such as personal ties, lineage networks, and patronage relations, to secure protection and favorable treatment (Helmke and Levitsky, 2004; Ang and Jia, 2014). Politically reliant firms benefit from selective local favoritism and cultivate local patrons through strategic investment: by boosting local GDP, they enhance the career prospects of officials who, in return, provide legal and regulatory leniency (Bai et al., 2020; Jiang and Mei, 2020; Shi et al., 2021).

These politically reliant firms, prevalent in capital-intensive sectors like real estate, energy, and infrastructure, derive advantages from privileged access to credit, regulation, and legal protec-

tion(Fisman, 2001; Khwaja and Mian, 2005; Faccio, 2006; Szakonyi, 2018). When reforms threaten these informal channels, they face a loss of protection rather than an improvement in market conditions. Consequently, they opt for exit the reformed regions and relocate elsewhere.

Hypothesis 1 (The Reallocation Strategy) *After centralization, politically reliant firms will exit reformed jurisdictions and relocate their investments to regions where informal institutions remain intact.*

Vertical Displacement and Spatial Capture. Centralizing reforms also create new patronage opportunities at higher levels of the bureaucracy. Under weak rule of law, the upward transfer of authority produces not impartiality but *spatial capture*: patronage networks reconstitute around the newly empowered tier. Expecting this, politically embedded firms respond by relocating to these new centers of power, where they can continue to extract rents. In this way, centralization shifts, rather than eliminates, the locus of state-business collusion.

In China's judicial centralization reform, the personnel and fiscal resources of local courts are concentrated to the provincial level. This helps reduce political interference in local prefectures but not in provincial capitals. Politically reliant firms, particularly those who invest in reformed regions, will respond by investment reallocation: by reallocating investments to provincial capitals and developing connections with higher-level officials, these firms can evade the reform's impact and continue to benefit from political patronage.

Hypothesis 2 (Centralized Patronage Networks) *In response to centralization, politically reliant firms withdraw investments from local prefectures where old patronage networks are disrupted, and relocate their investments to provincial capitals where new patronage networks can still be established.*

We conceptualize this adaptive dynamic as a *spatial capture*: as authority centralizes, informal patronage migrates upward, reaching a new equilibrium between state control and societal adaptation. As politically reliant firms often command a substantial share of capital, the centralization reform fails to spur economic growth in the short run. Moreover, the spatial investment reallocation reconfigures political patronage, nullifying its long-run goal to promote institutionalization.

2.3 Scope Conditions

The *centralization paradox* emerges under certain institutional conditions. First, the regime must possess sufficient state capacity to weaken local authority. Our argument departs from Finkel and Gehlbach (2020), who show that reforms by weak states often fail because rulers depend on local elites who can sabotage enforcement. By contrast, in strong states with weak institutions, centralization can effectively curb local collusion but create space for spatial capture: once local networks are dismantled, actors rebuild connections upward. Our theory therefore applies to high-capacity authoritarian and developmental states with weak rule of law, such as China, Vietnam, and Russia, where central control constrains local agents but cannot prevent the reallocation of patronage at higher levels. Second, affected actors must rely on local patronage rather than direct central access. When firms or bureaucratic elites are already integrated into national hierarchies, as in Russia's resource conglomerates, Vietnam's central state-owned enterprises, or Malaysia's government-linked companies, local collusion purges rarely threaten their patronage networks and thus induce little adjustment. In contrast, private firms rooted in local patronage, like China's real-estate developers or Indonesia's provincial contractors, face greater exposure to institutional change. Once local protection erodes, they relocate toward higher political centers to restore patronage.

While cross-national tests of these scope conditions lie beyond the empirical reach of this paper, we leverage within-China variation to probe their implications:

Hypothesis 3 (Scope Conditions) *Centralization reforms are least likely to induce spatial capture when (i) existing political patronage remains intact; and (ii) actors possess resources or ties that insulate them from local restructuring.*

Although developed in authoritarian contexts, the framework also applies to emerging democracies that combine strong state capacity with weak rule of law. Centralizing reforms in Brazil, Indonesia, and Mexico show how efforts to discipline local governance can weaken patronage yet reproduce capture at higher administrative tiers. In these settings, political and economic actors adapt by rebuilding access within restructured hierarchies.

3 Background

3.1 China's Judicial System

China's judicial system, as stipulated by the Organic Law of the People's Courts (enacted in 1980), operates as a four-tier hierarchy. At the top is the Supreme People's Court (SPC), followed by province-level High People's Courts (32 in total), city-level Intermediate People's Courts (404), and county-level Basic People's Courts (3,111).[5]

Before reform, local courts answer to the territorial party-state (CPC and government) rather than a higher level court (Mertha, 2009; Wang, 2018) as the party-state holds personnel and budget power. Court budgets, including salaries, bonuses, and operating expenses, are approved by local governments. Personnel decisions, such as promotions and appointments, are ratified by local People's Congresses and overseen by the Organization Department of the local CPC. Consequently, the judiciary is highly vulnerable to political interference: judges are often pressured to align rulings with the preferences of local leaders (Gong, 2004; Peerenboom, 2009; Ng and He, 2017). Such favoritism undermines the credibility of adjudication but benefits firms with political ties. Though China's central leadership repeatedly stipulate that local judiciaries adjudicate cases independently, interference is prevalent and has reinforced China's model of state capitalism.

3.2 "State Capitalism" in China

State-business collusion in China is driven by two intertwined objectives: bureaucratic promotion and rent extraction. Firms contribute to local economic growth by generating employment and output, thereby enhancing prospects for advancement within the bureaucracy (Li and Zhou, 2005). At the same time, close ties with firms allow officials to extract substantial rents through bribery and other unofficial channels (Deng and Wei, 2025). This symbiotic relationship reflects a model of "state capitalism," characterized by pervasive state intervention in markets (Tsai, 2011; Baccini et al., 2019). Such intervention provides politically reliant firms with various forms of priv-

5. We use "city" to refer to both provincial capitals and local prefectures.

ilege, ranging from special deals to favorable treatments in political and judicial decisions (Ang and Jia, 2014; Hou, 2019; Bai et al., 2020; Xu, 2020).

As court budgets and personnel decisions are approved by the local party-state, local officials can pressure judges into favoring connected firms. Studies from different backgrounds have agreed on the existence of judicial favoritism and its substantial economic value for Chinese firms (Ang and Jia, 2014; Lu et al., 2015; Xu, 2020; Lei and Li, 2025). Notably, it is reported that Huawei has never lost a case in Basic court of Longgang,[6] and Tencent has an 94.3% win rate in the Basic Court of Nanshan and a 98.15% win rate in the Intermediate Court of Shenzhen.[7] Such judicial favoritism can translate into huge economic benefits, driving firms to rebuild patronage when it is disrupted, as shown below.[8]

3.3 The Judicial Centralization Reform

Collusion between local governments and firms may stimulate short-term growth but distorts judicial rulings and undermines social justice. Recognizing this structural flaw, the CPC launched a judicial centralization reform in 2014 to curb local political interference. Following the CPC Central Committee's directive to "promote unified financial and personnel management of local courts at the provincial level" in 2013, the SPC implemented the reform in 2014, transferring both fiscal and personnel authority over Basic and Intermediate Courts from county- and city-level governments to the provinces (see Figure 1). The reform was rolled out in a staggered manner, a common feature of Chinese policy experimentation (Wang and Yang, 2025). In 2014, 152 courts were designated as pilots, with the reform gradually expanding to other regions; by 2021, more than 3,500 local courts had adopted the reform (see Figure A1).

In reformed cities, local leaders could no longer pressure judges, thereby curbing local patronage networks (Panels A and B of Figure 2).[9] Yet authority was only shifted upward to the

6. See the Chinese news report https://bbs.mysipo.com/thread-1109742-1-1.html for detail.
7. See the Chinese news report https://baijiahao.baidu.com/s?id=1692466384041368633 for detail.
8. In our dataset, high-PRI firms face an average of 1.9 lawsuits per year, with total dispute values of 50.06 million RMB, roughly 7% of annual operating revenue. Even low-PRI firms face 1.2 cases annually, with disputes totaling 27.81 million RMB, roughly 3% of revenue.
9. Its effectiveness has been noted by legal scholars (Zhang and Ginsburg, 2019), economists (Liu et al., 2022), and political scientists (Lei and Li, 2025).

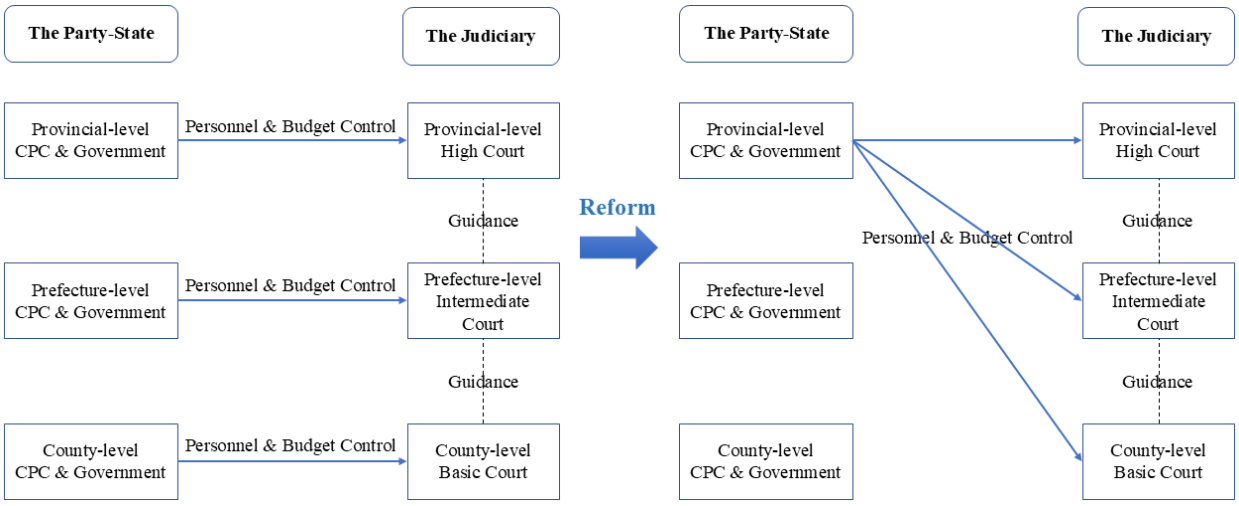


FIGURE 1 The Centralization Reform

Notes: Solid arrows indicate the party-state (starting point) that manages the personnel and budget of different tiers of courts (ending point). Dashed arrows indicate the higher-level court (start) that provides adjudication guidance to its lower-level court (end).

provincial level, making provincial capitals new centers of political influence. As a result, firms can strategically respond to the reform by reallocating investments. Market-oriented firms benefit from a cleaner business environment and less state-business collusion, while politically reliant firms suffer from reduced patronage. As shown in Figure 2, to maintain political advantages, the latter shifted investment toward provincial capitals, where provincial officials retained political influence over judges of all levels.

4 Data

4.1 Firm-Investment Data

We construct a firm-investment location-year panel dataset from the China Stock Market and Accounting Research Database (CSMAR), a widely utilized financial database covering China's privately-owned listed firms. The sample period spans 2012 to 2019. We begin in 2012 to exclude confounding effects due to leadership turnover, as President *Xi* assumed office that year. We end in 2019 to exclude disruptions related to the COVID-19 pandemic.

We extract the headquarters city of each parent firm from CSMAR and link it to the geo-

graphic locations of its subsidiaries using associated company files, constructing a panel at the firm–city–year level that records investment across cities. We exclude observations in which the investment location coincides with the parent firm's headquarters city, as well as those in directly administered municipalities to their distinct administrative status. Given that judicial reforms primarily affect political patronage channels among private firms, and that SOEs maintain vertically integrated political ties, we restrict our main sample to privately-owned listed firms and use SOEs as a placebo group in further analyses.

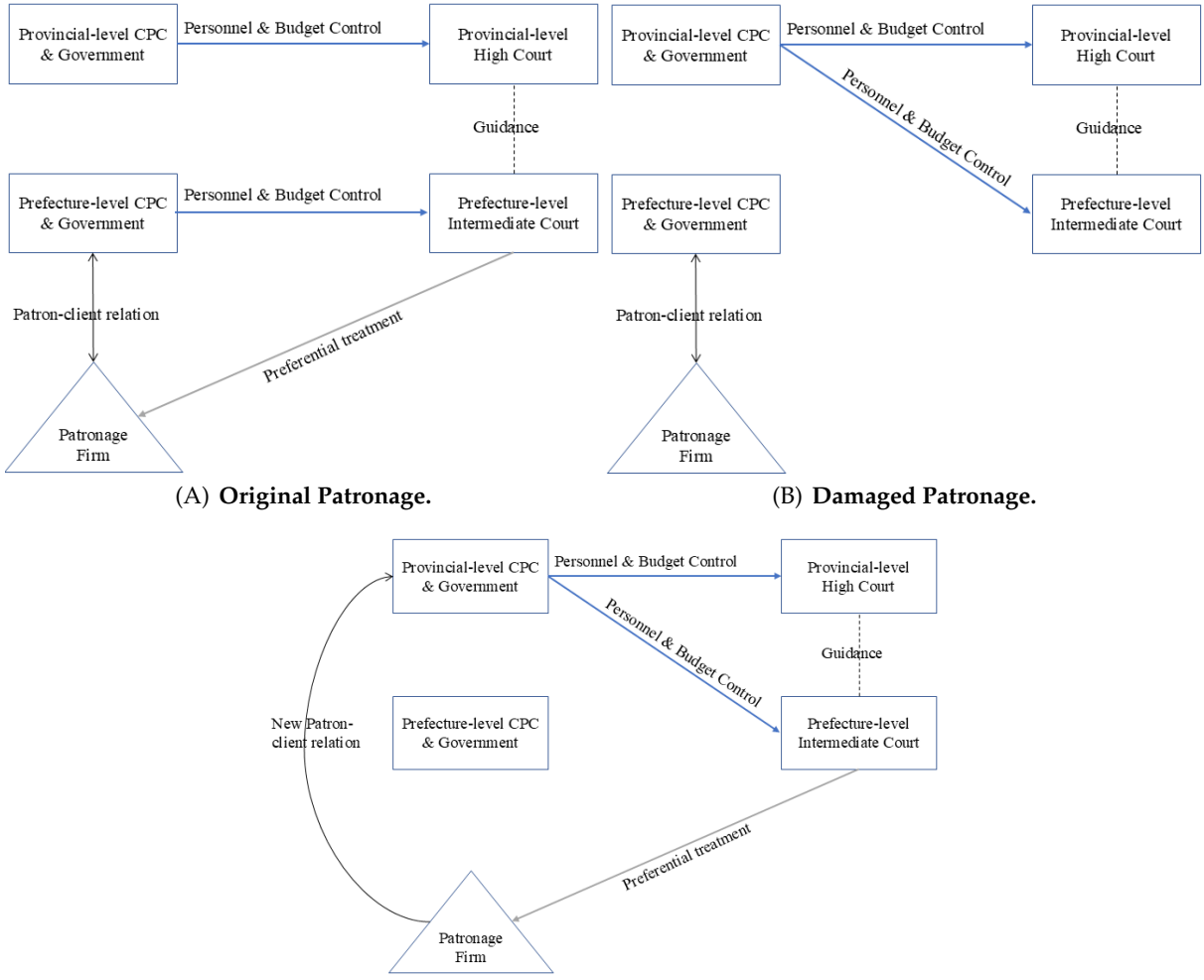


FIGURE 2 Patronage Networks before and after the Reform

Notes: Solid blue arrows indicate the party-state (starting point) that manages the personnel and budget of different tiers of courts (ending point). Solid gray arrows indicate the firm (starting point) that seeks preferential treatment from the party-state and courts (ending point). Dashed arrows indicate the higher-level court (starting point) that provides adjudication guidance to its lower-level court (ending point).

Table A1 presents summary statistics on firms' investment activities across cities. In our sample, 73% of firms have investments in cities outside their headquarters location, with inter-provincial investments exceeding intra-provincial ones. The average investment amount is 1.25 billion CNY, and the mean distance to investment destinations is approximately 874 kilometers, indicating geographically dispersed investment networks. The substantial variation in investment scale and geographic scope provides ample variation for our empirical analysis.

4.2 Measuring Political Reliance

We measure firm-level political reliance by the share of the conversation between institutional investors and firm management that centers on political issues, such as government policy or political environment. We obtain the data from Lin (2025), based on more than 400,000 Q&A transcripts texts from various meetings between institutional investors and listed firms, including quarterly earnings calls, IPO roadshows, analyst meetings, and other forms of investor-management conferences. Then we use supervised machine-learning model Bidirectional Encoder Representations from Transformers (BERT) to classify Q&A texts into nonpolitical or political contents. Finally, we calculate political reliance index (PRI) with the proportion of discussion focused on political topics during these interactions. As institutional investors have more knowledge about firms and indeed care a lot about the firm's development, if a firm's performance rely heavily on political patronage, then they would ask more questions related to politics (Hassan et al., 2019). Thus, higher PRI indicates that a firm's growth is driven more by political patronage than by market forces.

To validate the construct validity of our measure, we begin by demonstrating that PRI is not spuriously correlated with other observable firm characteristics. As shown in Figure A2, PRI exhibits no significant association with key dimensions of firm performance, including growth, operational efficiency, profitability, and other core financial indicators.[10]

10. One may be concerned that relying on politics does not mean relying on court trials, so firms with high PRI may care nothing about the judicial reform. However, in the context of China, it is those high-PRI firms that favor using lawsuits over informal tools (Ang and Jia, 2014). Correspondingly, we measure court-dependence explicitly and conduct heterogeneity analysis in Figure B2.

Next, we assess whether PRI plausibly captures political reliance. Drawing on a rich array of supplementary data—ranging from CEO biographies and government procurement records to firm annual reports and hand-collected Party-building information—we construct six independent indicators of political patronage. These include: (1) a hand-collected dataset of CEO biographical characteristics compiled from firm websites, public profiles, and platforms such as Wikipedia. Based on this information, we identify whether the CEO is a former government official, as well as the highest administrative rank attained, (2) the proportion of former officials in the senior management team, (3) the Economic Policy Uncertainty (EPU) index, computed from the frequency of policy-relevant language in corporate disclosures following Nie et al. (2020), (4) whether the firm has an internal CPC organization or the share of managers who are Party members, as a proxy for reliance on party-state institutions (Lu et al., 2025), (5) the firm's participation in public procurement, measured by both supplier status and transaction volume based on web-scraped official records,[11] and (6) the Firm-Government Agency Exposure index, which quantifies references to specific state agencies in annual reports and their frequency relative to total report length (Armstrong et al., 2025). As shown in Figure A3, PRI is positively correlated with all the six alternative measures. We also use these measures as robustness checks.

4.3 Timing of Judicial Reform

Following Liu et al. (2022) and Zhou et al. (2021), we compile information on the roll-out of judicial reform from the Yearbook of Judicial Reforms in China (2012–2019). Published annually by the Supreme People's Court, the yearbooks document the design, implementation, and progress of reform nationwide and include province-specific chapters detailing the timeline at the city level. We use the yearbooks to construct our baseline measure of reform onset across regions.

To address potential discrepancies between official plans and actual implementation, we validate this measure with local fiscal expenditure data. Following Lei and Li (2025), we code a court as reformed if either (a) its fiscal expenditures are listed under the provincial budget or (b) its host city's court spending falls by at least 50 percent relative to the baseline year when the province

11. See the China Government Procurement Website https://www.ccgp.gov.cn/index.shtml for detail.

was instructed to adopt the reform. Our results are robust to both definitions.

4.4 Other Data

Controls. We employ two datasets for covariates. We incorporate firm characteristics from the *CSMAR database*, including revenue, profitability, fixed asset ratio, asset growth rate, and whether the CEO also serves as the chairman of the board. City-level controls are drawn from the *China City Statistical Yearbook* and include total GDP, GDP per capita, the number of large-scale industrial enterprises, total bank loans, average wage, urbanization rate, and the composition of the local industrial structure. Table A2 reports descriptive statistics on our main variables.

Additional datasets. We also leverage several auxiliary datasets to support extended analyses. These include: (1) the judicial judgments database, (2) individual-level profiles of local officials, (3) the revolving-door officials database, (4) the political orientation Baidu keyword search index, (5) local market development indices, (6) public records on official corruption, and (7) various micro-level survey datasets. Detailed descriptions of these sources are provided in subsection A.3.

5 Empirical Strategy

5.1 Empirical Strategy Overview

This section outlines our empirical strategy for examining how the judicial reform affects firms' investment patterns, especially focusing on firms with different levels of political reliance. Figure 3 visualizes the structure of our dataset and the two empirical approaches employed.

We adopt two complementary identification strategies. The first is a difference-in-differences (DiD) framework that estimates the effect of judicial reform on firms' investment in local prefectures, leveraging variation in both reform timing and firms' political reliance. As shown in Panel A of Figure 3, we focus on firms' investments in local prefectures, and our sample is firm–local prefecture–year observations from 2012 to 2019. This allows us to track within-firm changes in

investment following the reform in each location.

Second, to examine firms' strategic reallocation toward regions where political patronage remains feasible, we construct a Bartik (i.e., shift-share) measure based on firms' pre-reform investment portfolios and the reform's staggered implementation across local prefectures. The firm's exposure to reform is a weighted average of reform status across its investment locations, and the weights are proportional to its pre-reform investment in that prefecture. As shown in Panel B of Figure 3, Prefecture 1 and 2 underwent the reform while Prefecture n didn't experience the reform. The firm above has investments in Prefecture 1 and 2 in base period, while the firm below has investments in Prefecture 2 and n. Therefore, although both firms face the same policy, the firm above will have a higher reform exposure than the firm below since it have more investment localities undergoing the reform.

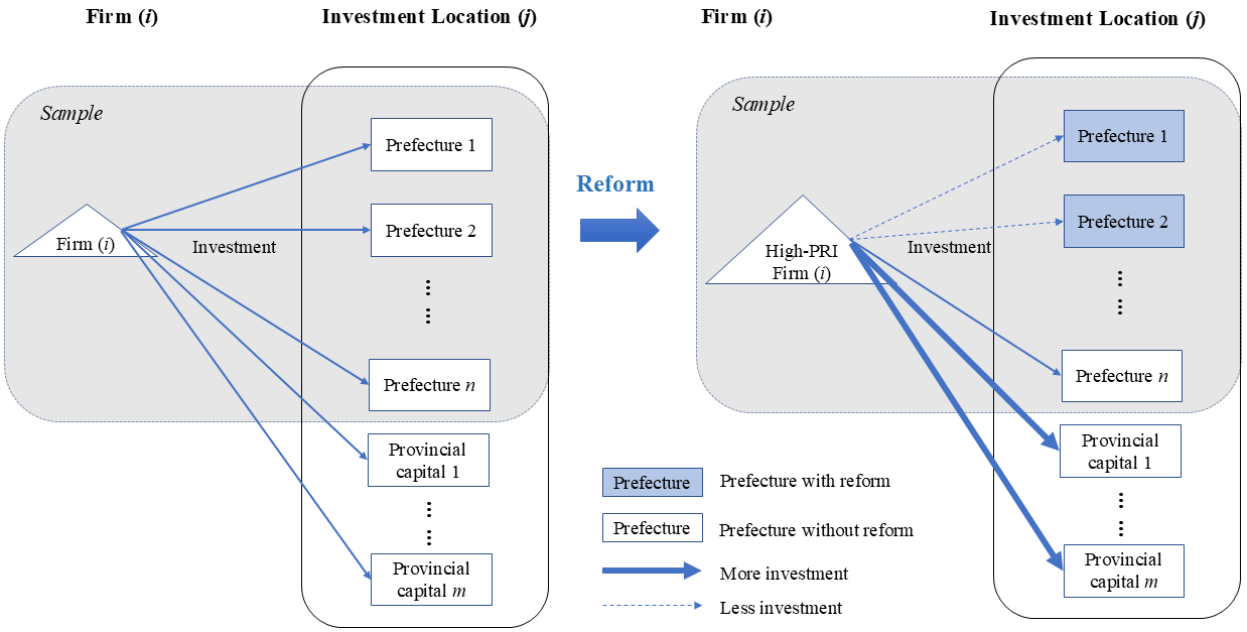
5.2 Difference-in-Differences Approach

We begin by applying the DiD approach, comparing the changes in investments across reformed and non-reformed prefectural cities over time. The specification is as follows:

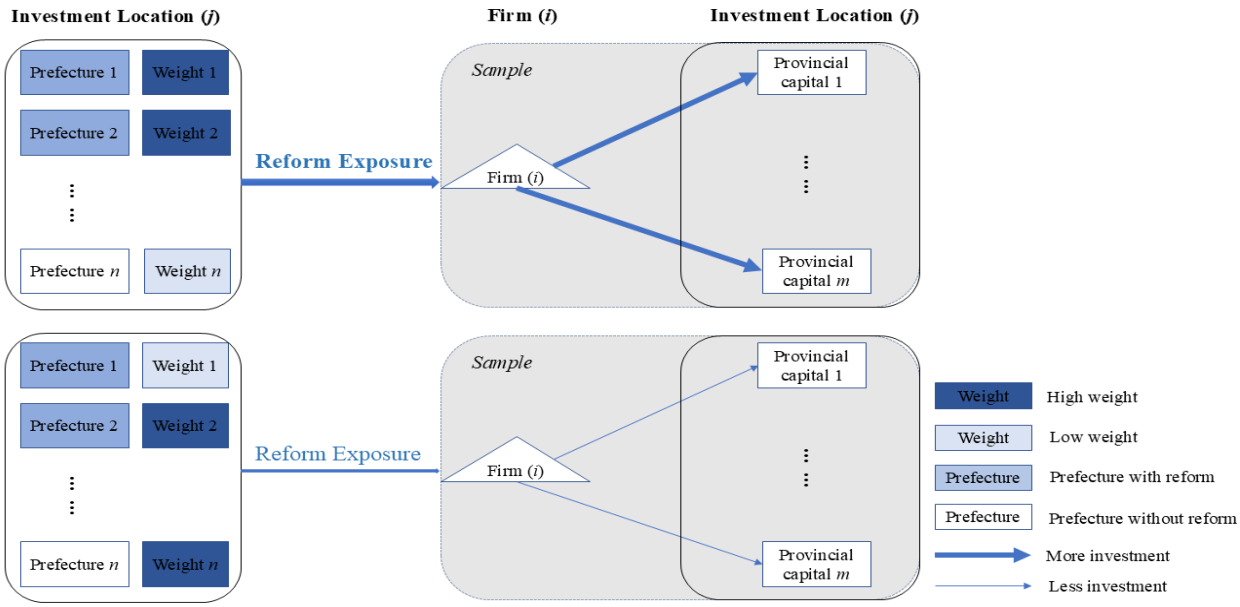
$$Y_{ic,jp,t} = \alpha_0 + \alpha_1 Reform_{jt} + \alpha_2 \phi' \boldsymbol{X_{ij}} \times T_t + \phi_{ij} + \lambda_{ct} + \theta_p \times t + \epsilon_{ic,jp,t}, \tag{1}$$

where the dependent variable $Y_{ic,jp,t}$ denotes either a binary indicator for investment activity or the logarithm of investment value made by firm i (located in city c) into city j , province p in year t. The treatment variable, $Reform_{jt} = 1$ if city j had implemented the judicial reform by year t.

To address potential confounders, we incorporate an extensive set of fixed effects. Firm-destination pair fixed effect ϕ_{ij} absorbs firms' investment preferences for specific cities, and other time-invariant characteristics of the firm and the investment destination, such as the firm's industry affiliation or the city's geographical features. λ_{ct} is the city-year fixed effect for the firm's headquarters location, controlling for time-varying factors of the firm's location, such as local industrial policy changes. $\theta_p \times t$ is the province-specific time trend, which accounts for time-varying investment conditions of the destination city. Finally, to control for other time-varying factors, we



(A) **Data Structure: Difference-in-Differences Approach.**



(B) **Data Structure: Bartik Design.**

FIGURE 3 Empirical Strategies.

Notes: Figure 3 visualizes our two empirical strategies and the corresponding data structures. Panel A presents the DiD design using the sample of non-provincial investment locations. The color intensity of each arrow indicates the theoretically expected change in investment magnitude, with dashed arrows representing reductions and thicker solid arrows denoting larger increases. Panel B is our second Bartik design identification strategy. It depicts two firms facing the same judicial reform policy but experiencing different intensities of reform exposure in non-provincial regions due to their different investment locations, and their investments in provincial capitals. On the left, blue cities indicate those that underwent the reform. The weight represents the intensity of the firm's investment in that location. The reform exposure is defined in a shift-share style as the sum of the product of whether the region was reformed (shift) and the importance of the region to the firm's investment (share).

include a set of predetermined covariates $\boldsymbol{X}_{ij}$ interacted with time dummies T_t. At the firm level, these include pre-policy revenue, ROA, CEO profile, fixed asset ratio, and asset growth; at the destination level, we include total GDP, per capita GDP, credit availability, wage levels, urbanization rates, and industrial structure. Standard errors are clustered at the (investment destination) city level since the variation we rely on stems from policy changes in the investment destinations.

The coefficient α_1 captures the average treatment effect on the treated (ATT) of judicial reforms on firms' investment. However, our theory underscores a *centralization paradox*: potential reform victims, political reliance firms, respond strategically through *spatial capture*, reallocating investment away from reformed jurisdictions. To explore this heterogeneity, (1) we estimate Equation 1 separately across industries with varying degrees of political reliance. We expect α_1 to be negative in politically reliant sectors and positive in market-oriented sectors; (2) we then formally incorporate this interaction into the regression framework, extending the baseline specification by interacting treatment exposure with firm-level political reliance index:

$$Y_{ic,jp,t} = \beta_0 + \beta_1 Reform_{jt} + \beta_2 Reform_{jt} \times PRI_i + \beta_3 \phi' \boldsymbol{X_{ij}} \times T_t + \phi_{ij} + \lambda_{ct} + \theta_p \times t + \epsilon_{ic,jp,t}. \quad (2)$$

Equation 2 introduces an interaction between the reform indicator and a the firm's pre-reform political reliance index PRI_i.[12] Equation 2 builds on the baseline by The coefficients β_1 and β_2 capture, respectively, the effect of reform on fully market-driven firms (i.e., $PRI_i = 0$) and the differential impact as firms' political reliance increases.

5.3 Bartik Design

Equation 2 captures firms' investment responses to judicial reform within local prefectures. The flip side of the *spatial capture* is a vertical reallocation of investment along the higher administrative hierarchy—spatially manifested as an investment concentration toward provincial capitals. Accordingly, we construct a reform exposure index, aggregating reform shocks across a firm's existing portfolio of local prefecture investments, weighted by pre-reform investment

12. We use pre-reform measure to exclude potential confounding between PRI_i and reform. Yet Empirically, the reform has only a negligible effect on firms' PRI_i. Alternative PRI measures are employed in robustness checks to ensure consistency.

shares, thereby capturing the intensity of reform shock by each firm. We then restrict the analysis to firm–provincial capital–year observations to assess how reform exposure in local prefectures influences investment in provincial capitals using the following equation:

$$Y_{ijcpt} = \gamma_0 + \gamma_1 Reform\ exposure_{it} + \gamma_2 Reform\ exposure_{it} \times PRI_i + \beta_3 \phi' \boldsymbol{X_{ij}} \times T_t + \phi_{ij} + \lambda_{ct} + \theta_p \times t + \epsilon_{ijcpt}, \tag{3}$$

where, $Reform\ exposure_{it}$ is the intensity of judicial reform exposed to firm i in year t. Considering that firms with more investment in a city would be affected more by that city's reform, the index aggregates reform shocks across all local prefectures, weighted by each firm's ex-ante investment distribution:

$$Reform\ exposure_{it} = \sum_{c \neq \text{provincial capital}} weight_{i,c,t=t_0} \times Reform_{ct} \tag{4}$$

Here, $Reform_{ct}$ is a dummy variable indicating whether city c implemented judicial reforms in year t, and $weight_{i,c,t=t_0}$ represents the importance of city c in firm i's base period (t_0) investment portfolio, calculated in two ways. First, we use *destination share*, a binary variable indicating whether the city is the firm's investment destination, divided by the total number of the firm's investment locations. Second, we use *amount share*, the firm's investment amount in the city, divided by the firm's total investment amount. In robustness checks, we re-estimate the weights using investment data from alternative pre-reform years to ensure that our findings are not sensitive to the choice of the base period. The parameters of primary interest in Equation 3 are γ_1 and γ_2. The former captures the effect of reform exposure on investment in provincial capitals for fully market-oriented firms (i.e., $PRI_i = 0$), while the latter measures how this effect varies with increasing political reliance.

5.4 Identification Checks

DiD: Parallel Trends Assumption. Our DiD identification hinges on the parallel trends assumption: in the absence of reform, the investment trajectories of firms in treated and untreated cities conditional on political reliance would have evolved similarly over time. We enhance this as-

sumption in two ways. First, we estimate a fully dynamic specification to both test for pre-trends and observe dynamic treatment effects:

$$Y_{ijcpt} = \beta_0 + \beta_1 Reform_{jt} + \sum_{m \neq -1} \beta_{2,m} PRI_i \times Post_{mj} + \beta_3 \phi' \boldsymbol{X_{ij}} \times T_t + \phi_{ij} + \lambda_{ct} + \theta_p \times t + \epsilon_{ijcpt}, \quad (5)$$

where $Post_{mj}$ denotes the years relative to the city j's first implementation of the judicial reform, and $\beta_{2,m}$ captures the effect of the reform on firm investment across different levels of PRI over relative years. Second, following Rambachan and Roth (2023), we formally assess the sensitivity of our findings to potential violations of the assumption.

DiD: Advanced DiD Estimation. Another concern in Equation 2 is the use of two-way fixed effects (TWFE) in a staggered treatment setting, which may deliver biased or even reversed-sign results when treatment effects are heterogeneous. To address this issue, we stratify firms by quartiles of political reliance, estimate ATEs within each group, and obtain consistent results using both TWFE and heterogeneity-robust estimators (Cengiz et al., 2019; De Chaisemartin and d'Haultfoeuille, 2024; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Gardner, 2022; Borusyak et al., 2024). [13] This also helps to relax the linearity assumption regarding the impact of PRI_i in Equation 2.

Checks for Bartik Design. To further assess the robustness of our Bartik specification, we conduct a series of sensitivity checks. Regarding the exogeneity of the shock ($Reform_{ct}$), we compare pre-reform characteristics between treated and never-treated cities (Borusyak et al., 2022). We also conduct three additional checks to ensure our results are not driven by specific shares or weighting choices. First, following Goldsmith-Pinkham et al. (2020), we exclude cities with disproportionately large Rotemberg weights (the top 1, top 3, and top 10 cities) to test whether a few outliers drive our results. Second, we use alternative measures of investment shares and alter the base period. Third, we perform a series of permutation tests, randomly reassigning investment shares

13. Recent studies have proposed a number of new estimators to overcome this issue (Cengiz et al., 2019; De Chaisemartin and d'Haultfoeuille, 2024; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Gardner, 2022; Borusyak et al., 2024), yet little is known about the way to estimate the staggered DiD with an interaction term.

and reform timings to construct pseudo–reform exposure measures and assess the resulting distributions. Finally, we re-estimate our main specifications using spatially clustered standard errors to account for spatial correlation in the residuals. All robustness checks confirm the stability of our baseline findings, shown in Appendix C.

Quasi-Exogeneity of Reform. Although the DiD design does not require exogeneity of the judicial reform, concerns remain that unobserved factors, such as local investment conditions, political leadership traits, or the severity of corruption and government–business collusion, might influence both reform rollout and firms' investment. Panel A and B of Figure A4 show that, as of 2012 (pre-reform period), cities that subsequently implemented the reform were not systematically different from those that did not in key economic characteristics and political conditions. In Panel C-F, we conduct these balance tests respectively for provincial capitals and local prefectures and find similar results across sub-samples.[14] Furthermore, we exclude the concern that the reform may target firms with specific features. As shown in Figure A5, all dimensions of firm characteristics are balanced between the never-treated firms and firms that are treated at various time (policy waves). Combined together, the judicial reform did not target certain regions or specific firms.

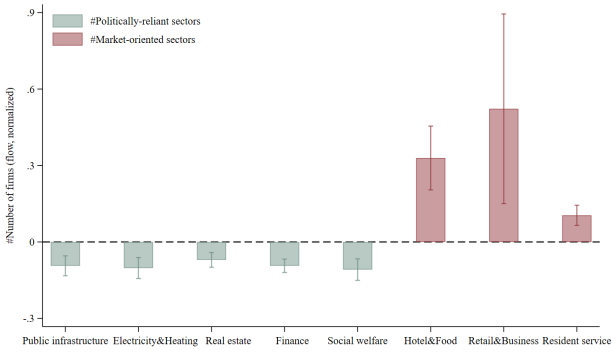
6 Empirical Results

6.1 Selective Disinvestment Following Pro-Market Judicial Reforms

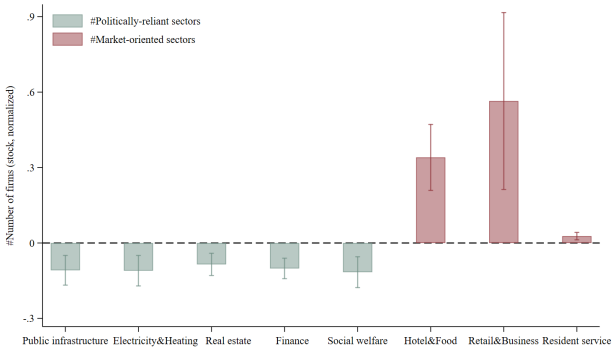
Subgroup Analysis by Sectors. We begin by estimating Equation 1 by sector at the city–year level, using *comprehensive business-registration records* covering virtually the universe of Chinese firms. Sectors such as infrastructure, electricity, real estate, finance, and social-security services exhibit higher political reliance (Hou, 2019), measured by a greater prevalence of CEOs with prior government service, larger exposure to policy uncertainty, denser CPC party presence, greater involvement in public procurement, and more frequent investor discussions linking business and politics, as validated in Figure B1. By contrast, sectors such as hotels, food services, retail, and res-

14. Consistent with our findings, Lei and Li (2025) shows that neither political ties nor the career histories of local officials predict the timing of reform rollout.

idential services are more market oriented. In Figure 4, Panel A plots the effects of judicial reform on new firm entries, while Panel B plots the effects on the stock of active firms. Consistent with our argument, firms in politically reliant industries (green) reduced local investment following the reform, while firms in market-oriented industries (red) expanded. Because these sector groupings are coarse, we then construct a continuous firm-level Political Reliance Index (PRI) using data on listed firms and estimate the interaction specification in Equation 2.



(A) **New firm (flow).**



(B) **Existing firm (stock).**

FIGURE 4 The Impact of Reform on Firm Entrance in Local Prefectures.

Notes: Figure 4 plots the impact of judicial reform on firm number across sectors in local prefectures using an interaction model with all sectors. The dependent variable in Panel A is the number of newly registered firms (flow) with normalization. In Panel B, the dependent variable is the number of existing firms (stock) with normalization. Columns show the pure effect of reform on different sectors and teal columns represent politically-reliant sectors while maroon ones display market-oriented sectors. All regressions control for city FE, year FE, province-specific time trends, and interactions between city characteristics and year dummies. Shaded areas denote 95% confidence intervals.

Baseline results. Table 1 presents the baseline DiD estimates from Equation 2, examining how judicial centralization influenced firms' investment decisions in local prefectures. Columns (1) uses a binary investment indicator as the dependent variable. Panel A includes firm-destination

pair fixed effects, headquarter-year, and destination-year fixed effects. The results indicate that while market-oriented firms modestly increased investment in local prefectures following the reform, politically reliant firms were significantly less likely to invest, since the reform weakened their access to patronage networks. Panel B adds a set of time-varying controls by interacting firm and destination characteristics with year dummies. Under this specification, a 10 percentage point (p.p.) increase in PRI is associated with a 0.035 p.p. decline in post-reform investment probability, equivalent to 8.5% of the mean (0.0349 / 0.4085). Given that firms typically invest in only a small number of locations, this magnitude is economically substantial, implying that politically reliant firms experienced a pronounced contraction in investment following the reform. The balanced PRI-scaled ratio ($|\beta_2/\beta_1| = 0.0687$) maps to the 35.5-th percentile in the PRI distribution, implying that roughly two-thirds of firms respond in line with the *spatial capture* mechanism, shifting investment away from reformed regions. Panel C further includes controls for firms' total investment levels to rule out the concern about overall capital contraction, yielding similar conclusions.

In column (2), we replace the dependent variable with the logarithm of investment amount and obtain consistent results. Under our preferred specification in Panel C, a 10-p.p. increase in PRI reduced post-reform investment growth by 0.5 p.p., equivalent to 7.1% of the mean (10 $\times$ 0.0506 / 0.0711). Equivalently, 28.2% of firms (more market-oriented) expanded investment in reformed regions, while the rest (more politically reliant) invested less.

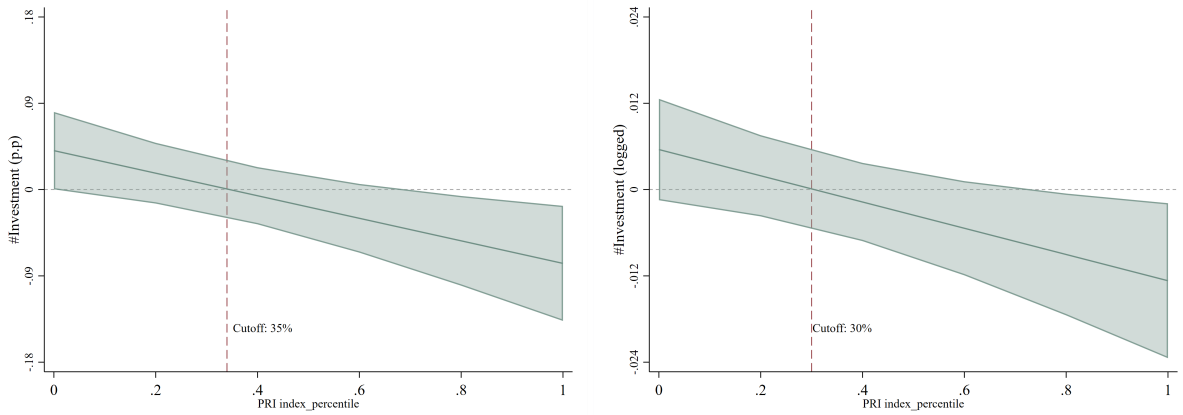
Figure 5 visualizes how judicial reform influenced firms' investment decisions across the distribution of PRI. The figure reveals that firms in the bottom 30-35% of the PRI distribution, those most market-oriented, were more likely to invest in reformed regions, while the rest became more likely to exit. This large-scale withdrawal by politically reliant firms likely imposed considerable economic costs on local economies given the size and scope of their investments.

Event study. Figure 6 plots dynamic DiD estimates from Equation 5. Panel A includes firm-destination, headquarter-year, and destination-year fixed effects; Panel B further interacts baseline firm and destination characteristics with year dummies. The absence of significant pre-treatment differences by PRI supports the parallel trends assumption. After the reform, investment patterns

TABLE 1 Baseline: Effects of Judicial Reform on Investment across Destinations

Sample:	Local prefectures	
Dependent variable:	Investment (p.p.)	Investment (logged)
	(1)	(2)
Panel A. *With basic FEs*		
PRI × Reform (β_2)	-0.3183***	-0.0492**
	(0.1218)	(0.0211)
Reform (β_1)	0.0154	0.0020
	(0.0184)	(0.0033)
Balanced PRI value ($\lvert\beta_1/\beta_2\rvert$)	0.0485	0.0405
Pr(PRI<balanced value) (%)	26.4959	22.5641
Panel B. *Control firm and city predetermined characteristics*		
PRI × Reform (β_2)	-0.3491***	-0.0540**
	(0.1223)	(0.0213)
Reform (β_1)	0.0240	0.0030
	(0.0191)	(0.0034)
Balanced PRI value ($\lvert\beta_1/\beta_2\rvert$)	0.0687	0.0547
Pr(PRI<balanced value) (%)	35.4915	29.1790
Panel C. *Additionally control firm total off-site investment (to local prefecture & provincial capital)*		
PRI × Reform (β_2)	-0.3244***	-0.0506**
	(0.1214)	(0.0211)
Reform (β_1)	0.0215	0.0026
	(0.0187)	(0.0033)
Balanced PRI value ($\lvert\beta_1/\beta_2\rvert$)	0.0662	0.0518
Pr(PRI<balanced value) (%)	34.3647	28.2194
Firm-location pair FE	✓	✓
Headquarters-year FE	✓	✓
Province (subsidiary)-year trend	✓	✓
Dependent-variable mean	0.4085	0.0711
Observations	2,386,162	2,386,162

Notes: Table 1 reports the impact of judicial reform on investment in local prefectures, using Equation 2. The unit of observation is at the firm-investment location-year level. In Column (1), the dependent variable is a binary indicator of whether the firm invested in the location, with the regression coefficient multiplied by 100 for interpretability, while Column (2) is the investment amount (logged). The key independent variable, $Reform$, is a dummy variable that equals 1 if the city has implemented judicial reform and 0 otherwise. The variable PRI is the firm's political reliance index. Panel A includes firm-investment destination pair fixed effects, headquarters-year fixed effects, and investment location-specific time trends. Panel B adds interactions between predetermined firm, destination characteristics and year dummies. Firm characteristics include revenue, ROA, CEO profile, fixed asset ratio, and asset growth while city characteristics incu predetermined GDP, per capita GDP, credit availability, wage levels, urbanization rates, and industrial structure. Panel C further adds time-varying firm's total annual investment amount (logged) to both provincial capital and local prefecture compared to Panel B. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.



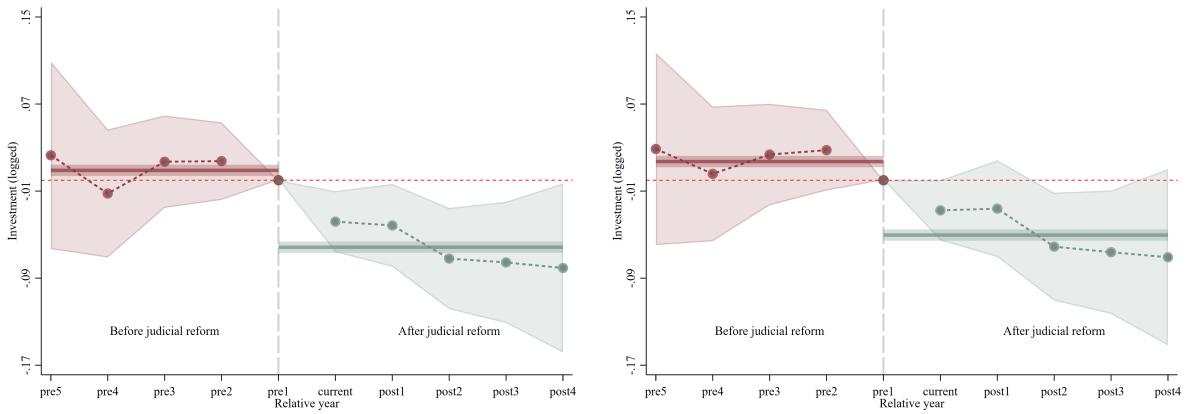
(A) **Investment (p.p.).** (B) **Investment amount(logged).**

FIGURE 5 Firm Investment Responses in local prefectures Post-reform.

Notes: Figure 5 plots firm investment responses in local prefectures following judicial reform, by percentile of the Political Reliance Index (PRI). The x-axis represents PRI percentiles. The dependent variable in Panel A is a binary indicator equal to one if the firm invested in the location; coefficients are multiplied by 100 for interpretability. In Panel B, the dependent variable is the investment amount(logged). All regressions control for firm–destination pair fixed effects, headquarters–year fixed effects, destination-specific time trends, and interactions between firm and destination characteristics and year dummies. Shaded areas denote 95% confidence intervals.

differ by PRI, and the difference becomes more pronounced over time: high-PRI firms experience a greater reduction in investment compared to low-PRI firms. As potential victims of the centralization reform, high-PRI firms strategically withdraw from reformed jurisdictions, engaging in *spatial capture* to seek new sources of patronage.

Advanced DiD estimation. Recent econometric research has raised concerns that two-way fixed effects (TWFE) estimators may yield biased estimates in staggered treatment settings with heterogeneous effects (De Chaisemartin and d'Haultfoeuille, 2024; Sun and Abraham, 2021; Cengiz et al., 2019; Gardner, 2022; Callaway and Sant'Anna, 2021). To address these concerns, we implement alternative heterogeneity-robust estimators in Figure B3. Across all estimators, pre-trends remain flat, supporting the parallel trends assumption. Following the reform, high-PRI firms exhibit a substantial decline in investment, while low-PRI firms show no significant change. These results echo Figure 6, providing robust support for our theory.



(A) **Basic FEs.** (B) **Basic FEs and full controls.**

FIGURE 6 Dynamic Effects of Judicial Reform on Investment in Local Prefectures.

Notes: Figure 6 plots the dynamic effects of judicial reform on firm investment by relative year using Equation 5. The dependent variable is the investment amount(logged). Panel A includes firm–destination pair fixed effects, headquarters–year fixed effects, and investment location-specific time trends. Panel B additionally includes interactions between firm and destination characteristics and year dummies, as well as the firm's total annual investment. Shaded areas denote 95% confidence intervals.

6.2 Strategic Investment Reallocation in Provincial Capitals

Our theory predicts that as local patronage erodes following the reform, high-PRI firms withdrew investment from reformed jurisdictions and re-invest upward, seeking new patronage in provincial capitals. With solid empirical evidence for the first part of the story, we then proceed to the second half. Table 2 presents estimates from the Bartik-specification outlined in Equation 3, restricting the sample to firm–capital investment pairs. In columns (1)–(3), the dependent variable is a binary indicator for whether the firm invests in the provincial capital. Panel A defines reform exposure using *destination share*: the positive coefficient on the interaction term suggests that firms with more political reliance became more inclined to invest in provincial capitals in response to the reform. When firms are fully exposed to judicial centralization, a 10 p.p. increase in PRI raise the probability of capital-city investment by 0.6 p.p., approximately 16% of the sample mean. The findings support the predictions in Hypothesis 2 that judicial centralization displaced, rather than dismantled, existing patronage networks.

Columns (4)–(6) use the log of investment amount as the dependent variable and yield similar results: politically reliant firms increased their investment in provincial capitals post-reform.

Panel B replicates the analysis using an alternative weight (*amount share*), with robust results. In sum, judicial centralization did not crowd out politically reliant firms but facilitated their strategic investment relocation through *spatial capture*, allowing state-business collusion and political patronage to persist at a higher level.

TABLE 2 Baseline: Effects of Judicial Reform on Investment in Provincial Capital

Sample:	Provincial capital					
Dependent variable:	Investment (p.p.)			Investment (logged)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. *Investment destination share*						
PRI × Reform exposure	6.3379***	6.1612***	7.0046***	0.9862***	0.9612***	1.0969***
	(1.8619)	(1.8356)	(1.9227)	(0.3243)	(0.3201)	(0.3343)
Reform exposure	-2.3593***	-1.8797***	-0.8357**	-0.3762***	-0.2990***	-0.1317*
	(0.4637)	(0.4373)	(0.3974)	(0.0789)	(0.0744)	(0.0669)
Panel B. *Investment amount share*						
PRI × Reform exposure	4.4844**	4.0983**	4.8757**	0.7232**	0.6631**	0.7872**
	(1.9156)	(1.8794)	(1.9404)	(0.3278)	(0.3217)	(0.3320)
Reform exposure	-1.8087***	-1.3632***	-0.4459	-0.2980***	-0.2262***	-0.0812
	(0.4536)	(0.4307)	(0.4154)	(0.0774)	(0.0734)	(0.0700)
Pre-policy city-level control × year		✓	✓		✓	✓
Pre-policy firm control × year		✓	✓		✓	✓
Base period (2012) investment × year			✓			✓
Firm-location pair FE	✓	✓	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓	✓	✓
Dependent-variable mean	3.6465	3.6607	3.6607	0.6171	0.6195	0.6195
Observations	240,011	238,670	238,670	240,011	238,670	238,670

Notes: Table 2 reports the impact of judicial reform on investment in provincial capitals, using Equation Equation 3. The unit of observation is at the firm-investment location-year level. In Columns (1)–(3), the dependent variable is a binary indicator of whether the firm invested in the location, with the regression coefficient multiplied by 100 for interpretability. In Columns (4)–(6), the dependent variable is the natural logarithm of the investment amount. The key independent variable, $Reform\ exposure$, measures the intensity of judicial reform impact on firms in local prefectures, defined by Equation 4. In Panel A, $Reform\ exposure$ is weighted by the number of investment destinations. In Panel B, it is weighted by the amount of investment. The variable PRI represents the firm's political reliance index. Columns (1) and (4) use basic FEs that identical to those in Table 1. Columns (2) and (5) add interactions between predetermined firm, destination characteristics and year dummies. Columns (3) and (6) additionally control interaction between base period (2012) total investment amount to local prefecture (logged) and year dummies. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

6.3 Falsification Tests

In this subsection, we present falsification tests to corroborate our findings. If the observed investment reallocation was driven by changing patronage networks, three patterns should emerge. First, the decline in investment in reformed regions should be attenuated when local leaders retain political control over the judiciary. Second, SOEs that rely on national rather than local political patronage should remain unaffected. Third, firms with both high litigation exposure and high political reliance should exhibit the strongest responses.

We begin by exploiting heterogeneity analysis in local political hierarchies. Some city-level party secretaries simultaneously held seats in the provincial standing committee (SC), the top decision-making body at the provincial level.[15] Despite the reform, these dual-role party secretaries could leverage their institutional authority to influence court outcomes through the provincial SC and shield politically reliant firms, thereby weakening the reform's effect. We partition our sample into regions with dual-role party secretaries and those without, and estimate the results in Table 3: politically reliant firms reduced investment only in regions without dual-role party secretaries. In regions with a leader serving on the provincial SC, high-PRI firms actually increased their investment after the reform.

We then estimate Equation 2 and Equation 3 using a subsample of central SOEs, which are embedded in national-level political ties and depend less on local patronage networks (Wang, 2016, 2018). Table B1 presents the results, showing no significant change in investment for these firms following the judicial reform. This null finding reinforces our theoretical claim: the observed investment reallocation is driven by the disruption of local patronage ties and the strategic search for new connections, so firms that do not rely on local patronage will not respond to the reform.

Taken together, these two tests corroborate Hypothesis 3 and reinforce the theoretical mechanism proposed in section 2. They demonstrate how the logic of spatial capture extends beyond the Chinese case. Consistent with our broader framework, such dynamics are most salient in settings with strong state capacity but limited institutional constraint, where centralizing reforms disci-

15. Each provincial SC typically includes 7–13 members, including several city-level party secretaries. The SC hold authority over all key governance domains, including economic, political, and certainly, judicial affairs.

pline local elites yet inadvertently reallocate rent-seeking upward, reproducing the centralization paradox beyond the Chinese case.

Finally, we examine heterogeneity in firms' dependence on judicial rulings. If the reform weakened local officials' ability to shape court outcomes, firms that were both politically reliant and frequently involved in litigation should experience the sharpest contraction. Figure B2 shows that firms with high PRI and high litigation exposure reduced investment in reformed prefectures significantly more than firms with lower PRI or limited exposure. This provides strong evidence that the reform affects firm investment through court rulings.

TABLE 3 Falsification Tests: Local Political Hierarchies

Dependent variable:	Investment (p.p.)		Investment (logged)	
	Whether prefecture party secretary cum provincial standing committee member			
	Yes	No	Yes	No
	(1)	(2)	(3)	(4)
PRI × Reform	0.3194	-0.2965**	0.0556	-0.0436**
	(0.2265)	(0.1184)	(0.0387)	(0.0205)
Reform	-0.0416	0.0290	-0.0075	0.0042
	(0.0483)	(0.0190)	(0.0086)	(0.0033)
Reform	-0.1523	0.0290	-0.0270	0.0042
Pre-policy city-level control × year	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓
Dependent-variable mean	1.2631	0.3446	0.2168	0.0601
Observations	161,028	1,996,952	161,028	1,996,952

Notes: Table 3 reports falsification tests results, considering whether cities have provincial-level political ties. The unit of observation is at the firm-investment location-year level. In Columns (1) and (3), the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Columns (2) and (4), the dependent variable is the natural logarithm of the investment amount. The key independent variable, $Reform$, is a dummy variable that equals 1 if the city has implemented judicial reform and 0 otherwise. The variable PRI represents the firm's political reliance index. Columns (1) and (3) include samples where the city's party secretary also holds a position in the provincial standing committee, while Columns (2) and (4) include samples where such a political connection does not exist. Control variables and FEs are identical to those in Table Table 1. Standard errors are clustered at the city level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6.4 Robustness Checks

This subsection summarizes a series of robustness checks, with detailed specifications provided in Appendix C. If applicable, these checks are conducted for both the DiD design (Equation 2) and the Bartik design (Equation 3).

First, we conduct additional identification checks. For the DiD specification, following Rambachan and Roth (2023) and Ge et al. (2025), we formally assess the sensitivity of our estimates to deviations from parallel trends. As shown in Figure C1, the main coefficients remain statistically significant under moderate deviations. For the Bartik design, we test its sensitivity to the share component. Specifically, we exclude cities with high Rotemberg weights (Figure C2), recalculate weights using alternative base period (Table C1), and conduct separate permutation tests on both the share and shift components (Figure C3). We also conduct permutation tests for PRI index and policy implementation year (Figure C3).

Second, we apply alternative constructions of PRI. We begin by testing different functional forms, including a binary above-median indicator, a 2015-only measure, a time-varying index, and a log-transformed version to reduce the influence of outliers (Table C2). We then validate PRI using other proxies of political reliance, including CEO revolving-door backgrounds, economic uncertainty exposure, firm-level party-building engagement, and firms' dependence on government procurement (Table C3). Across all variants, our findings remain highly consistent.

Third, we account for some confounders. We first modify the estimation sample and introduce additional controls. These modifications include: (i) incorporating firms' local investment samples (in the parent company's location); (ii) including samples from province-directly-administered counties; (iii) excluding non-balanced panel firms, financial firms, or never-treated samples; and (iv) controlling for firm-year covariates and destination-specific characteristics, as well as capping excessively high investment amounts (Table C4, Table C5). We also incorporate detailed controls for local official characteristics, including their gender, age, birthplace, education, and ethnicity (Table D6). Meanwhile, we eliminate interference from other concurrent judicial reforms (Table D7). Finally, we adopt an alternative definition of the policy implementation timeline, fol-

lowing Lei and Li (2025) (Table D8). Across these specifications, our findings remain robust.

Finally, to account for within-firm correlation in investment and control for spillover, we use alternative low-anticipatory control group and cluster standard errors (Figure C4). We also re-estimate our main specifications using heteroskedasticity and autocorrelation consistent (HAC) spatial standard errors (Conley, 1999) in Figure C5 and obtain consistent results.

7 Mechanism: Spatial Capture and Patronage Reproduction

This section examines the logic behind the observed investment reallocation. The *spatial capture* mechanism arises from the centralization paradox: judicial centralization does not eliminate resilient patronage ties but reshapes their spatial distribution by reconstituting the patronage network. First, by dismantling local patronage networks, the reform erodes the judicial favors that politically reliant firms previously enjoyed. Second, provincial capitals—where political authority remains concentrated and patron-client networks persist—emerge as new havens for politically reliant firms. Our empirical tests provide robust support for the above hypothesized channels.

7.1 Uneven Effectiveness of Centralization Reform

We begin by presenting micro-level survey evidence on how judicial centralization shaped public perceptions of the judiciary. Using the 2012–2018 waves of the China Family Panel Studies (CFPS), we construct city-year measures of satisfaction with local courts and perceived judicial fairness. Panel A of Table D1 shows that both measures improved significantly following the reform, but only in local prefectures. In provincial capitals, public perceptions remain largely unchanged. These findings support our argument that judicial reforms reduced political interference in local prefectures while leaving capitals largely intact. To exclude the influence of broader shifts in public sentiments, Panel B conducts placebo tests using respondents' evaluations of local companies and fairness in firm promotion. These tests reveal no significant changes, which shows that the observed improvements are ascribed to changes in the judicial system.

Next, we assess how judicial centralization affected firm-level litigation outcomes. Following

Lei and Li (2025), we compile firm-level lawsuit data from the Wind Database, a comprehensive source for financial and legal information on Chinese listed firms. Table D2 reports the changes in firms' litigation success rates: in local prefectures, market-oriented firms saw an increase in win rates after the reform, while politically reliant firms experienced a decline. These patterns hold whether the outcome is defined as partial or complete win. Quantitatively, a 10 p.p. increase in PRI is associated with a 0.15 p.p. reduction in win rates, equivalent to 40.6% of the mean win rate (0.151/0.372). Notably, we find no change in the win-rate gap between high- and low-PRI firms in provincial capitals. These findings not only accord with Lei and Li (2025) in showing the effectiveness of the reform (weakening political interference in court rulings), but also support our claim that provincial capitals become new hubs for political patronage. This pattern marks a critical point in the logic of the *centralization paradox*: despite strong top-down enforcement capacity, authoritarian centralization reforms aimed at constraining state–business collusion often fail precisely at the centers of power.

7.2 Reduced Patronage in Local Prefectures

We leverage cross-regional variation to substantiate our mechanism. If the observed investment withdrawal of high-PRI firms stems from waning local patronage networks, the reduction should be more pronounced in regions where the reform more effectively strengthened market institutions and where pre-reform investments were more tightly tied to political patronage.

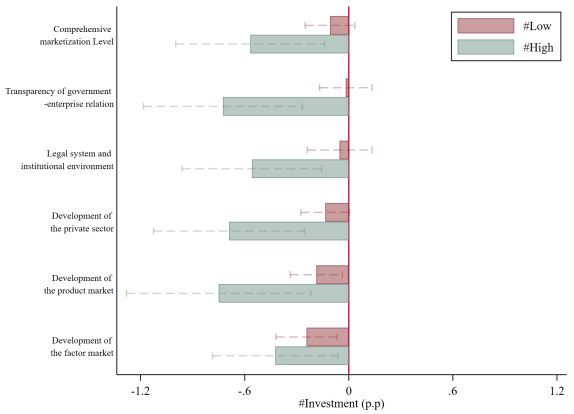
Post-Reform Market Orientation. In areas with more advanced post-reform market institutions, politically reliant firms would face greater disadvantages and thus reduce investment more sharply. We classify regions based on the *Fan Gang Marketization Index* and its five subcomponents, in which higher values indicate stronger market institutions. Panel A and B of Figure 7 show that following the reform, high-PRI firms significantly reduced investment in high-marketization regions relative to low-marketization ones. Regression results in Table D3, which interact the PRI index with each marketization sub-component, further confirm the pattern.

Pre-Reform Patronage Dependence. Firms more deeply embedded in local patronage networks prior to reform should face greater disruption and thus retreat more. Following Li (2024) and Li and Lu (2025), we use two proxies to measure pre-reform political embeddedness: (1) the amount of government subsidies received, and (2) the presence of former government officials among top executives ("revolving-door" managers). We compute pre-reform (2014 or 2011–2014 average) values of these measures at the firm-city pair level and interact them with our baseline DiD term. As shown in Table D4 and Table D5, firms receiving more subsidies or having more revolving-door managers experienced significantly greater post-reform investment withdraw. Panels C-F of Figure 7 illustrate that high-PRI firms sharply reduced investment in high–patronage-dependence regions relative to those with low dependence. These results further support the central role of disrupted government–business collusion in driving firms' strategic reallocation.

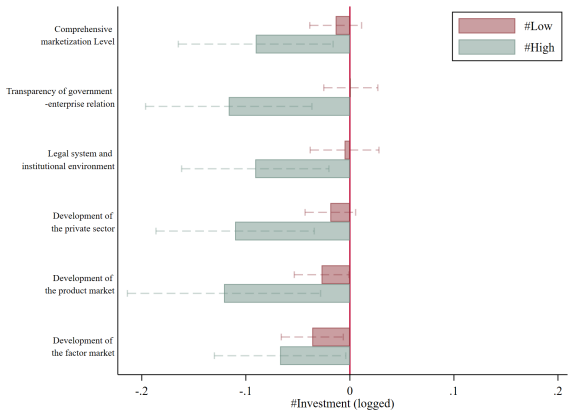
7.3 Resilient Patronage in Provincial Capitals

We next provide both indirect and direct evidence that provincial capitals have become new hubs of political patronage—the most salient manifestation of *spatial capture* and a central source of the centralization paradox. Indirectly, we show that high-PRI firms disproportionately increased investment in provincial capitals with higher levels of corruption, stronger political orientation, and more firm–government ties, consistent with a strategic search for new patronage. Directly, we show that high-PRI firms allocate greater benefits to officials with prior provincial-level (but not prefectural-level) positions, reflecting targeted efforts to co-opt provincial elites and sustain resilient patronage ties at higher administrative levels.

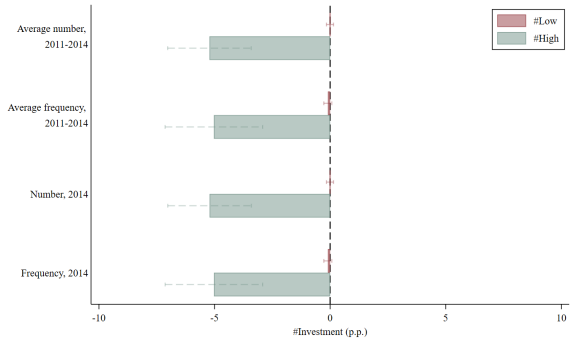
Corruption, Favoritism and Investment Attraction. As high-PRI firms shifted investment to provincial capitals to build political ties, more corrupt capitals, where officials were more prone to collude with firms, would attract greater inflows. Following Zhu (2017), we measure corruption by objective corruption cases, obtained from local government websites and cross-validated with social media sources. We construct two measures based on the pre-reform period (2011–2014): (1) the number of city-level officials investigated for corruption (*Unweighted Investigate*) and (2) a



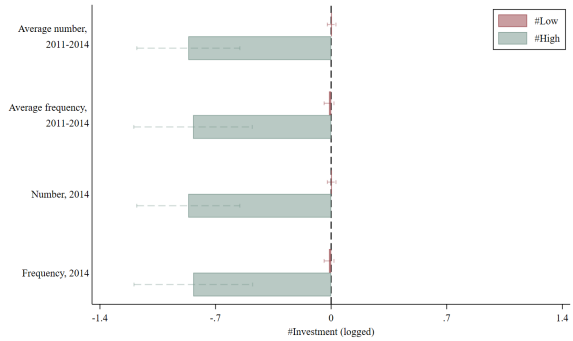
(A) **Marketization level, p.p..**



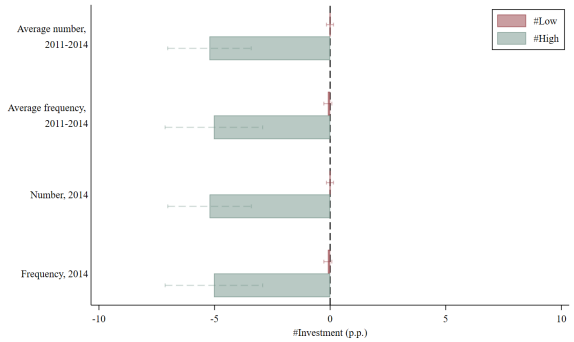
(B) **Marketization level, logged.**



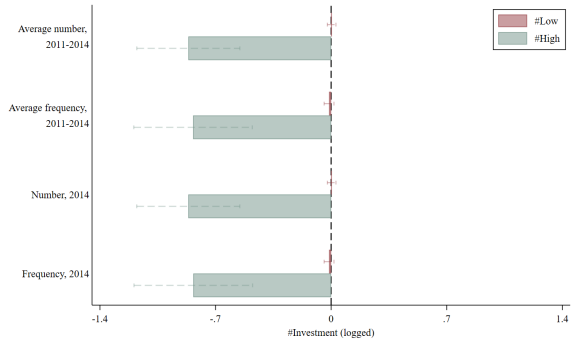
(C) **Predetermined government subsidy, p.p..**



(D) **Predetermined government subsidy, logged.**



(E) **Predetermined government revolver , p.p..**



(F) **Predetermined government revolver, logged.**

FIGURE 7 Heterogeneous Effects of Judicial Reform on Investment in Local Prefecture.

Notes: Figure 7 reports the heterogeneous effects of judicial reform in local prefecture cities by different marketization level, predetermined government subsidy, and government revolver. We split the sample by different median values of the above indices and estimate Equation 2 separately for low and high regions sample. Marketization level is derived from *Fan Gang Marketization Index*. Government subsidy is measured by the average frequency of government subsidies received by the firm and the number of subsidy projects in 2014 (or from 2011 to 2014). Government revolver is measured by whether the firm's senior management includes revolving-door officials and by the average number of such officials in 2014 (or from 2011 to 2014). Red bars correspond to low capitals; green bars show estimates for high ones. The plotted coefficients represent the interaction between judicial reform exposure and PRI. The dependent variable in Panels A, C, E is a binary indicator equal to one if the firm invested in the location, with coefficients multiplied by 100 for interpretability. In Panels B, D, F, the dependent variable is the investment amount(logged). Fixed effects and controls are identical to those in Figure 5. Dashed lines denote 95% confidence intervals.

rank-weighted index reflecting the seniority of investigated officials (*Weighted Investigate*).[16] Both measures capture the ex-ante intensity of government–business collusion in provincial capitals.

We classify provincial capitals into high- and low-corruption groups using the two pre-reform corruption measures and estimate Equation 3 separately for each group. Panels A and B of Figure 8 presents the results, showing high-PRI firms exposed to judicial reform systematically redirected investment toward more corrupt capitals, with much weaker effects observed in less corrupt ones. Table D6 further supports this mechanism in a triple-difference specification.

We further validate this channel by leveraging government procurement data to capture the extent to which provincial capitals favor politically reliant firms. Specifically, we identify the share of local procurement contracts awarded to firms with prior political ties. In Table D7, Panel A interacts the baseline with a dummy indicating whether connected firms secured a larger contract share than unconnected firms. Panel B replaces the dummy with the share of contracts awarded to connected firms. The positive and significant coefficients on the triple interaction suggest that, following reform, high-PRI firms disproportionately expanded investment in capitals whose governments exhibited stronger favoritism toward politically reliant firms. Panels C and D of Figure 8 illustrate this pattern. Estimating Equation 3 separately for cities with high and low levels of favoritism in public procurement, we find that politically reliant firms are more inclined to invest in provincial capitals with greater government favoritism toward connected firms.

Political Orientation. Regions with stronger political orientation are more likely to deliver patronage and less oriented toward market competition. To proxy for the salience of political orientation, we use *Baidu* Search Indices related to Communist ideology and Party-building. A higher local search frequency for such keywords indicates a stronger emphasis on Party ideology and political loyalty over market-oriented values.[17] Panels E and F of Figure 8 stratify provincial capitals by the median level of political orientation and re-estimate Equation 3, showing that high-PRI firms with greater reform exposure increased investment significantly in capitals exhibiting

16. For more detail about this index, see subsection A.3.

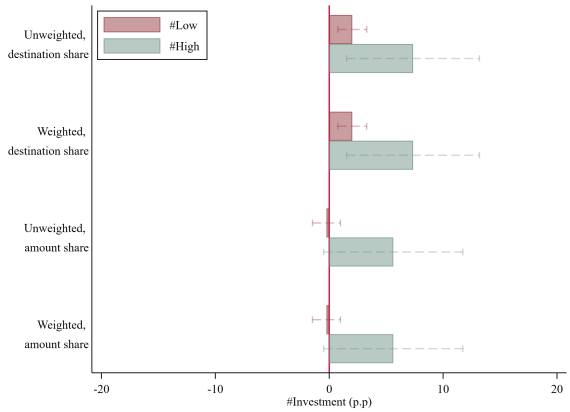
17. The *Baidu* Search Index measures the frequency of keyword searches by users, providing geographically disaggregated, behavior-based indicators of political orientation. Given *Baidu*'s dominance in China's search engine market, these indices offer a credible proxy for local political orientation. Following the standard approach, we normalize the annual average of each index at the prefectural level.

stronger political orientation. This pattern is robust across alternative definitions of search behavior (desktop vs. mobile) and reform exposure. Table D8 reinforces this evidence by including triple interactions with the standardized political orientation index.

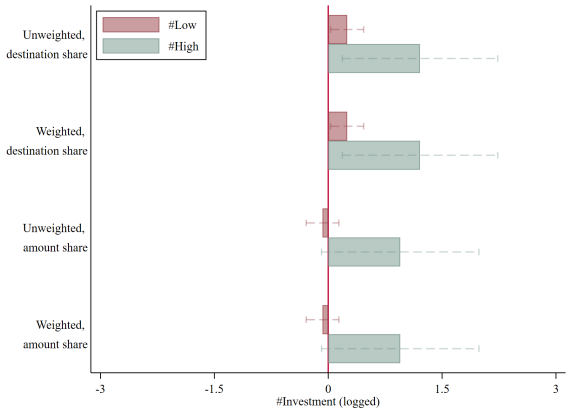
Other Factors. To address the concern that other factors may drive the observed investment reallocation pattern, Figure D1 examines three-way interactions between *PRI*, *Reform Exposure*, and a comprehensive set of location characteristics. Specifically, we test whether high-PRI firms facing greater reform exposure disproportionately reallocate to provincial capitals with advantages in (i) natural and resource conditions (land availability, water resources), (ii) economic conditions (industry and service structure), (iii) fiscal capacity (public expenditure, local debt, government R&D support), (iv) human capital (share of college-educated or high-school-educated residents), (v) transportation infrastructure (passenger and cargo volume), and (vi) environmental quality (air pollution levels). Across all specifications, the triple-interaction coefficients remain statistically indistinguishable from zero. Together with the heterogeneous effects documented in the previous subsection, these results indicate that, instead of choosing economically or geographically superior sites, high-PRI firms respond to judicial centralization primarily by seeking new patronage opportunities.

7.4 From Spatial Reallocation to Patronage Reproduction

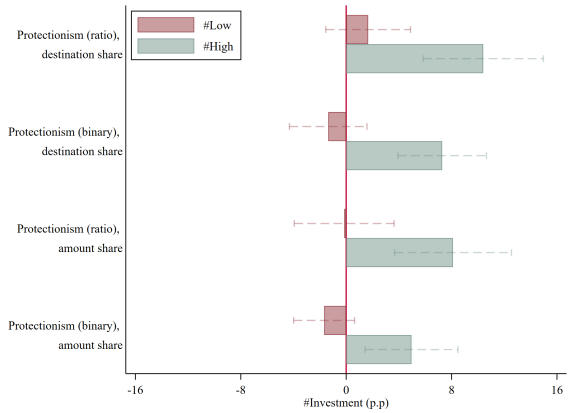
Finally, we provide some direct evidence for how firms reproduce patronage. Using a a novel dataset on compensation paid to revolving-door officials (Li, 2024; Li and Lu, 2025), we examine whether firms selectively co-opt officials with prior provincial-level positions, who may have political ties within the provincial bureaucracy. Using firm-official-year panel, Table 4 provides strong support for this channel: high-PRI firms exposed to greater reform intensity offer significantly higher compensation to officials who previously held provincial-level posts. By contrast, no such pattern emerges for officials with only prefecture-level experience. This suggests that the value of local patronage ties declined while provincial ties retained and even grew in importance. Expecting this, firms adjust their compensation for revolving-door officials who may



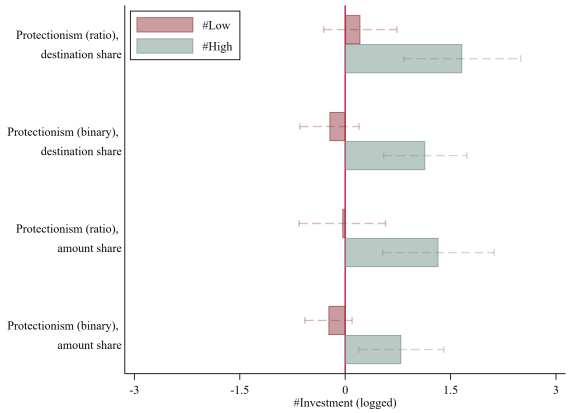
(A) **Predetermined corruption, p.p..**



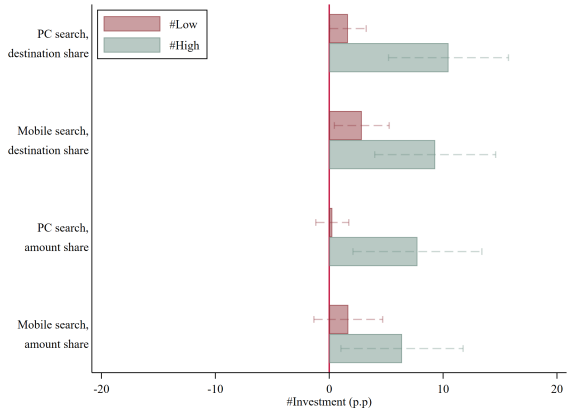
(B) **Predetermined Corruption, logged.**



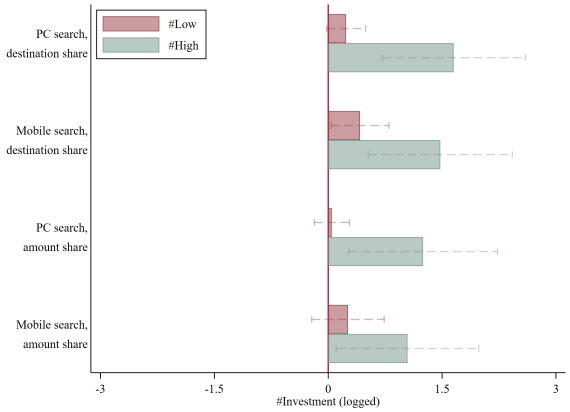
(C) **Predetermined Government Favoritism, p.p..**



(D) **Predetermined Government Favoritism, logged.**



(E) **Predetermined Political Orientation, p.p..**



(F) **Predetermined Political Orientation, logged.**

FIGURE 8 Heterogeneous Effects of Judicial Reform on Investment in Provincial Capital.

Notes: Figure 8 reports the heterogeneous effects of judicial reform in provincial capital cities by different predetermined corruption level, predetermined political orientation level, and predetermined government favoritism level. We split the sample by different median values of the above indices and estimate Equation 3 separately for low and high regions sample. Political orientation is measured by the frequency of online searches for politically relevant keywords, such as "Communist Party" and "Party-building". We use both PC-based and mobile-based search frequencies to capture this variation. Government favoritism is measured by the share of procurement awarded to connected firms. Red bars correspond to low capitals; green bars show estimates for high ones. The plotted coefficients represent the interaction between judicial reform exposure and PRI. The dependent variable in Panels A, C, E is a binary indicator equal to one if the firm invested in the location, with coefficients multiplied by 100 for interpretability. In Panels B, D, F, the dependent variable is the investment amount(logged). Fixed effects and controls are identical to those in Figure 5. Dashed lines denote 95% confidence intervals.

TABLE 4 Mechanism: Co-opting Provincial or Above Level Revolving-Door Officials

Dependent variable:	Salary		Allowance		Total earning	
	Above-provincial-level official before entering the firm					
	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)
PRI × Reform exposure	-1.4983	1.5382*	0.1993	2.1931**	-1.4606	1.6351*
	(1.2133)	(0.8515)	(0.8323)	(0.9380)	(1.2053)	(0.8769)
Reform exposure	0.1853	-0.4912**	-0.0120	-0.5691**	0.1823	-0.5134**
	(0.1577)	(0.2371)	(0.0980)	(0.2640)	(0.1570)	(0.2455)
Officials individual controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓
Province-year trend	✓	✓	✓	✓	✓	✓
Observations	1,721	929	1,721	929	1,721	929

Notes: Table 4 examines how firms' exposure to reform influences their efforts to co-opt revolving-door officials through changes in compensation. The unit of observation is at the firm-official-year level. In Columns (1)–(6), the dependent variables are the annual salaries, allowances, and total earnings of revolving-door officials. All the dependent variables are normalized. The key independent variable, $Reform_exposure$, is weighted by the proportion of investment locations affected by the reform relative to the total number of investment locations, defined in Equation 4. The variable PRI represents the firm's political reliance index. Control variables for individual revolving-door officials include age, age squared, gender, department of employment, and the year they transitioned from government to the firm. Standard errors are clustered at the city level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

possess provincial-level political ties.

8 Conclusion

This paper theorizes the *centralization paradox*: ambitious top-down efforts to centralize authority may undermine their own objectives facing strategic adaptation of societal agents. We identify a novel mechanism of *spatial capture*, whereby firms strategically reallocate investment across space to rebuild patronage ties disrupted by reform. Leveraging China's judicial centralization, we show that politically reliant firms retreat from regions where local patronage wanes and reinvest in provincial capitals where such ties persist. These findings contribute to broader debates on institutional change and authoritarian rule. Contrary to the view that centralization curb collusion (Malesky et al., 2014; Boffa et al., 2016), our results align with emerging work on the limits of centralization reforms (Wang, 2024). Reform outcomes are best understood as *co-evolutionary equilibria* between state and society: rather than eliminating political patronage, centralization reconfigures and reproduces patronage in new forms that often diverge from reformers' intent.

Our study encourages new perspectives on reforms. We contribute to what Migdal (2001) refers to as a "state-in-society" approach, emphasizing *bottom-up* societal adaptation more than *top-down* process: reform is not only a top-down process of implementing institutional blueprints but also a bottom-up process involving interaction between state and society. Under weak institutional constraints, top-down initiatives can be co-opted through strategic adaptation, not only for firms but also political parties, social organizations, and citizens. Such strategies hinder reform effectiveness, leading to a fundamental divergence between on-the-ground practices and formal rules (Gans-Morse, 2017).

By theorizing *spatial capture*, this study also advances research on the political geography of power that fundamentally shapes the trajectory of institutional change. Through spatial reallocation, patronage networks are not dismantled but reconstituted across regions. Thus, space itself operates as a mechanism of institutional resilience (Bisbee and You, 2024). In China, this logic helps explain increasing high-level capture in the past forty years (Wang, 2016), and more

recently, the rise of provincial capitals as new centers of political discretion and economic concentration (Lu and Ma, 2025). More generally, the fact that investment goes hand-in-hand with concentrated power implies how shifts in political power reorganize market structures, inducing firms to cluster spatially around newly empowered centers.

Finally, our theory extends beyond authoritarian regimes. In weakly institutionalized democracies, such as post-Suharto Indonesia (Slater, 2010), modern Brazil (Ferraz and Finan, 2011; Toral, 2024), or Japan's developmental-state era (Catalinac, 2016), efforts to centralize authority likewise reconstituted patronage as actors adapted to shifting centers of power. These parallels suggest that the dilemmas of centralization stem not from regime type but from institutional weakness: wherever formal constraints are weak and informal networks persist, reforms designed to discipline patronage often relocate rather than remove it. This broader logic cautions the conditional benefits and (de)centralization (Cai and Treisman, 2006) and invites further comparative studies into how spatial adaptation shapes the geography of power and the consequences of centralization across political systems.

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Online Appendix for "The Centralization Paradox: How Authoritarian Reforms Reproduce Patronage through Spatial Capture"*

Siyuan Fan Wenyi Lu Shangkun Xie

Appendix A More on Background and Data **A1**

A.1 Reform Progress A1

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*Siyuan Fan: Department of Political Science, UC San Diego, USA; email: sif004@ucsd.edu. Wenyi Lu: Wilf Family Department of Politics, New York University, USA; email: wenyilu@nyu.edu. Shangkun Xie: School of Applied Economics, Renmin University of China; email: shangkunecon@gamil.com.

Appendix A More on Background and Data

A.1 Reform Progress

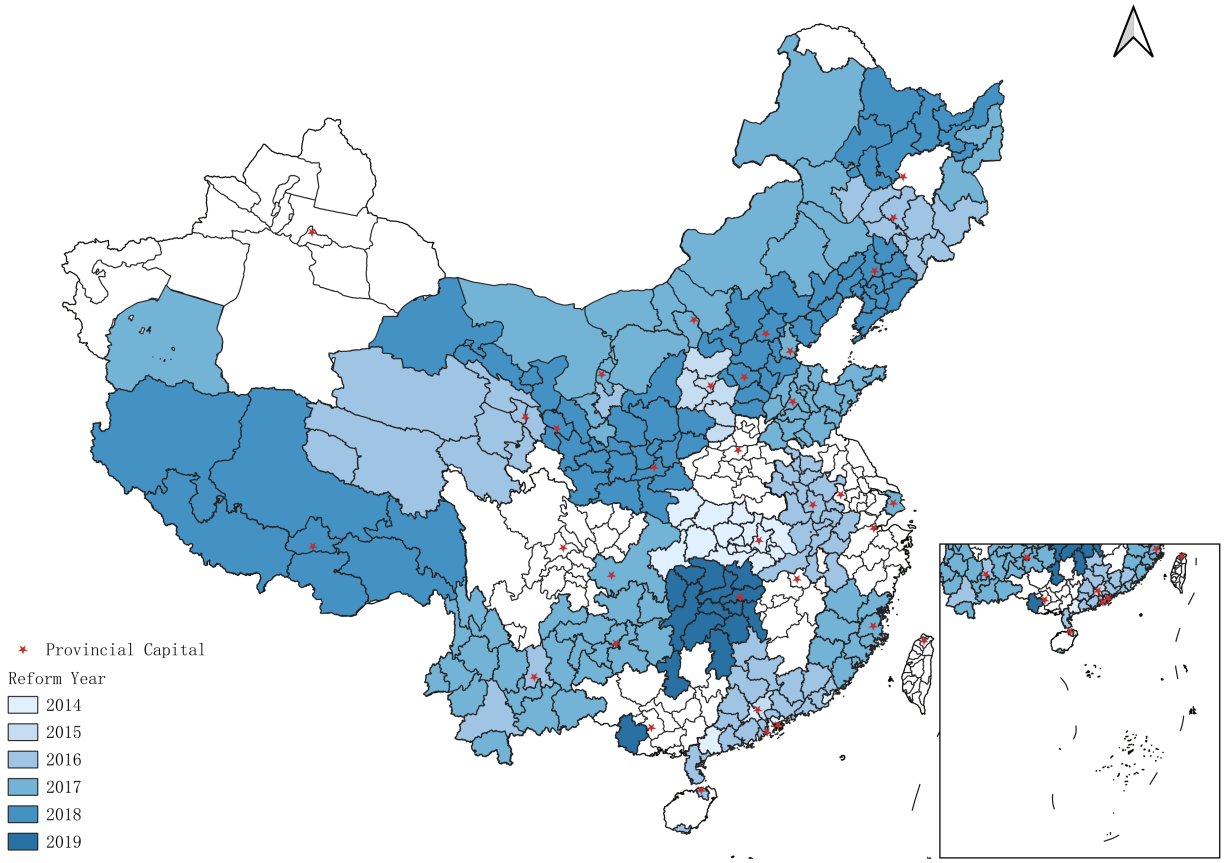


FIGURE A1 : Progress of Judicial Reform

Notes: Figure A1 illustrates the timeline of the judicial reform across different regions, with unmarked cities indicating those where the reform had not yet been implemented by 2019. In 2014, 152 courts were selected as pilot sites, and the reform gradually expanded to other regions. By 2021, over 3,500 local courts had adopted the reform.

A.2 Data Summary

TABLE A1 Summary Statistics: Investment Distribution (Firm-year Level)

	Mean	Std.Dev.	Min	Max	Observation
	(1)	(2)	(3)	(4)	(5)
Investment (binary, yes=1)					
Total	0.734	0.442	0	1	7,865
Intra-provincial, to Capital [a]	0.067	0.250	0	1	7,865
Intra-provincial, to Local	0.220	0.415	0	1	7,865
Inter-provincial, to Capital	0.509	0.500	0	1	7,865
Inter-provincial, to Local	0.475	0.499	0	1	7,865
Investment destination (number)					
Total	2.401	3.812	0	107	7,865
Intra-provincial, to Capital	0.067	0.250	0	1	7,865
Intra-provincial, to Local	1.053	1.882	0	26	7,865
Inter-provincial, to Capital	0.313	0.814	0	20	7,865
Inter-provincial, to Local	0.968	2.087	0	67	7,865
Investment amount (100 million)					
Total	12.464	591.377	0	48,012	7,865
Intra-provincial, to Capital	1.095	69.420	0	6,000	7,865
Intra-provincial, to Local	2.473	101.970	0	8,900	7,865
Inter-provincial, to Capital	7.538	545.377	0	48,003	7,865
Inter-provincial, to Local	1.359	5.931	0	202	7,865
Investment ratio (%) [b]					
Intra-provincial, to Capital (%)	3.5	15.1	0	100	5,770
Intra-provincial, to Local (%)	41.8	41.0	0	100	5,770
Inter-provincial, to Capital (%)	16.0	31.5	0	100	5,770
Inter-provincial, to Local (%)	38.8	40.0	0	100	5,770
Investment distance (km) [b]					
Average	873.819	510.234	20	3,979	5,770
Min	523.228	527.704	20	3,778	5,770
Max	1278.319	749.112	20	4,180	5,770

Notes: Table A1 reports descriptive statistics on firms' investment activities in cities other than their parent company's location city.

[a]: *Capital* refers to provincial capital cities, while *Local* refers to local prefectures.

[b]: Sample restricted to firms with investment outside their headquarters city.

TABLE A2 Summary Statistics

	Mean	Std.Dev.	Min	Max	Observation
	(1)	(2)	(3)	(4)	(5)
City-firm-year level					
Average PRI index	0.122	0.102	0	0.667	2,642,640
Reform	0.278	0.448	0	1	2,642,640
Reform exposure (destination weight) [a]	0.137	0.307	0	1	2,642,640
Reform exposure (amount weight) [b]	0.133	0.314	0	1	2,642,640
City level (2014)					
GDP per capita (logged)	10.684	0.496	9.304	12.241	337
GDP (10k, logged)	16.552	0.846	14.458	19.342	337
Enterprise number above designated size (logged)	6.615	1.031	3.178	10.631	337
Loan (10k, logged)	16.822	1.021	9.982	20.924	337
Wage per capita (logged)	10.869	0.215	8.509	11.649	337
Share of the tertiary (%)	40.988	8.172	24.170	79.650	337
Firm level (2014)					
Main operating revenue (logged)	20.888	1.123	16.799	25.654	1,066
ROA	0.043	0.065	-0.885	0.482	1,066
CEO-chairman Duality [c]	0.378	0.485	0	1	1,061
Fixed asset ratio	0.196	0.134	0	0.782	1,066
Asset growth ratio	0.421	1.222	-0.573	20.215	1,065

Notes: Table A2 reports descriptive statistics on our main variables.

[a] : *Reform exposure (destination weight)* captures the intensity of judicial reform faced by firms operating outside provincial capitals. It is constructed as defined in Equation 4, and is weighted by the share of a firm's investment locations affected by the reform relative to its total number of investment locations.

[b] : *Reform exposure (amount weight)* , also measures the intensity of judicial reform faced by firms operating outside provincial capitals, weighted by the share of total investment amounts allocated to affected locations.

[c] : a dummy variable equal to one if the firm's CEO and board chair are the same person.

A.3 Index Construction and Additional Datasets

Economic Policy Uncertainty (EPU) Index. Following Nie et al. (2020), we construct a firm-level EPU index based on textual analysis of the Management Discussion and Analysis (MD&A) section in annual reports. We extract the Chinese text, remove numbers and symbols, and segment it into sentences. A sentence is coded as reflecting policy uncertainty if it contains both policy-related terms (e.g., "policy," "government," "macroeconomic regulation") and uncertainty-related expressions (e.g., "uncertainty," "volatility," "risk"). The EPU index is then calculated as the proportion of such sentences within the MD&A. Keyword lists are available upon request.

Firm–Government Agency Exposure Index. We construct a firm-level index that captures the extent to which companies report interactions with government agencies in their annual reports. Following the approach of Armstrong et al. (2025), we extract and segment MD&A section of each report, and identify sentences that mention both (a) central or local government agencies (e.g., the Ministry of Foreign Affairs, the Supreme People's Court) and (b) governance-related action verbs (e.g., regulate, inspect, approve, enforce). The index is defined as the proportion of such sentences relative to the total number of sentences in the MD&A.

Government agencies include ministries, commissions, bureaus, and affiliated institutions under the State Council and other central authorities, covering key areas such as economic planning, law enforcement, environmental regulation, public health, and media oversight. Action verbs reflect common regulatory functions and include terms such as "authorization," "audit," "compliance," "ruling," and "inspection."

City-level Weighted Investigate (Corruption) index We draw on the *Investigated Officials Database*, a hand-collected dataset documenting all county-level and above officials investigated for corruption. This information—sourced from official government websites and cross-validated with social media—includes detailed profiles of each official's administrative rank and personal background, providing a measure of city-level corruption exposure.

Note that in China, only those investigated for violating party discipline or law are made public, so "investigation" here is equivalent to prosecution and legal punishment. We denote a city-level "investigated" if its main officials were investigated. The weighted average is calculated by averaging the rank of the investigated officials in the city. For city c,

$$\textit{Weighted Investigate} = \sum_{i \in I} i \times \frac{\#(\text{investigated officials with rank } i \text{ in city } c)}{\#(\text{investigated officials in city } c)}.$$

The rank i is the official administrative range of the Chinese government, ranging from 1 to 10 ($I = \{1, 2, \ldots, 10\}$), representing from vice-township level to country level. The ranks of city

officials primarily lie between 5 (vice-city level) and 7 (vice-province level).

Political Orientation Search index We get this city-year level index by frequency of online searches in each city using Baidu (most popular search engine in China) for politically relevant keywords, including "Communist Party" and "Party-building" two parts (In Chinses, 中国共产党, 党务工作, 党员先锋模范作用, 党员教育, 党建, 党建带团建, 党性, 党章, 党风廉政建设, 基层党建, 意识形态, 社会主义核心价值观, 纪律教育) . Next, we sum up and normalize the search volume of all keywords to get the city-year level Political Orientation Search index.

Additional Datasets. To support our empirical strategy and account for political, institutional, and contextual heterogeneity, we incorporate a series of additional datasets beyond firm-level financials.

First, to identify cities that may retain provincial-level political ties despite judicial reform, we construct an original dataset on *Provincial Standing Committee (SC) Membership*. We collect appointment records from local government websites, identifying prefectural officials who concurrently hold seats on the provincial SC—an institutional marker of enduring patronage ties that may insulate localities from reform impacts.[1]

Second, we use the *Revolving-Door Officials Database* from Li and Lu (2025), which contains rich demographic and career data on more than 15,000 government officials who have served on the boards of listed firms since the early 2000s. This includes salary, allowances, and equity holdings, allowing us to track post-bureaucratic mobility and institutional embeddedness.

Third, we include the *Fan Gang Marketization Index*, which measures provincial-level market development across five dimensions: government–enterprise transparency, legal system quality, private sector vitality, product market development, and factor market liberalization.

Fourth, we use the *Judicial Judgments Database for Listed Firms*, which links court rulings to specific publicly listed companies. The dataset includes detailed information on case types, legal outcomes, and parties involved, offering direct insights into firm-level judicial exposure.

Sixth, we supplement our firm- and city-level data with the *Chinese Social Survey (CSS)*, a nationally representative micro survey that includes individual attitudes toward judicial institutions.

Finally, we manually construct an *Officials Information Database*, compiling biographical characteristics (e.g., place of birth, gender, ethnicity, age, and education) for provincial governors and Party secretaries. This information supports heterogeneity analyses related to political leadership profiles.

1. In the Chinese political system, concurrent SC membership signals access to higher-level networks that are typically beyond the reach of judicial reform.

A.4 Identification Checks

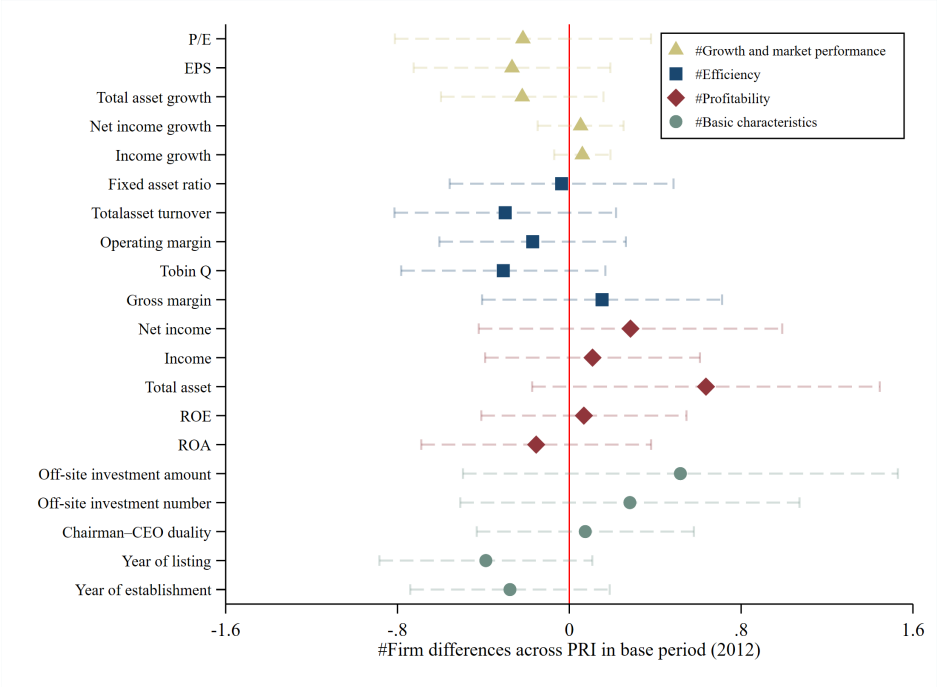
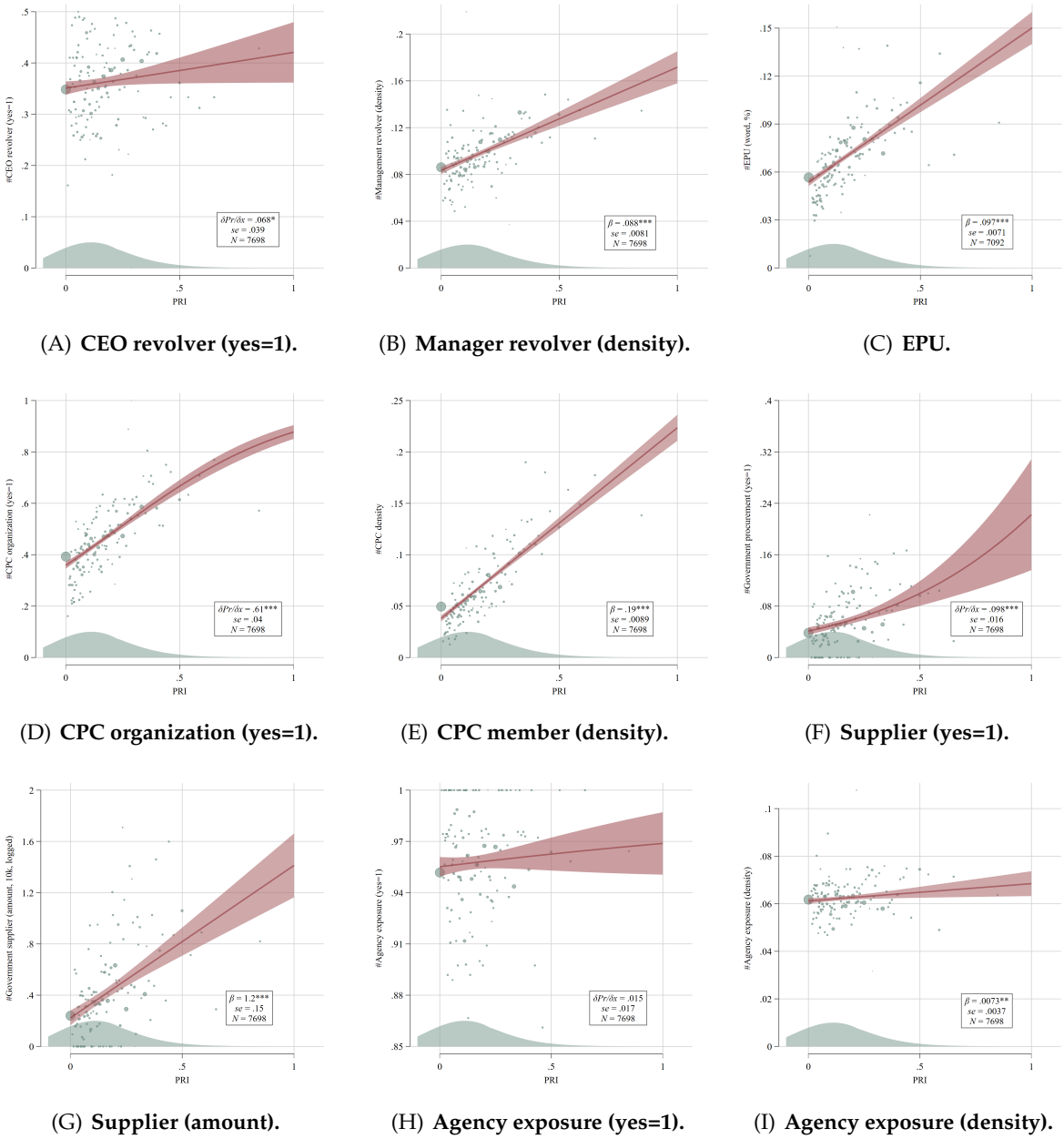


FIGURE A2 Correlations between the Political Reliance Index (PRI) and a Set of Pre-determined Firm Characteristics.

Notes: Figure A2 shows the correlations between the Political Reliance Index (PRI) and a set of pre-determined firm characteristics. Each point represents a separate regression in which the dependent variable is a standardized firm characteristic, and the independent variable is PRI. Dashed lines denote 95 % confidence intervals.



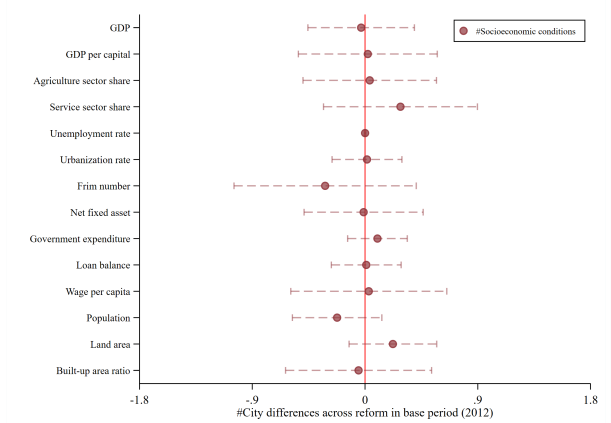
(A) **CEO revolver (yes=1).** (B) **Manager revolver (density).** (C) **EPU.**

(D) **CPC organization (yes=1).** (E) **CPC member (density).** (F) **Supplier (yes=1).**

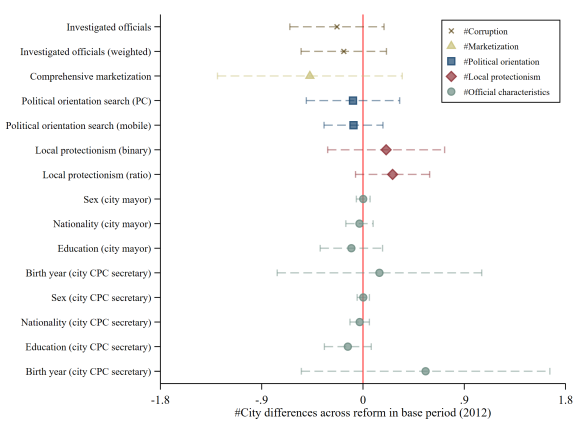
(G) **Supplier (amount).** (H) **Agency exposure (yes=1).** (I) **Agency exposure (density).**

FIGURE A3 PRI and Alternative Political Reliance Proxies.

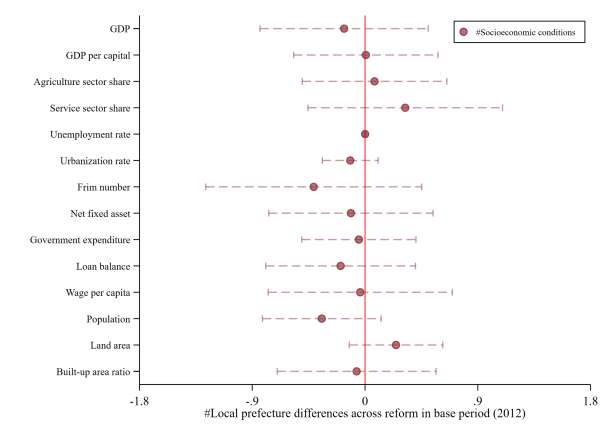
Notes: Figure A3 presents the relationship between our baseline measure of the Political Reliance Index (PRI) and several alternative proxies for firm-level political reliance. We construct six dimensions to validate the PRI: (1) whether the CEO is a former government official (Panel A) ; (2) the proportion of former government officials among the firm's senior management (Panel B); (3) firm-level economic policy uncertainty, proxied by the frequency of relevant words in annual reports (Panel C); (4) the existence of a Communist Party of China (CPC) organization within the firm and the proportion of party-affiliated managers (Panels D and E); (5) the firm's degree of participation in government procurement (supplier) (Panels F and G); and (6) firm-government agency exposure measured by the frequency of relevant terms in annual reports (Panels H and I). Shaded areas indicate 95 % confidence intervals.



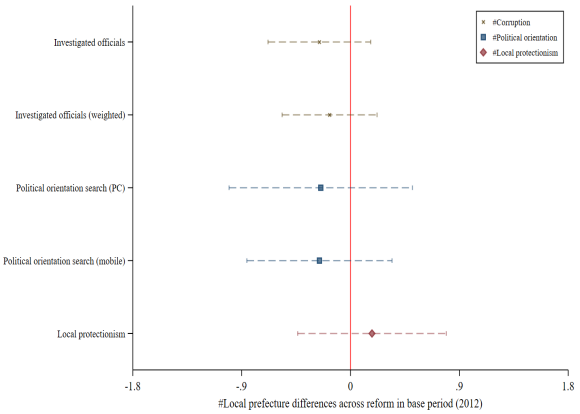
(A) **Socioeconomic conditions, both.**



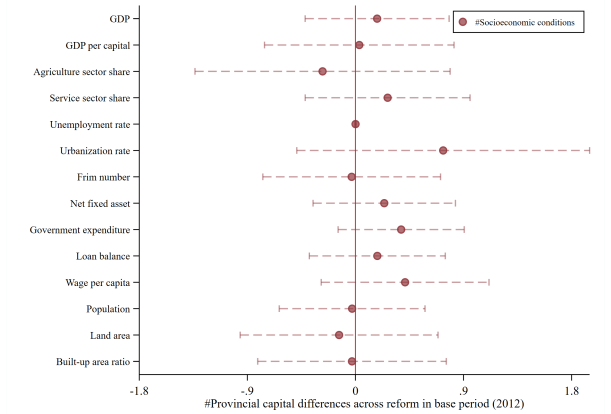
(B) **Political conditions, both.**



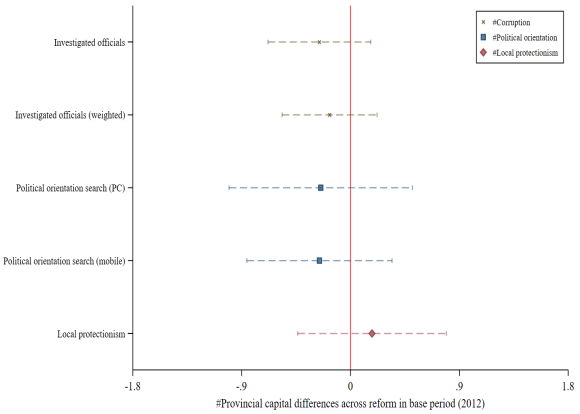
(C) **Socioeconomic conditions, local prefecture.**



(D) **Political conditions, local prefecture.**



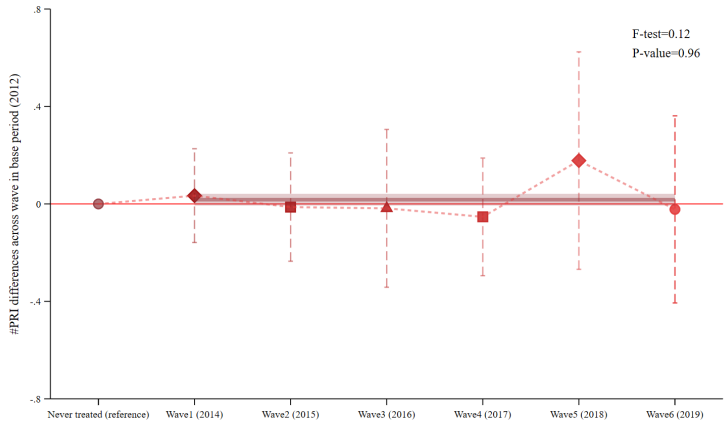
(E) **Socioeconomic conditions, provincial capital.**



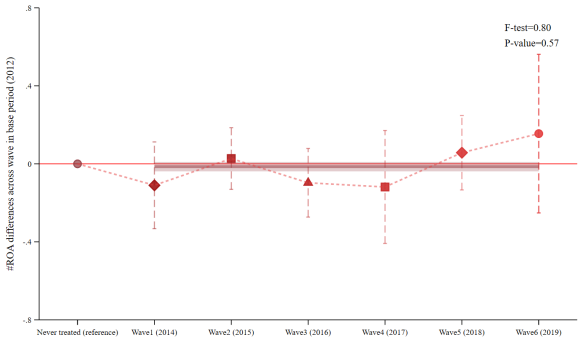
(F) **Political conditions, provincial capital.**

FIGURE A4 Differences between Treatment and Control Cities.

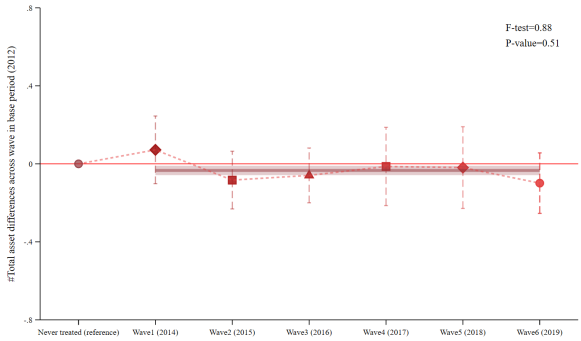
Notes: Figure A4 examines whether cities that implemented judicial reform in 2012 differ systematically from those that did not. Each dot represents a separate city-level regression point estimate. Panels A, B include both local prefecture and provincial capital. Panels C, D, E, F are separately for local prefectures and provincial capitals, respectively. Panels A, C, E report differences in 2012 economic characteristics, including total GDP, GDP per capita, industrial structure, unemployment rate, urbanization, government expenditure, wage levels, credit access, population size, land area, and built-up area. Panels B, D, F report differences in political characteristics, including local corruption, marketization, party-state political orientation, local protectionism, and the demographic profiles of mayors and party secretaries (gender, ethnicity, education, and birth year). Dashed lines indicate 95% confidence intervals.



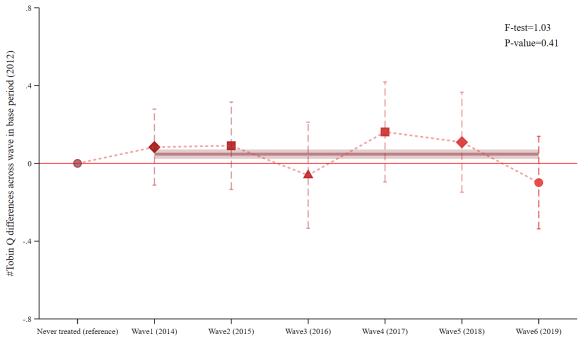
(A) **PRI.**



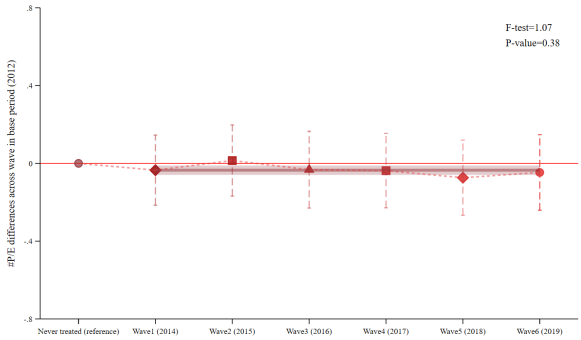
(B) **ROA.**



(C) **Total asset.**



(D) **Tobin Q.**



(E) **P/E.**

FIGURE A5 Correlations between the Policy Wave and a Set of Pre-determined Firm Characteristics.

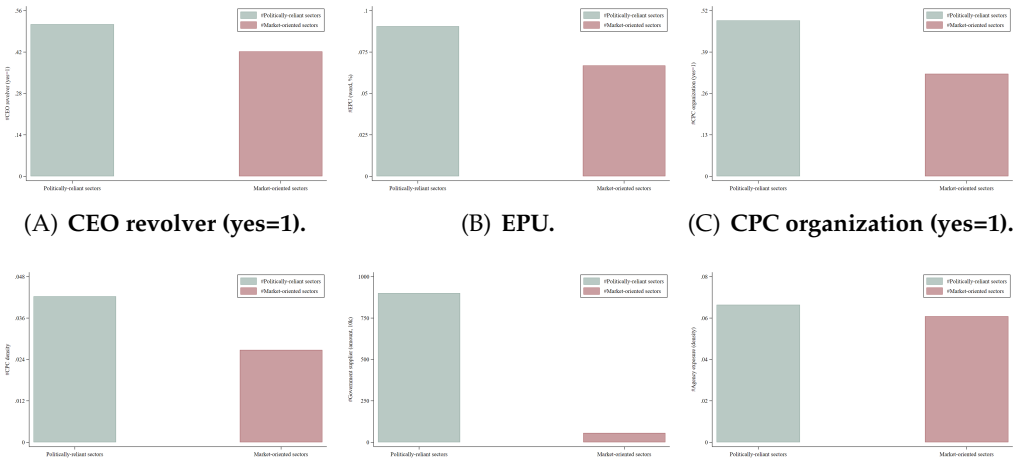
Notes: Figure A5 examines whether the judicial reform targets firms with specific characteristics. Each dot estimates the difference between firms in the never-treated group and firms treated in different years (policy waves). Panels A-E report differences in PRI, ROA, total asset, Tobin's Q-value, and P/E, respectively. Dashed lines indicate 95% confidence intervals.

Appendix B More on Baseline

TABLE B1 Falsification Tests: State-Owned Enterprise (SOE)

Dependent variable:	Investment (p.p.)			Investment (logged)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A (Equation 2). *Local prefecture*						
PRI × Reform	-0.0384	-0.0321	-0.0267	-0.0101	-0.0077	-0.0069
	(0.1523)	(0.1598)	(0.1612)	(0.0263)	(0.0275)	(0.0278)
Reform	-0.0117	-0.0016	-0.0027	-0.0025	-0.0009	-0.0011
	(0.0411)	(0.0429)	(0.0431)	(0.0072)	(0.0074)	(0.0074)
Dependent-variable mean	0.5210	0.5266	0.5266	0.0939	0.0949	0.0949
Observations	556,061	546,285	546,285	556,061	546,285	546,285
Panel B (Equation 3). *Provincial capital*						
PRI × Reform exposure	3.3514	3.4385	2.6040	0.4393	0.4463	0.3041
	(2.4470)	(2.6099)	(2.6779)	(0.4121)	(0.4384)	(0.4508)
Reform exposure	0.1511	0.6395	1.8069**	0.0226	0.0936	0.2630**
	(0.6525)	(0.6643)	(0.6795)	(0.1141)	(0.1165)	(0.1174)
Dependent-variable mean	3.5973	3.6030	3.6030	0.6362	0.6363	0.6363
Observations	55,459	54,483	54,483	55,459	54,483	54,483
Pre-policy city-level control × year		✓	✓		✓	✓
Pre-policy firm control × year		✓	✓		✓	✓
Base period (2012) investment × year			✓			✓
Firm-location pair FE	✓	✓	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓	✓	✓

Notes: Table B1 presents falsification test results using centrally state-owned enterprises (SOEs) sample. The unit of observation is at the firm-investment location-year level. In Columns (1)–(3), the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Columns (4)–(6), the dependent variable is the investment amount (logged). Panel A reports the results using Equation 2. The key independent variable, $Reform$, is a dummy variable that equals 1 if the city has implemented judicial reform and 0 otherwise. The variable PRI represents the firm's political reliance index. Panel B reports the results using Equation 3. $Reform_exposure$, measures the intensity of the judicial reform's impact on firms in local prefectures, as defined in Equation 4 and it is weighted by the proportion of investment locations affected by the reform relative to the total number of investment locations. Control variables are identical to those in Table 2. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

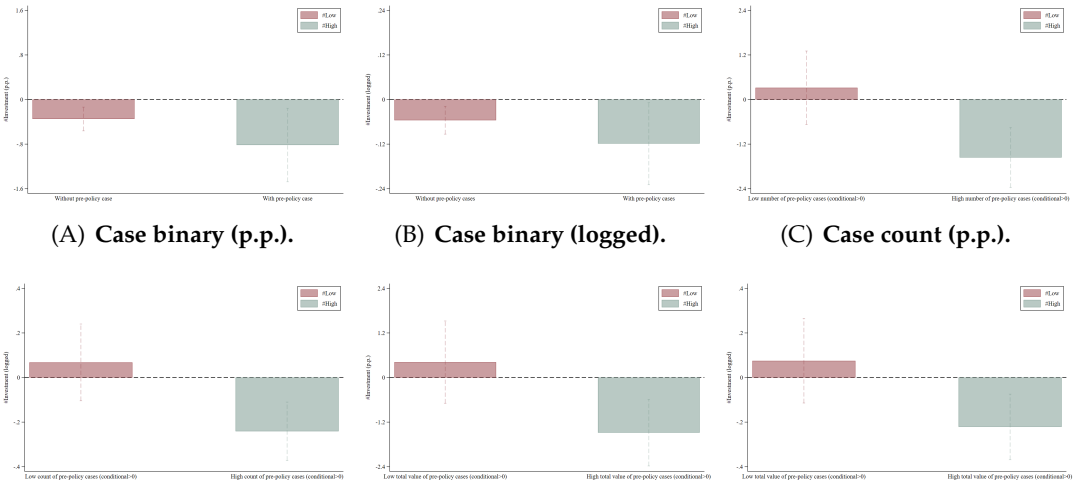


(A) **CEO revolver (yes=1).** (B) **EPU.** (C) **CPC organization (yes=1).**

(D) **CPC member (density).** (E) **Supplier (amount).** (F) **Agency exposure (density).**

FIGURE B1 Differences of Alternative PRI Proxies across Sectors.

Notes: Figure B1 illustrates how the industries represented by the green and red bars in Figure 4 differ across multiple indicators of political reliance. Green bars denote industries such as infrastructure, electricity, real estate, finance, and social-security services; red bars denote hotels, food services, retail, and residential services. Panels A–F show differences between the two groups of industries in (A) the share of CEOs with prior government service, (B) exposure to policy uncertainty, (C) establishment of CPC party organizations, (D) share of managers who are CPC members, (E) involvement in public procurement, and (F) the frequency of investor discussions linking business and politics.

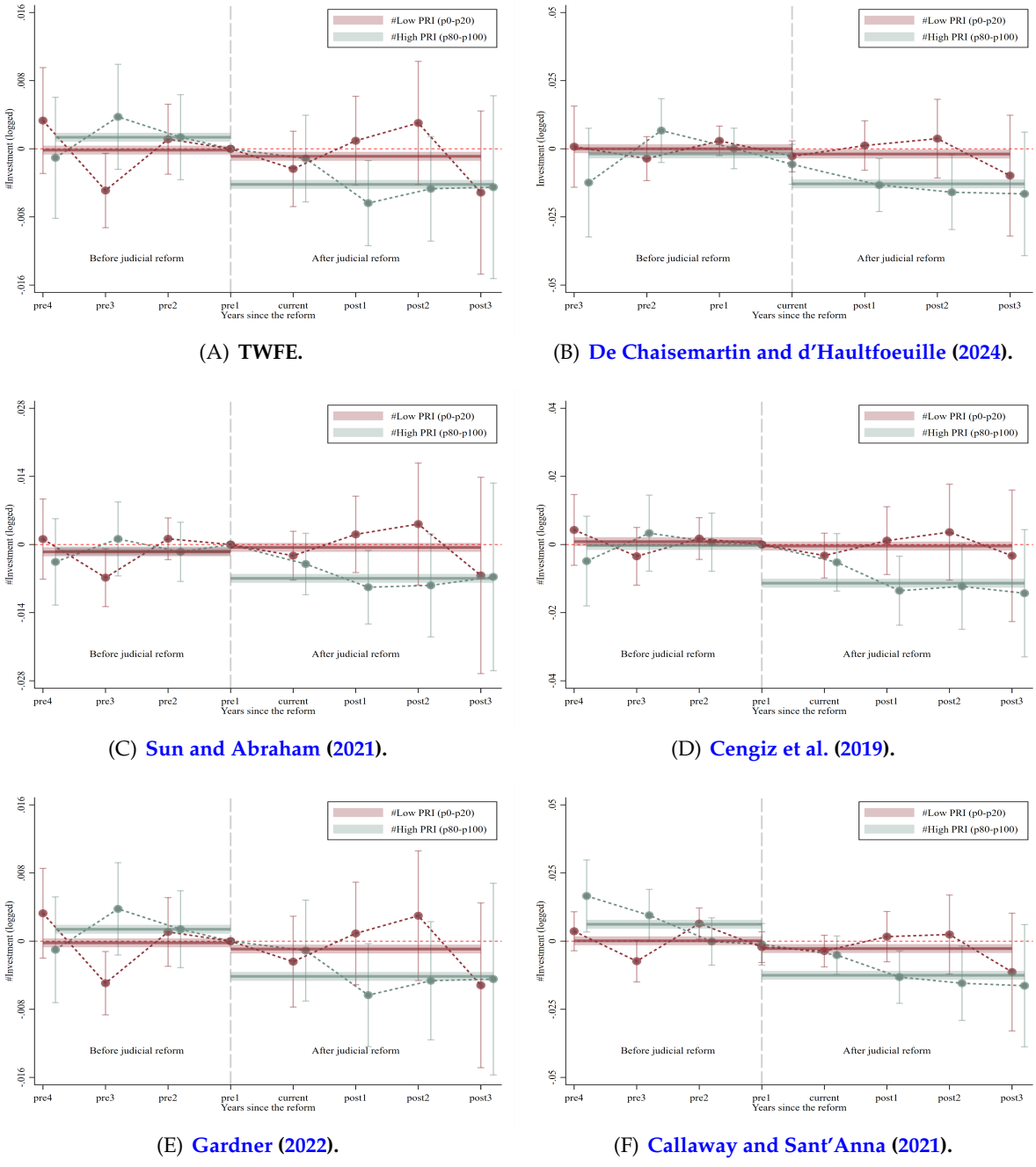


(A) **Case binary (p.p.).** (B) **Case binary (logged).** (C) **Case count (p.p.).**

(D) **Case count (logged).** (E) **Case value (p.p.).** (F) **Case value (logged).**

FIGURE B2 Effects of Judicial Reform across Predetermined Case Count and Value.

Notes: Figure B2 presents heterogeneous treatment effects by firms' exposure to judicial rulings. Panels A, C, and E use a binary outcome indicating whether a firm invested in the location (coefficients scaled by 100 for ease of interpretation). Panels B, D, and F use logged investment amounts. Green bars report subsample estimates of Equation 2 for firms with high litigation exposure, while red bars report those with low exposure. Specifically, Panels A and B distinguish firms based on whether they faced major legal disputes prior to the reform; Panels C and D classify firms above or below the median number of disputes; Panels E and F restrict the sample to firms with at least one major legal dispute and split them by whether the total value of cases is above or below the median. All specifications include the same controls and fixed effects as in Figure 5. Dashed lines denote 95% confidence intervals.



(A) **TWFE.**

(B) **De Chaisemartin and d'Haultfoeuille (2024).**

(C) **Sun and Abraham (2021).**

(D) **Cengiz et al. (2019).**

(E) **Gardner (2022).**

(F) **Callaway and Sant'Anna (2021).**

FIGURE B3 Effects of Judicial Reform on Investment by Relative Year (Investment Amount).

Notes: Figure B3 plots the dynamic investment responses of firms with high and low PRI relative to judicial reform implementation. Firms are divided into high-PRI and low-PRI groups, and we estimate dynamic treatment effects corresponding to dynamic version of Equation 1. The dependent variable is a the investment amount(logged), with regression coefficients multiplied by 100 for interpretability. Red lines depict the estimated effects for low-PRI firms, while green lines correspond to high-PRI firms. Vertical lines represent 95 % confidence intervals.

Appendix C More on Robustness Checks

In this section, we conduct a series of robustness checks on our baseline results.

Additional identification check First, we conduct additional identification checks. For the DID design, following Rambachan and Roth (2023), we examine the robustness of our main results to potential violations of the parallel trends assumption. Specifically, we construct a set Δ of permissible deviations from parallel trends and compute confidence intervals for the parameters of interest under these deviations. In line with the notation in Rambachan and Roth (2023), let δ denote the trend, $M \geq 0$ the maximum allowable change in its slope between consecutive periods, and $\Delta^{\text{SD}}(M)$ the corresponding set of deviations:

$$\Delta^{\text{SD}}(M) = \left\{\delta : \left|(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})\right| \leq M,\ \forall t\right\}.$$

When $M = 0$, the trend is exactly linear. Following Ge et al. (2025), we set M equal to half the standard error of the estimated coefficient and apply this test to each post-treatment period. Figure C1 presents the sensitivity-adjusted confidence intervals for the coefficient on $PRI_i \times Post_{mj}$ from Equation 5, allowing for deviations from parallel trends with M ranging from 0 to a more conservative upper bound. The red vertical dashed line marks the baseline value of $M = 0.5\times$ the standard error for each post-treatment period. In most cases, the coefficients of primary interest remain statistically significant, suggesting that the main findings are robust to moderate violations of the parallel trends assumption.

For the Bartik design, we test the sensitivity to the construction of the share component. Specifically, we exclude cities with large Rotemberg weights (top 1, top 3 & top 10, respectively following Goldsmith-Pinkham et al. (2020) (Figure C2), recalculate weights using alternative base-year data (Table C1), and conduct separate permutation tests on both the share and shift components (Panels A-H of Figure C3). Additionally, Panels I-P of Figure C3 randomly permute the timing of judicial reforms and firms' PRI. The figure displays the distribution of coefficients from 500 random permutations, with most coefficients being indistinguishable from zero and far away from the true effect.

Alternative Political Reliance Index We then assess the robustness of the Political Reliance Index (PRI). Table C2 explores four alternative definitions: (1) a binary indicator equal to 1 if the PRI is above the median and 0 otherwise; (2) the PRI measured in 2014 only; (3) a time-varying PRI index; and (4) the logarithm of the baseline PRI to mitigate the influence of extreme values. Across all specifications, the results remain consistent: high-PRI firms reduce investment more than low-PRI firms after the reform, and firms with higher PRI and greater reform exposure are more likely to shift investment to provincial capitals.

Additionally, Table C3 further measures the political sensitivity risk of firms using alternative data sources. We construct five indicators: (1) whether the firm's CEO is a former government official (*CEO revolver*); (2) the rank of government positions held by firm managers (rank of *CEO revolver*); (3) the proportion of former government officials among the firm's management (*Manager revolver (density)*) ; (4) the economic risk index of firms, measured using textual analysis of economic uncertainty in annual reports (*EPU*) ; (5) whether the firm has a party organization (which is believed to strengthen the party's control over firms and establish connections with the government, *CPC Organization*; and (6) the proportion of party members among firm managers (which is believed to reflect firms' reliance on government support, *CPC share*);(7) the value of government procurement contracts obtained by the firm, which reflects the extent to which its commercial activities depend on state provision and demand (*Procurement Dependence*). Consistent patterns are observed across all indicators used to measure firm PRI.

Exclude confounders In this part, we first estimate our baseline using different samples and include additional controls. Table C4 and Table C5 use two alternative samples or specifications. Columns (1) and (2) reintroduce the samples of firms' parent company locations and cities with provincial direct management that were excluded in our baseline. Columns (3) and (4) exclude firms that are not continuously present in the sample period (to ensure a balanced panel) and financial firms (whose investment logic differs from that of other real economy firms), respectively. Column (5) excludes cities that did not implement the reform during the sample period. Columns (6) and (7) include additional interactions between firm or investment location pre-reform characteristics and year dummies, respectively. Column (8) replaces interactions with predetermined controls and year dummies with predetermined control time linear trend. Column (9) applies winsorization to the dependent variable to address outliers with exceptionally high investment amounts. Moreover, Table D6 further controls for the potential influence of official characteristics. We manually collect detailed personal information on provincial governors and party secretaries, including gender, age, birthplace, education, and ethnicity. By controlling for these characteristics, we rule out the possibility that the traits of local officials simultaneously affect the implementation of the reform and the relationship between officials and firms. All of the results above remain robust.

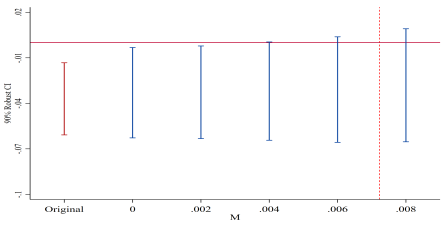
Apart from judicial centralization reform, we check the interference of other judicial reforms implemented during the same period. During this period, China introduced several key judicial reforms, including the lifelong accountability system for judges, the separation of adjudication and enforcement powers, and the establishment of circuit courts by the Supreme People's Court to hear major administrative and civil/commercial cases across administrative regions. These reforms were aimed at enhancing the rule of law and promoting judicial transparency. Additionally, to address environmental protection and pollution control, specialized environmental courts were established to handle cases related to environmental resources. While these reforms are likely to

increase judicial openness and promote market institutionalization, they do not involve centralization of political power and can hardly induce investment reallocation towards provincial capitals. Table D7 extends the baseline model by including a dummy variable indicating whether the investment location has undergone these reforms and its interaction with the PRI. The results show that neither the dummy variable nor its interaction term is statistically significant, suggesting other judicial reforms implemented during the same period do not affect our conclusions.

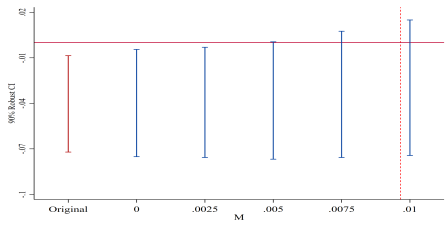
Since there is often a time lag between when a province receives instructions from Beijing and when the local party-state actually implements judicial reform, the timing of reform planning may differ from the timing of implementation. We conduct robustness checks using an alternative definition of the policy timeline, following the approach proposed by Lei and Li (2025). Under this alternative, we consider a Basic or Intermediate Court as having implemented the reform if either (a) its fiscal expenditures are listed under the provincial government's budget, or (b) the city where the court is located spends at least 50% less on courts than in the baseline year when the central government instructed the province to initiate the reform. Table D8 reports the results, which remain consistent with our main findings.

Addressing Spillover and Anticipatory Effects A potential concern is that firms in later-reform cities may anticipate policy shifts by observing outcomes in early-reform cities, thereby biasing our estimates. We confront this concern on theoretical and empirical grounds. First, anticipatory behavior would attenuate our estimates: if politically reliant firms exit before reform, the measured post-reform withdrawal understates the true effect, rendering our estimates a lower bound. Second, investment re-allocations are low-cost and reversible relative to relocation, giving firms little incentive to act on expectations alone; they can wait to observe whether local patronage networks actually erode.

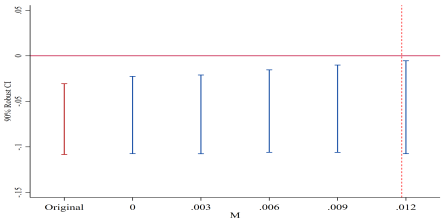
Empirically, we address anticipatory concerns in four complementary ways. First, in Figure C4, we restrict the control group to (i) never-treated cities, (ii) cities treated only in the first wave, and (iii) the union of first-wave and never-treated cities. Because these observations occur before any jurisdictional precedent exists, firms cannot have learned from earlier reforms, eliminating anticipatory behavior. Second, we apply the firm level and at the investment location tow-way clustering to address any firm-specific spillover correlation that might shift across locations within firm's different investment destination in Figure C4. Third, we add firm-year controls for the share of a firm's existing investment locations that have already undergone reform, proxying spillover intensity in Figure C4. Fourth, we re-estimate our main specifications using heteroskedasticity and autocorrelation consistent (HAC) spatial standard errors in Figure C5 following Conley (1999), obtaining the same results.



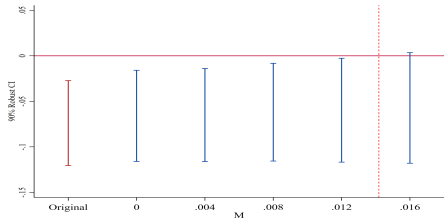
(A) **PRI** $\times Post_1$**.**



(B) **PRI** $\times Post_2$**.**



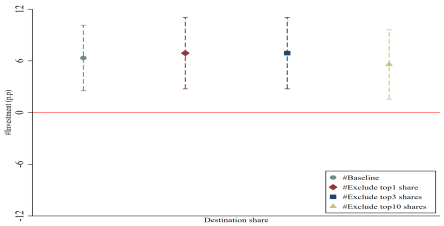
(C) **PRI** $\times Post_3$**.**



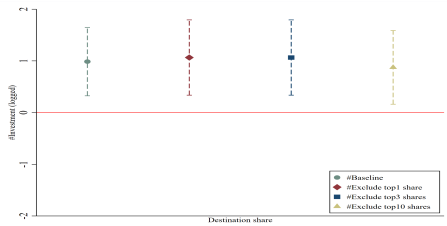
(D) **PRI** $\times Post_4$**.**

FIGURE C1 Sensitivity of CIs to Nonlinear Pre-trends following Rambachan and Roth (2023)

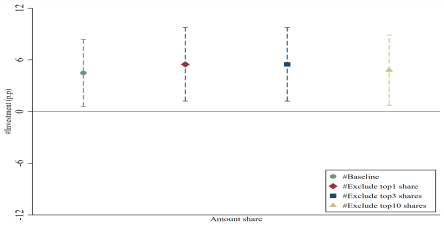
Notes: Figure C1 presents the sensitivity-adjusted 90% confidence intervals for the coefficient on $PRI_i \times Post_{mj}$ from Equation 5, based on the methodology of Rambachan and Roth (2023). Panels A–D report results for relative years 1 through 4 after the judicial reform. The confidence intervals are estimated for various values of M, ranging from 0 (implying exact linear pre-trends) to a more conservative upper bound. The red vertical dashed line marks the benchmark value of $M = 0.5\times$ the standard error, following Ge et al. (2025).



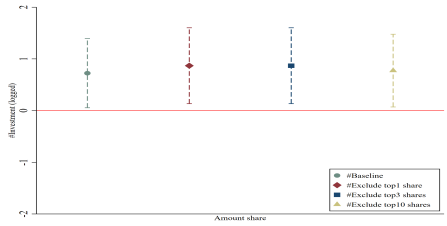
(A) **P.p., destination share.**



(B) **Logged, destination share.**



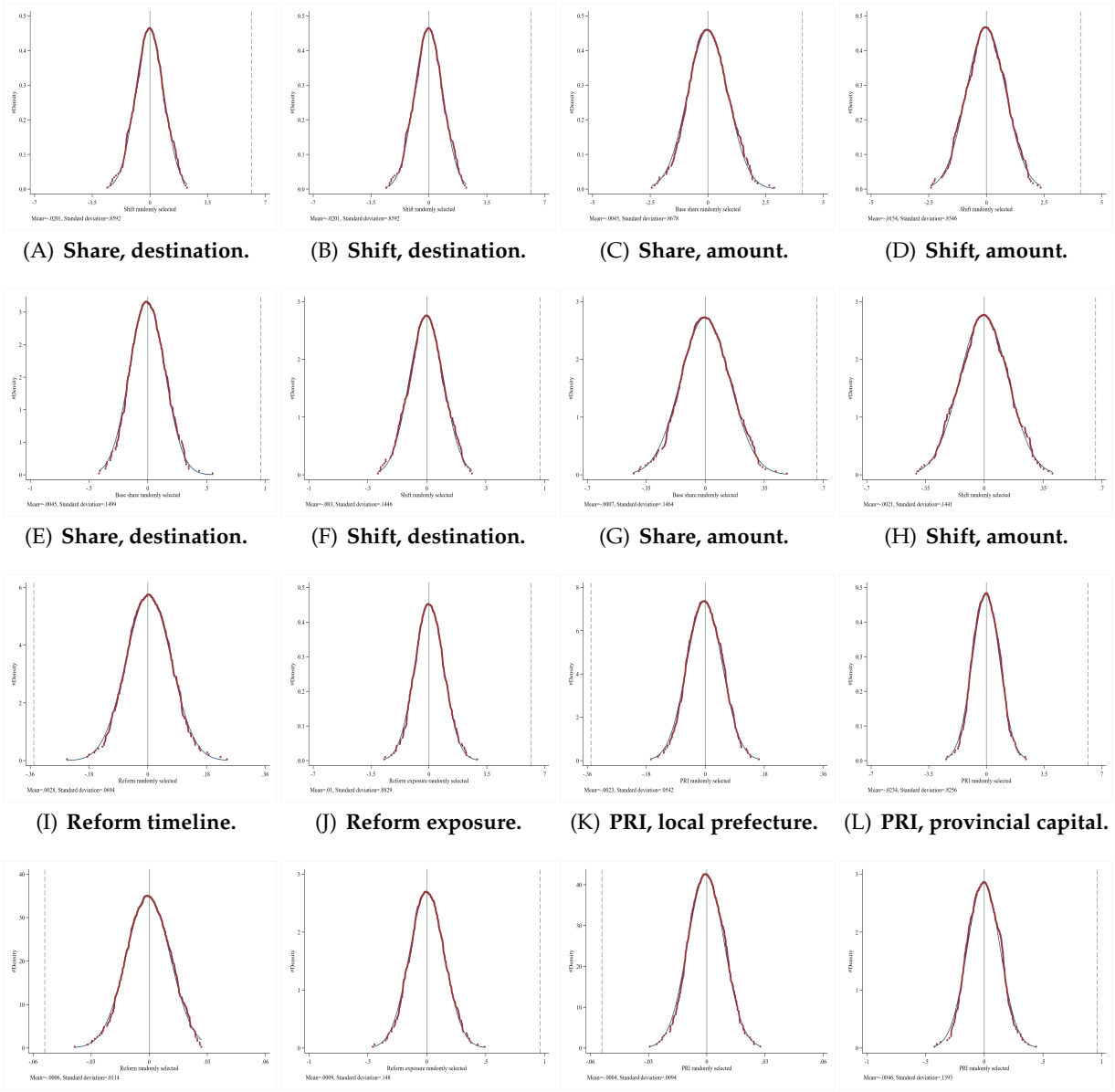
(C) **P.p., amount share.**



(D) **Logged, amount share.**

FIGURE C2 Excluding Highly Concentrated Investment Locations in Constructing Weights

Notes: Figure C2 reports robustness checks in which we re-estimate Equation 3 after excluding cities where firm-level investment is highly concentrated. Each dot represents a separate regression estimate. In Panels A and B, reform exposure is measured as the share of a firm's investment locations affected by the reform. In Panels C and D, exposure is measured as the share of investment amounts in affected locations. The dependent variable in Panels A and C is a binary indicator for whether the firm invested in the location; in Panels B and D, it is investment amount (logged). In each panel, we exclude the top 1, top 3, and top 10 most invested cities from the weight calculation. Dashed lines denote 95% confidence intervals.



(A) **Share, destination.** (B) **Shift, destination.** (C) **Share, amount.** (D) **Shift, amount.**

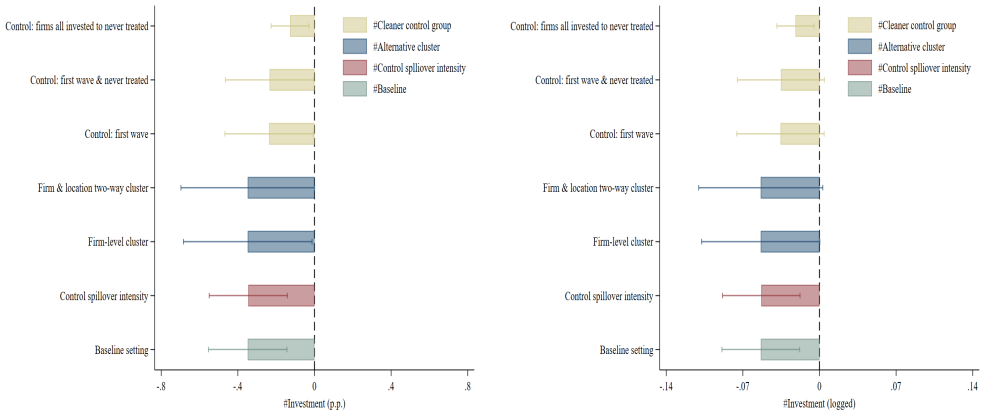
(E) **Share, destination.** (F) **Shift, destination.** (G) **Share, amount.** (H) **Shift, amount.**

(I) **Reform timeline.** (J) **Reform exposure.** (K) **PRI, local prefecture.** (L) **PRI, provincial capital.**

(M) **Reform timeline.** (N) **Reform exposure.** (O) **PRI, local prefecture.** (P) **PRI, provincial capital.**

FIGURE C3 Permutation-test Coefficients for "Pseudo Shift", "Pseudo Share", "Pseudo Reform (exposure)" and "Pseudo PRI"

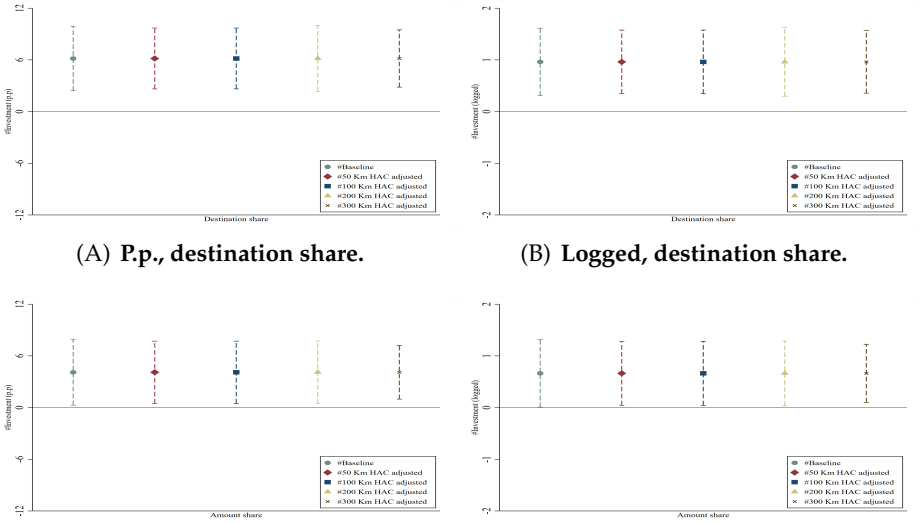
Notes: Figure C3 shows the distribution of estimated coefficients from a series of permutation tests. The dependent variable in Panels A to D and Panels I to L is a binary indicator for whether a firm invested in a given location; in Panels E to H and Panels M to P, it's the logarithm of investment amount. All coefficients are scaled by 100 for interpretability in Panels A to D and Panels I to L. Panels A to H show the permutation-test results of shift-share design (Equation 3) based on randomly assigning "shares" and "shifts". Panels I to P display the distribution of permutation-test coefficients for placebo reform (or reform exposure) and placebo PRI assignments. Specifically, in Panels A, C, E, G, we randomly assign investment shares in the construction of shift-share-style reform exposure while Panels B, D, F, H randomly assign policy timing (shift). "Destination" represent the reform exposure is constructed by investment destination share and "amount" represent it's constructed by investment amount share. In Panels I and M, we implement Equation 2 with randomly assigned reform timing; Panels J and N apply Equation 3 with randomized reform exposure. Panels K and O estimate Equation 2 with permuted PRI values, while Panels L and P randomize PRI assignments using Equation 3.



(A) **Investment (p.p.).** (B) **Investment (logged).**

FIGURE C4 Robust Estimators for Potential Network Spillover

Notes: Figure C4 reports robustness checks for potential network spillover. Each column represents a separate regression estimate. The dependent variable in Panel A is a binary indicator for whether the firm invested in the location; in Panel B, it is investment amount (logged). All the specifications are identical to Table 1. The teal columns show the results of baseline setting and the maroon ones add the proportion of the firm's treated investment destinations in year t to measure spillover intensity. Navy columns apply firm cluster and firm-city two-way cluster, respectively. Khaki column employ three cleaner control group with almost no spillover effect: first wave cities of each province, first wave cities of each province and never treated cities, and firms that all invested to the never treated cities during the policy. Dashed lines denote 95% confidence intervals.



(A) **P.p., destination share.** (B) **Logged, destination share.**

(C) **P.p., amount share.** (D) **Logged, amount share.**

FIGURE C5 Heteroscedasticity and Autocorrelation Consistent (HAC) spatial standard errors following Conley (1999)

Notes: Figure C5 reports robustness checks using heteroscedasticity and autocorrelation consistent spatial standard errors. Each dot represents a separate regression estimate. In Panels A and B, reform exposure is measured as the share of a firm's investment locations affected by the reform. In Panels C and D, exposure is measured as the share of investment amounts in affected locations. The dependent variable in Panels A and C is a binary indicator for whether the firm invested in the location; in Panels B and D, it is investment amount (logged). Dashed lines denote 95% confidence intervals.

TABLE C1 Robustness Check: "Share" Sensitivity Check

	2007 destination share		2010 destination share		2013 destination share	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. *Investment (p.p.)*						
PRI × Reform exposure	3.7848*	5.1112**	4.4115**	3.9141**	5.5188**	5.3813**
	(1.9122)	(2.0963)	(1.9890)	(1.8940)	(2.2582)	(2.2630)
Reform exposure	-1.7078***	-1.4802***	-1.8363***	-1.1130***	-2.5012***	-2.0014***
	(0.4805)	(0.4989)	(0.4376)	(0.3835)	(0.5623)	(0.5295)
Dependent-variable mean	3.6607	3.8392	3.6607	3.8392	3.6607	3.8392
Panel B. *Investment (logged)*						
PRI × Reform exposure	0.5740*	0.7726**	0.6632*	0.5845*	0.9003**	0.8805**
	(0.3233)	(0.3514)	(0.3443)	(0.3297)	(0.3852)	(0.3868)
Reform exposure	-0.2655***	-0.2273***	-0.2927***	-0.1770**	-0.4477***	-0.3697***
	(0.0794)	(0.0811)	(0.0748)	(0.0647)	(0.0989)	(0.0930)
Dependent-variable mean	0.6195	0.6507	0.6195	0.6507	0.6195	0.6507
Pre-policy city-level control × year	✓	✓	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓	✓	✓
Base period investment × year		✓		✓		✓
Firm-location pair FE	✓	✓	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓	✓	✓
Observations	238,670	214,786	238,670	214,786	238,670	214,786

Notes: Table C1 reports robustness checks using alternative base years to compute investment shares in the construction of reform exposure. Columns (1)–(2) use 2007 as the base year, Columns (3)–(4) use 2010, and Columns (5)–(6) use 2013 when calculating weights in Equation 4. $Reform_exposure$ is weighted by the number of investment destinations. In Panel A, the dependent variable is a binary indicator for whether the firm invested in the location; coefficients are multiplied by 100 for interpretability. In Panel B, the dependent variable is the logarithm of investment amount. Columns (1), (3) and (5) use basic FEs and city-level, firm-level predetermined control. Columns (2), (4) and (6) additionally control interaction between base period (2012) total investment amount to local prefecture (logged) and year dummies. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE C2 Robustness Check: Alternative PRI Transformations

Dependent variable:	$PRI_{>Median}$	$PRI_{t=2014}$	PRI_{it}	$Ln(PRI_i+1)$
	(1)	(2)	(3)	(4)
Panel A. *Local prefecture, investment (p.p.)*				
PRI × Reform	-0.0676***	-0.3294***	-0.1497*	-0.4222***
	(0.0233)	(0.1091)	(0.0783)	(0.1441)
Dependent-variable mean	0.4085	0.4162	0.4085	0.4085
Observations	2,386,162	2,019,431	2,386,162	2,386,162
Panel B. *Local prefecture, investment (logged)*				
PRI × Reform	-0.0100**	-0.0501***	-0.0235*	-0.0650***
	(0.0040)	(0.0189)	(0.0134)	(0.0251)
Dependent-variable mean	0.0711	0.0722	0.0711	0.0711
Panel C. *Provincial capital, investment (p.p.)*				
PRI × Reform exposure	0.9533***	9.3058***	3.2462**	7.4678***
	(0.3097)	(2.0381)	(1.4141)	(2.1758)
Dependent-variable mean	3.6607	3.6633	3.6607	3.6607
Observations	238,670	201,865	238,670	238,670
Panel D. *Provincial capital, investment (logged)*				
PRI × Reform exposure	0.1417**	1.4959***	0.5347**	1.1645***
	(0.0546)	(0.3381)	(0.2392)	(0.3788)
Dependent-variable mean	0.6195	0.6195	0.6195	0.6195
Pre-policy city-level control × year	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓

Notes: Table C2 presents robustness results for alternative functional transformations of the firm's political reliance index (PRI). The unit of observation is at the firm-investment location-year level. In Panels A and C, the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Panels B and D, the dependent variable is the investment amount(logged). Panels A and B report results using Equation 2. The key independent variable, $Reform$, is a dummy variable that equals 1 if the city has implemented judicial reform and 0 otherwise. Panels C and D report results using Equation Equation 3. $Reform_exposure$ measures the intensity of the judicial reform's impact on firms in local prefectures, as defined in Equation 4. The variable PRI represents the firm's political reliance index. In Column (1), PRI is redefined as a dummy variable indicating whether the firm's PRI is above the median. In Column (2), PRI is measured using 2014 data. In Column (3), PRI is time-varying. In Column (4), the logarithm of PRI replaces the baseline PRI definition. Control variables are identical to those in Table Table 1. Standard errors are clustered at the city level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE C3 Robustness Check: Alternative PRI Measurement Proxies

	CEO revolver (yes=1)	Rank of CEO revolver	Manager revolver (density)	EPU	CPC organization (yes=1)	CPC share	Procurement dependence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. *Local prefecture, investment (p.p.)*							
PRI Index × Reform	-0.0232**	-0.0194*	-0.0284***	-0.0219**	-0.0065	-0.0085	-0.0223**
	(0.0096)	(0.0099)	(0.0108)	(0.0092)	(0.0101)	(0.0106)	(0.0095)
Dependent-variable mean	0.4085	0.4085	0.4213	0.4085	0.4085	0.4085	0.4085
Observations	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162
Panel B. *Local prefecture, investment (logged)*							
PRI Index × Reform	-0.0035**	-0.0032*	-0.0043**	-0.0033**	-0.0011	-0.0015	-0.0035*
	(0.0016)	(0.0017)	(0.0018)	(0.0016)	(0.0018)	(0.0019)	(0.0018)
Dependent-variable mean	0.0711	0.0711	0.0732	0.0711	0.0711	0.0711	0.0711
Panel C. *Provincial capital, investment (p.p.)*							
PRI Index × Reform exposure	0.4050***	0.3298**	0.1907	0.7954***	0.4969***	0.5636***	0.6311**
	(0.1404)	(0.1365)	(0.1235)	(0.1610)	(0.1306)	(0.1124)	(0.3016)
Dependent-variable mean	3.6607	3.6607	3.7327	3.6607	3.6607	3.6607	3.6607
Observations	238,670	238,670	213,651	238,670	238,670	238,670	238,670
Panel D. *Provincial capital, investment (logged)*							
PRI Index × Reform exposure	0.0736***	0.0598**	0.0192	0.1306***	0.0747***	0.0882***	0.0654**
	(0.0223)	(0.0220)	(0.0197)	(0.0265)	(0.0214)	(0.0184)	(0.0315)
Dependent-variable mean	0.6195	0.6195	0.6308	0.6195	0.6195	0.6195	0.6195
Pre-policy city-level control × year	✓	✓	✓	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓	✓	✓	✓

Notes: Table C3 presents robustness results using alternative datasets to measure firm's political reliance. The unit of observation is at the firm-investment location-year level. In Panels A and C, the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Panels B and D, the dependent variable is the investment amount (logged). Panels A and B report results using Equation 2, while Panels C and D report results using Equation 3. The variable *PRI Index* represents the firm's political reliance index, measured using different definitions: Column (1) uses *Revolver CEO*, a dummy variable indicating whether the firm's CEO is a former government official; Column (2) uses the rank of government positions held by firm managers (*Revolver CEO Rank*); Column (3) uses the proportion of former government officials among the firm's management; Column (4) uses the economic risk index of firms; Column (5) uses a dummy variable indicating whether the firm has a party organization; Column (6) uses the proportion of party members among firm managers (*Party Exposure*); and Column (7) uses the firm's degree of participation in government procurement. Control variables are identical to those in Table 1. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE C4 Robustness Check: Restriction and Control (Local Prefecture)

	Include local investment	Include DACs	Use balanced panel	Exclude financial firms	Exclude never-treated	More firm control	More prefecture control	Use covariates linear trend	Exclude investment outliers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. *Investment (p.p.)*									
PRI × Reform	-0.3341***	-0.3492***	-0.3487***	-0.3835***	-0.3792***	-0.4107***	-0.3714**	-0.3911***	NA
	(0.1238)	(0.1213)	(0.1228)	(0.1465)	(0.1248)	(0.1251)	(0.1465)	(0.1242)	NA
Reform	0.0253	0.0252	0.0237	0.0325	0.0364*	0.0243	0.0369	0.0337*	NA
	(0.0187)	(0.0183)	(0.0190)	(0.0219)	(0.0190)	(0.0207)	(0.0224)	(0.0195)	NA
Dependent-variable mean	0.4289	0.3747	0.4085	0.4331	0.3686	0.4086	0.4674	0.4085	NA
Panel B. *Investment (logged)*									
PRI × Reform	-0.0512**	-0.0543**	-0.0537**	-0.0600**	-0.0593***	-0.0637***	-0.0585**	-0.0620***	-0.0538**
	(0.0216)	(0.0211)	(0.0213)	(0.0254)	(0.0217)	(0.0216)	(0.0256)	(0.0219)	(0.0213)
Reform	0.0032	0.0036	0.0029	0.0044	0.0052	0.0028	0.0056	0.0046	0.0030
	(0.0033)	(0.0032)	(0.0034)	(0.0038)	(0.0033)	(0.0037)	(0.0039)	(0.0034)	(0.0034)
Dependent-variable mean	0.0748	0.0652	0.0711	0.0754	0.0639	0.0711	0.0814	0.0711	0.0711
Controls	✓	✓	✓	✓	✓	✓	✓	✓	
Fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	
Observations	2,390,472	2,620,522	2,385,856	2,146,622	1,669,098	2,237,069	1,917,750	2,386,162	2,386,162

Notes: Table C4 presents robustness results using subsamples or additional controls, based on Equation 2. In Panel A, the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Panel B, the dependent variable is the investment amount (logged). The variable PRI represents the firm's political reliance index. Columns (1) and (2) reintroduce the samples of firms' parent company locations and cities with provincial direct management (DACs). Columns (3) and (4) exclude firms that are not continuously present in the sample period to ensure a balanced panel and financial firms, respectively. Column (5) excludes cities that did not implement the reform during the sample period. Columns (6) and (7) include additional interactions between firm (shareholding ratio of the largest shareholder, Tobin's Q, total assets (logged), debt-to-asset ratio, and asset turnover rate) or investment location (population (logged), unemployment rate, the proportion of the secondary industry, urban developed area, and population density) pre-reform characteristics and year dummies, respectively. Column (8) replaces interactions with predetermined controls and year dummies with predetermined control time linear trend. Column (9) applies tail-shortening on the 1% of the samples with non-zero investment amounts. Control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE C5 Robustness Check: Restriction and Control (Provincial Capital)

	Include local investment	Include DACs	Use balanced panel	Exclude financial firms	Exclude the never-treated	More firm control	More city-level control	Use covariates linear trend	Exclude investment outliers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. *Investment (p.p.)*									
PRI × Reform exposure	6.2726***	6.1613***	6.1525***	5.7679***	6.7194***	6.8635***	7.1022***	6.3681***	NA
	(2.0123)	(1.8355)	(1.8337)	(1.7944)	(1.8135)	(1.9361)	(2.2802)	(1.8743)	NA
Reform exposure	-1.8552***	-1.8797***	-1.8792***	-1.7890***	-2.0672***	-1.9524***	-2.3329***	-2.3671***	NA
	(0.4352)	(0.4373)	(0.4372)	(0.4280)	(0.5029)	(0.4307)	(0.5279)	(0.4660)	NA
Dependent-variable mean	3.7700	3.6607	3.6612	3.8392	3.7662	3.6569	3.5818	3.6607	NA
Panel B. *Investment (logged)*									
PRI × Reform exposure	0.9928***	0.9612***	0.9596***	0.8994***	1.0388***	1.0509***	1.0665**	0.9911***	0.9612***
	(0.3526)	(0.3201)	(0.3199)	(0.3143)	(0.3062)	(0.3421)	(0.3964)	(0.3264)	(0.3200)
Reform exposure	-0.2977***	-0.2990***	-0.2990***	-0.2832***	-0.3320***	-0.3062***	-0.3680***	-0.3778***	-0.2994***
	(0.0744)	(0.0744)	(0.0744)	(0.0724)	(0.0841)	(0.0735)	(0.0902)	(0.0794)	(0.0745)
Dependent-variable mean	0.6410	0.6195	0.6196	0.6507	0.6368	0.6189	0.6065	0.6195	0.6193
Controls	✓	✓	✓	✓	✓	✓	✓	✓	
Fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	
Observations	242,172	238,670	234,330	210,840	177,103	223,795	177,147	238,670	238,670

Notes: Table C5 presents robustness results using subsamples or additional controls, based on Equation 3. In Panel A, the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability, while in Panel B, is the investment amount (logged). $Reform_exposure$ measures the intensity of the judicial reform's impact on firms in local prefectures, as defined in Equation 4. The variable PRI represents the firm's political reliance index. Columns (1) and (2) reintroduce the samples of firms' parent company locations and cities with provincial direct management (DACs). Columns (3) and (4) exclude firms that are not continuously present in the sample period to ensure a balanced panel and financial firms, respectively. Column (5) excludes cities that did not implement the reform during the sample period. Columns (6) and (7) include additional interactions between firm or investment location pre-reform characteristics and year dummies, respectively. Column (8) replaces interactions with predetermined controls and year dummies with predetermined control time linear trend. Column (9) applies tail-shortening on the 1% of the samples with non-zero investment amounts. Other Control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D6 Robustness Check: Control for Official Characteristics

Dependent variable:	Investment (p.p.)			Investment (logged)		
	Provincial secretary	Provincial governor	Both	Provincial secretary	Provincial governor	Both
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. *Local prefecture*						
PRI × Reform	-0.3523***	-0.2820**	-0.3568***	-0.0479**	-0.0488**	-0.0555**
	(0.1233)	(0.1242)	(0.1243)	(0.0218)	(0.0217)	(0.0216)
Reform	0.0181	0.0113	-0.0176	-0.0001	0.0001	-0.0045
	(0.0249)	(0.0310)	(0.0351)	(0.0048)	(0.0051)	(0.0066)
Dependent-variable mean	0.4037	0.4133	0.4082	0.0702	0.0719	0.0710
Observations	2,354,642	2,230,838	2,199,318	2,354,642	2,230,838	2,199,318
Panel B. *Provincial capital*						
PRI × Reform exposure	6.2440***	6.3481***	6.4556***	0.9636***	0.9603***	0.9771***
	(1.8638)	(1.7584)	(1.7740)	(0.3252)	(0.3173)	(0.3204)
Reform exposure	-1.8943***	-1.8285***	-1.8517***	-0.2994***	-0.2850***	-0.2886***
	(0.4449)	(0.4273)	(0.4328)	(0.0751)	(0.0723)	(0.0730)
Dependent-variable mean	3.5284	3.4539	3.4564	0.5973	0.5852	0.5857
Observations	233,960	222,590	219,650	233,960	222,590	219,650
Pre-policy city-level control × year	✓	✓	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓	✓	✓

Notes: Table D6 presents robustness results with additional controls for provincial official characteristics. The unit of observation is at the firm-investment location-year level. In Columns (1)–(3), the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Columns (4)–(6), the dependent variable is the investment amount (logged). Panel A reports results using Equation 2, while Panel B reports results using Equation 3. The variable PRI represents the firm's political reliance index. Columns (1) and (3) control for the characteristics of the provincial party secretary, Columns (2) and (5) control for the characteristics of the provincial governor, and Columns (3) and (6) control for both. Official characteristics include gender, age, birthplace, education, and ethnicity. Control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D7 Robustness Check: Control for Other Judicial Reforms

Dependent variable:	Investment (p.p.)				Investment (logged)			
Other judicial reform:	Accountability reform	Separation reform	Circuit court	Environmental court	Accountability reform	Separation reform	Circuit court	Environmental court
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. *Local prefecture*								
PRI × Reform	-0.3084**	-0.3585***	-0.2865**	-0.3410***	-0.0487**	-0.0559**	-0.0445**	-0.0528**
	(0.1336)	(0.1330)	(0.1249)	(0.1205)	(0.0231)	(0.0230)	(0.0216)	(0.0210)
PRI × Other reform	0.1612	-0.0650	-0.1790	-0.1201	0.0203	-0.0150	-0.0268	-0.0154
	(0.1745)	(0.1968)	(0.1410)	(0.2353)	(0.0296)	(0.0320)	(0.0236)	(0.0401)
Dependent-variable mean	0.4085	0.4085	0.4085	0.4085	0.0711	0.0711	0.0711	0.0711
Observations	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162
Panel B. *Provincial capital*								
PRI × Reform exposure	5.7724***	5.7050***	4.4807**	5.2556***	0.9047***	0.9059**	0.7177**	0.8310***
	(1.7848)	(1.9044)	(1.8705)	(1.6761)	(0.3198)	(0.3371)	(0.3283)	(0.2935)
PRI × Other reform exposure	-0.9227	-0.9059	2.1767**	2.6346	-0.1340	-0.1098	0.3153**	0.3786
	(1.0422)	(0.9827)	(0.8208)	(1.6681)	(0.1710)	(0.1692)	(0.1411)	(0.2815)
Dependent-variable mean	3.6607	3.6607	3.6607	3.6607	0.6195	0.6195	0.6195	0.6195
Observations	238,670	238,670	238,670	238,670	238,670	238,670	238,670	238,670
Pre-policy city-level control × year	✓	✓	✓	✓	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Table D7 presents robustness results with additional controls for other judicial reform. The unit of observation is at the firm-investment location-year level. In Columns (1)–(4), the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Columns (5)–(8), the dependent variable is the investment amount (logged). Panel A reports results using Equation 2, while Panel B reports results using Equation 3. The variable PRI represents the firm's political reliance index. Columns (1)–(8) all include additional various judicial reforms dummy and their interactions with PRI. Specifically: Columns (1) and (5) control for the reform of the lifelong accountability system for judges. Columns (2) and (6) control for the separation of adjudication and enforcement powers. Columns (3) and (7) control for the establishment of circuit courts by the Supreme People's Court. Columns (4) and (8) control for the establishment of specialized environmental courts. Control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D8 Alternative Definition of Policy Timeline

Dependent variable:	Local prefecture		Provincial capital	
	Investment (p.p.)	Investment (logged)	Investment (p.p.)	Investment (logged)
	(1)	(2)	(3)	(4)
PRI × Reform	-0.3748***	-0.0575***		
	(0.1264)	(0.0218)		
Reform	0.0147	0.0002		
	(0.0384)	(0.0072)		
PRI × Reform exposure			6.1612***	0.9612***
			(1.8356)	(0.3202)
Reform exposure			-1.8796***	-0.2990***
			(0.4373)	(0.0744)
Pre-policy city-level control × year	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓
Dependent-variable mean	0.4085	0.0711	3.6607	0.6195
Observations	2,386,162	2,386,162	238,670	238,670

Notes: Table D8 reports robustness checks using an alternative definition of the policy timeline, as proposed by Lei and Li (2025). Columns (1) and (2) re-estimate Equation 2, while Columns (3) and (4) re-estimate Equation 3, using this alternative definition. The dependent variable in Columns (1) and (3) is a binary indicator for whether a firm invests in a given location, scaled by 100 for ease of interpretation. Columns (2) and (4) report results for the investment amount (logged). The main independent variable, $Reform$, is a binary indicator equal to one if judicial reform had been implemented in the city. $Reform_exposure$ measures the intensity of the judicial reform's impact on firms in local prefectures, as defined in Equation 4. The variable PRI denotes the firm's Political Reliance Index. Control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Appendix D More on Mechanism

TABLE D1 Mechanism: Satisfaction and Fairness

Dependent variable:	Unweighted				Weighted by 2014 population			
	Satisfaction		Fairness		Satisfaction		Fairness	
local prefectures	Yes	No	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. *Judiciary*								
Reform	0.1237***	0.0800	0.0808*	0.0572	0.1093**	0.0540	0.0875**	0.0526
	(0.0468)	(0.0733)	(0.0471)	(0.0881)	(0.0478)	(0.0660)	(0.0421)	(0.0724)
Dependent-variable mean	2.9751	2.9628	2.7860	2.7783	2.9714	2.9628	2.7842	2.7783
Panel B. *Company (placebo)*								
Reform	-0.0052	0.0196	0.0166	0.0075	-0.0077	0.0071	0.0225	0.0167
	(0.0552)	(0.0772)	(0.0467)	(0.0740)	(0.0562)	(0.0755)	(0.0424)	(0.0548)
Dependent-variable mean	2.9743	2.9870	2.5892	2.6001	2.9737	2.9870	2.5897	2.6001
Pre-policy city-level control × year	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
City FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	324	72	324	72	312	72	312	72

Notes: Table D1 reports the impact of judicial reform on public perceptions of the judiciary. The unit of observation is aggregated at the city-year level, using data from the Chinese Social Survey (CSS) from 2011 to 2021. Panel A examines the effect on public satisfaction and perceived fairness of the judiciary. Columns (1)–(2) and (5)-(6) use public satisfaction with the judiciary as the dependent variable, while Columns (3)–(4) and (7)-(8) use perceived fairness of the judiciary, where higher values indicate greater fairness. Panel B presents placebo tests, with Columns (1)–(2) and (5)-(6) using public satisfaction with their own company and Columns (3)–(4) and (7)-(8) using perceived fairness of promotion within the company as dependent variables. Columns (1)–(4) report unweighted regressions. Columns (5)–(8) present population-weighted regressions, where weights are based on 2014 city-level population. The key independent variable, $Reform$, is a dummy variable that equals 1 if the city has implemented judicial reform and 0 otherwise. Columns (1)–(8) control for interactions between pre-policy city characteristics and year dummies, as well as city and year fixed effects. City-level controls are consistent with those in Table 1.***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D2 Mechanism: Impact of Judicial Reform on Win Rates

Dependent variable:	Partial win (p.p.)		Complete win (p.p.)	
Sample:	Local prefecture	Provincial capital	Local prefecture	Provincial capital
	(1)	(2)	(3)	(4)
Panel A. *With basic control and FEs*				
PRI × Reform	-1.5111***	0.0357	-1.2620***	0.1827
	(0.4223)	(0.4822)	(0.2871)	(0.4450)
Reform	0.1063	0.0226	0.1099	0.0375
	(0.1464)	(0.1268)	(0.0744)	(0.1043)
PRI	0.7990**	-0.0084	1.0851***	0.0595
	(0.3797)	(0.1954)	(0.2913)	(0.1757)
Panel B. *Additionally control firm & city predetermined characteristics*				
PRI × Reform	-2.6000***	-0.0323	-1.5708***	0.0079
	(0.5138)	(0.7218)	(0.3169)	(0.3567)
Reform	0.3924	-0.1151	0.2984	-0.2342
	(0.2385)	(0.1516)	(0.1847)	(0.1616)
PRI	1.5542***	-0.0637	1.0864***	0.2067
	(0.4228)	(0.2952)	(0.3459)	(0.1667)
Case control	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
City FE	✓	✓	✓	✓
Dependent-variable mean	0.3722	0.3785	0.6352	0.6781
Observations	403	494	403	494

Notes: Table D2 examines the impact of judicial reform on the win rates of corporate legal judgments. The unit of observation is at the case level of legal judgments. In Columns (1) and (2), the dependent variable is a dummy indicating whether the firm achieved a partial win, while Columns (3) and (4) consider whether the firm achieved a complete win, with regression coefficients multiplied by 100 for interpretability. Columns (1) and (3) utilize cases from local prefecture courts, whereas Columns (2) and (4) focus on cases from provincial capital courts. The key independent variable, $Reform$, is a dummy variable that equals 1 if the city has implemented judicial reform and 0 otherwise. The variable PRI represents the firm's political reliance index. All columns include the firm's city and year fixed effects, as well as control variables for case characteristics, including whether the firm is the plaintiff and whether is local firm, whether the opponent is the local, and listed firm, the type of case, whether there was arbitration before the trial, the amount involved in the case, whether there was mediation before the judgment, the time from judgment to execution, and the quality of the case. Panel B additionally includes firm characteristics and interactions between pre-policy characteristics of the investment location (city) and firm characteristics with year dummies. Control variables regarding the characteristics of city and firm are identical to those in Table 1. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D3 Mechanism: Marketization Level (Investment to Local Prefectures)

Dependent variable:	Comprehensive marketization Level	Transparency of government-enterprise relation	Legal system and institutional environment	Development of the private sector	Development of the product market	Development of the factor market
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. *Investment (p.p.)*						
PRI × Reform × Marketization	-0.3397**	-0.4723***	-0.3526**	-0.2492**	-0.2324*	-0.2027*
	(0.1353)	(0.1641)	(0.1448)	(0.1054)	(0.1236)	(0.1081)
PRI × Reform	-0.2546**	-0.3805***	-0.2882**	-0.3612***	-0.2638**	-0.2713**
	(0.1179)	(0.1273)	(0.1272)	(0.1371)	(0.1196)	(0.1161)
Dependent-variable mean	0.4085	0.4085	0.4085	0.4085	0.4085	0.4085
Panel B. *Investment (logged)*						
PRI × Reform × Marketization	-0.0575**	-0.0794***	-0.0600**	-0.0423**	-0.0428*	-0.0364*
	(0.0238)	(0.0291)	(0.0252)	(0.0181)	(0.0222)	(0.0192)
PRI × Reform	-0.0410**	-0.0591***	-0.0446**	-0.0564**	-0.0408**	-0.0435**
	(0.0204)	(0.0222)	(0.0221)	(0.0237)	(0.0205)	(0.0202)
Dependent-variable mean	0.0711	0.0711	0.0711	0.0711	0.0711	0.0711
Pre-policy city-level control × year	✓	✓	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓	✓	✓
Observations	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162

Notes: Table D3 examines how the impact of judicial reform on firm investment varies across different time-varying market institutional environments. The unit of observation is at the firm-investment location-year level. In Panels A, the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Panels B, the dependent variable is the investment amount (logged). The key independent variable, $Reform$, is a dummy variable that equals 1 if the city has implemented judicial reform and 0 otherwise. The variable PRI represents the firm's political reliance index. All columns include interactions between the baseline core variables and various measures of the marketization index, as well as their corresponding double interactions. Column (1) uses a comprehensive marketization index, while Columns (2)–(6) use five sub-indices of the marketization index (*Government–Enterprise Transparency*, *Legal System Quality*, *Private Sector Development*, *Product Market Development*, and *Factor Market Development*), where higher scores indicate greater market institutionalization and competitiveness, and less government-business collusion. Other control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level.$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

TABLE D4 Mechanism: Government Subsidy (Investment to Local Prefectures)

Subsidy:	Frequency, 2014		Number, 2014		Average frequency, 2011-2014		Average number, 2011-2014	
	Investment (p.p.)	Investment (logged)	Investment (p.p.)	Investment (logged)	Investment (p.p.)	Investment (logged)	Investment (p.p.)	Investment (logged)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PRI × Reform × Subsidy	-1.0003***	-0.1694***	-1.0707***	-0.1843***	-1.1161***	-0.1851***	-0.6974**	-0.1186**
	(0.2746)	(0.0469)	(0.3385)	(0.0586)	(0.2527)	(0.0440)	(0.2994)	(0.0525)
PRI × Reform	-0.3434***	-0.0531**	-0.3663***	-0.0566***	-0.3625***	-0.0560***	-0.3711***	-0.0573***
	(0.1198)	(0.0209)	(0.1197)	(0.0207)	(0.1205)	(0.0210)	(0.1215)	(0.0211)
Pre-policy city-level control × year	✓	✓	✓	✓	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓	✓	✓	✓	✓
Dependent-variable mean	0.4085	0.0711	0.4085	0.0711	0.4085	0.0711	0.4085	0.0711
Observations	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162	2,386,162

Notes: Table D4 examines how the impact of judicial reform on firm investment varies across different pre-reform patronage networks. The unit of observation is at the firm-investment location-year level. In Columns (1), (3), (5) and (7), the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Columns (2), (4), (6) and (8), the dependent variable is the investment amount (logged). The key independent variable, $Reform$, is a dummy variable that equals 1 if the city has implemented judicial reform and 0 otherwise. The variable PRI represents the firm's political reliance index. All columns include interactions between the baseline core variables and various measures of pre-reform government subsidy conditions, along with their corresponding double interactions. Columns (1) and (2) utilize a dummy variable indicating whether the firm received government subsidies in 2014. Columns (3) and (4) employ the number of government subsidy projects in 2014. Columns (5) to (8) use the average government subsidy situation from 2011 to 2014 as a measure of the extent to which firms relied on political ties when investing in the region. Other control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level.***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D5 Mechanism: Government Revolver (Investment to Local Prefectures)

Revolver:	Yes=1, 2014		Average number, 2011-2014	
	Investment (p.p.)	Investment (logged)	Investment (p.p.)	Investment (logged)
	(1)	(2)	(3)	(4)
PRI × Reform × Revolver	-0.7054***	-0.1139***	-0.7592***	-0.1223***
	(0.2393)	(0.0426)	(0.2451)	(0.0433)
PRI × Reform	-0.3257***	-0.0502**	-0.3335***	-0.0513**
	(0.1187)	(0.0207)	(0.1192)	(0.0208)
Pre-policy city-level control × year	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓
Dependent-variable mean	0.4085	0.0711	0.4085	0.0711
Observations	2,386,162	2,386,162	2,386,162	2,386,162

Notes: Table D5 examines how the impact of judicial reform on firm investment varies across different pre-reform patronage networks. The unit of observation is at the firm-investment location-year level. In Columns (1) and (3), the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Columns (2) and (4), the dependent variable is the investment amount (logged). The key independent variable, $Reform$, is a dummy variable that equals 1 if the city has implemented judicial reform and 0 otherwise. The variable PRI represents the firm's political reliance index. All columns incorporate interactions between the baseline core variables and various measures of pre-reform political reliance, including their corresponding double interactions. Columns (1) and (2) utilize a dummy variable indicating the presence of revolving door officials among the firm's senior management in 2014, while Columns (3) and (4) employ the average number of revolving door officials per year from 2011 to 2014. Other control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level.***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D6 Mechanism: Corruption (Investment to Provincial Capitals)

Reform exposure weight =	Destination		Amount	
Dependent variable:	Investment (p.p.)	Investment (logged)	Investment (p.p.)	Investment (logged)
	(1)	(2)	(3)	(4)
Panel A. *Corrupt official number*				
PRI × Reform exposure × Corruption	4.1626***	0.7994***	3.7692***	0.7177***
	(1.3375)	(0.2721)	(1.2896)	(0.2547)
PRI × Reform exposure	4.7958***	0.7482**	2.7980	0.4556
	(1.6796)	(0.2882)	(1.8095)	(0.3016)
Panel B. *Corrupt official number (weighted by administrative level)*				
PRI × Reform exposure × Corruption	5.2420***	0.9948***	4.7030***	0.8840***
	(0.8980)	(0.1794)	(0.8463)	(0.1646)
PRI × Reform exposure	4.8046***	0.7498***	2.8043	0.4568
	(1.5496)	(0.2605)	(1.7109)	(0.2804)
Pre-policy city-level control × year	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓
Dependent-variable mean	2.3780	0.4006	2.3780	0.4006
Observations	208,997	208,997	208,997	208,997

Notes: Table D6 examines how the impact of judicial reform exposure in local prefectures on firms' investment in provincial capital cities varies across levels of corruption in those cities. The unit of observation is at the firm-investment location-year level. In Columns (1) and (3), the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Columns (2) and (4), the dependent variable is the investment amount (logged). The key independent variable, $Reform_exposure$, measures the intensity of judicial reform impact on firms in local prefectures, as defined by Equation 4. The variable PRI represents the firm's political reliance index. All columns incorporate interactions between the baseline core variables and various measures of corruption levels in provincial capital cities, including their corresponding double interactions. Panel A utilizes the number of officials who fell in anti-corruption campaigns, while Panel B uses the number of fallen officials (weighted by their administrative rank) as a measure of city corruption levels (see subsection A.3 for detail). Other control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level.***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE D7 Mechanism: Government Favoritism (Investment to Provincial Capitals)

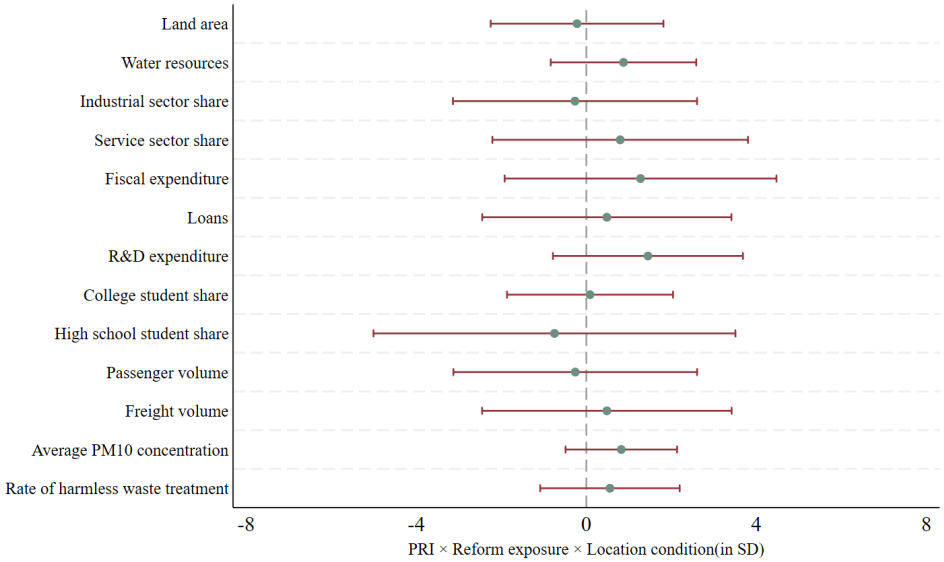
Reform exposure weight =	Destination		Amount	
Dependent variable:	Investment (p.p.)	Investment (logged)	Investment (p.p.)	Investment (logged)
	(1)	(2)	(3)	(4)
Panel A. *Government Favoritism procurement (yes=1)*				
PRI × Reform exposure × Favoritism	9.7954***	1.4540***	7.7216***	1.1142***
	(1.9872)	(0.3285)	(2.0337)	(0.3343)
PRI × Reform exposure	-2.3211	-0.3173	-2.5102	-0.3200
	(1.6840)	(0.2711)	(1.5576)	(0.2542)
Panel B. *Government Favoritism procurement (ratio)*				
PRI × Reform exposure × Favoritism	3.2515**	0.5121*	2.9668**	0.4749*
	(1.4537)	(0.2580)	(1.4481)	(0.2560)
PRI × Reform exposure	6.1671***	0.9620***	4.1037**	0.6639**
	(1.6647)	(0.2938)	(1.7354)	(0.2986)
Pre-policy city-level control × year	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓
Dependent-variable mean	3.6607	0.6195	3.6607	0.6195
Observations	238,670	238,670	238,670	238,670

Notes: Table D7 examines how the effect of judicial reform exposure in local prefectures on firms' investment in provincial capitals varies by the degree of local favoritism toward politically reliant firms. The unit of observation is at the firm-investment location-year level. In Columns (1) and (3), the dependent variable is a binary indicator for whether the firm invested in the location; coefficients are multiplied by 100 for interpretability. In Columns (2) and (4), the dependent variable is investment amount(logged). The key independent variable, $Reform_exposure$, measures the intensity of judicial reform impact on firms in local prefectures, as defined by Equation 4. The variable PRI represents the firm's political reliance index. All regressions interactions between the baseline core variables and alternative measures of government favoritism in provincial capitals. Panel A uses a dummy indicating whether connected firms receive procurement; Panel B uses the share of procurement awarded to connected firms. Other control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level.***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

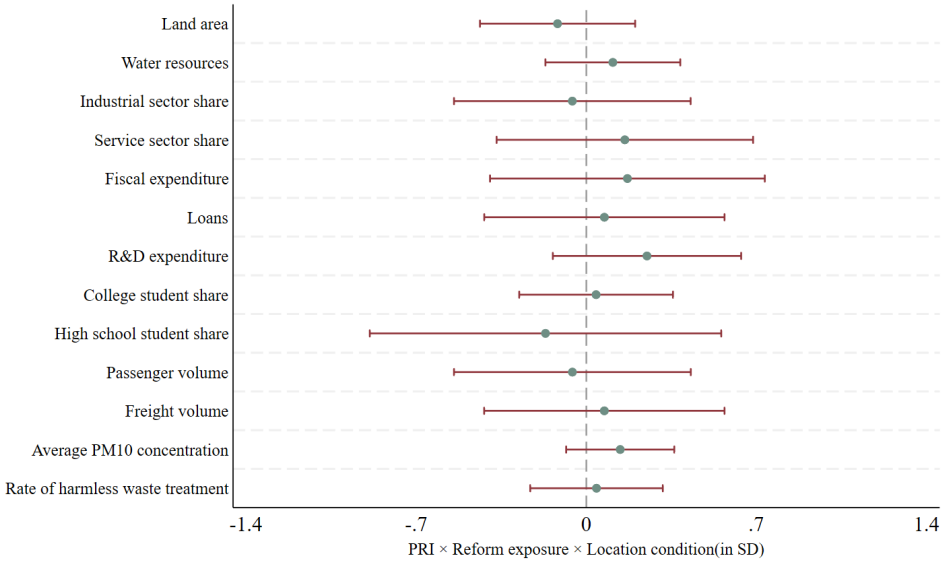
TABLE D8 Mechanism: Political Orientation Searching Index (Investment to Provincial Capitals)

Reform exposure weight =	Destination		Amount	
Dependent variable:	Investment (p.p.)	Investment (logged)	Investment (p.p.)	Investment (logged)
	(1)	(2)	(3)	(4)
Panel A. *Mobile search*				
PRI × Reform exposure × Political orientation	4.2402***	0.6642**	3.5639**	0.5679**
	(1.4782)	(0.2730)	(1.4655)	(0.2621)
PRI × Reform exposure	6.1451***	0.9586***	4.0821**	0.6604**
	(1.6185)	(0.2857)	(1.7432)	(0.2986)
Panel B. *PC search*				
PRI × Reform exposure × Political orientation	4.0212*	0.6192	3.6279*	0.5733
	(2.0810)	(0.3731)	(2.0538)	(0.3614)
PRI × Reform exposure	6.1349***	0.9570***	4.0713**	0.6587**
	(1.5662)	(0.2786)	(1.6873)	(0.2901)
Pre-policy city-level control × year	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓
Firm-location pair FE	✓	✓	✓	✓
Headquarters-year FE	✓	✓	✓	✓
Province (subsidiary)-year trend	✓	✓	✓	✓
Dependent-variable mean	3.6607	0.6195	3.6607	0.6195
Observations	238,670	238,670	238,670	238,670

Notes: Table D8 examines how the impact of judicial reform exposure in local prefectures on firms' investment in provincial capital cities varies across levels of political orientation in those cities. The unit of observation is at the firm-investment location-year level. In Columns (1) and (3), the dependent variable is a binary indicator of whether the firm invested in the location, with regression coefficients multiplied by 100 for interpretability. In Columns (2) and (4), the dependent variable is the investment amount (logged). The key independent variable, $Reform_exposure$, measures the intensity of judicial reform impact on firms in local prefectures, as defined by Equation 4. The variable PRI represents the firm's political reliance index. All columns include interactions between the baseline core variables and various measures of political orientation in provincial capital cities, along with their corresponding double interactions. Political orientation is measured by the search frequency of political terms related to the Communist Party of China, party members, and party building in *Baidu* Index (see subsection A.3 for detail). Panel A utilizes search frequency from mobile devices, while Panel B uses search frequency from PC devices, with both measures standardized. Other control variables and FEs are identical to those in Table 1. Standard errors are clustered at the city level.***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.



(A) **Investment (p.p.).**



(B) **Investment (logged).**

FIGURE D1 Ruling Out Relocation Driven by Locational Advantages

Notes: Figure D1 presents a test that rules out alternative explanations based on locational advantages. Building on the specification in Equation 3, we estimate three-way interactions between *PRI*, *Reform Exposure*, and six categories of provincial-capital characteristics, together with the corresponding two-way interactions. Each dot represents a separate regression. We examine whether high-PRI firms facing greater reform exposure disproportionately relocate to provincial capitals with (i) natural and resource advantages (land availability, water resources), (ii) economic structure (industry or services share of GDP), (iii) greater fiscal capacity (public expenditure, local debt, government R&D support), (iv) stronger human capital (share of college-educated or high-school-educated residents), (v) superior transportation infrastructure (passenger and cargo volume), or (vi) better environmental quality (air pollution levels and rate of harmless waste treatment). All variables capturing locational advantages are standardized. The dependent variable in Panel A is a binary indicator for whether the firm invested in the location; coefficients are multiplied by 100 for interpretability. In Panel B, the dependent variable is investment amount(logged). Red lines indicate 95% confidence intervals.

Appendix E Resilient Patronage and Corporate Risk Mitigation

In this subsection, we briefly discuss the efficacy of firms' investment reallocation. As shown in previous sections (subsection 7.2 and 7.3), these firms experienced heightened policy uncertainty and declined judicial favoritism, inducing a shift in investment toward new centers of political patronage. Below we show that their proactive investment shift helps preserve overall investment levels and economic performance.

Table E1 assess how firms with different political reliance respond to the reform. Columns (1) and (2) show that while reform led to a reduction in the number of investment locations, the extent of contraction did not differ significantly between high- and low-PRI firms. This implies both groups concentrate investment: low-PRI firms expanded into reformed regions while high-PRI firms concentrated in provincial capitals. Columns (3) and (4) reveal total investment amounts remained stable across both groups. Table E2 further assesses post-reform firm performance across key financial dimensions, including profitability, efficiency, liquidity, growth, and market value. The results reveal no significant post-reform performance deterioration for high-PRI firms, implying that their reallocation strategy successfully cushioned potential economic shocks.

TABLE E1 Extension: Impact on Total Investment

Dependent variable:	Investment (p.p.)		Investment (logged)	
Reform exposure weight=	Destination	Amount	Destination	Amount
	(1)	(2)	(3)	(4)
PRI × Reform exposure	0.2216	0.1487	0.3932	0.5340
	(0.6265)	(0.6149)	(0.5247)	(0.6494)
Reform exposure	-0.2924***	-0.2429***	0.0185	0.0251
	(0.0886)	(0.0888)	(0.0384)	(0.0326)
Pre-policy city-level control × year	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Province-year trend	✓	✓	✓	✓
Observations	7,812	7,812	7,812	7,812

Notes: Table E1 presents results on the impact of reform on total investment. The unit of observation is at the firm-year level. In Columns (1)–(2), the dependent variable is the total number of investment locations. In Columns (3)–(4), that is the investment amount (logged). The key independent variable, $Reform_exposure$ measures the intensity of the reform's impact on firms in local prefectures, as defined in Equation 4. The variable PRI represents the firm's political reliance index. All columns include firm and year fixed effects, province-specific linear time trends , as well as interactions between pre-policy firm location city and firm characteristics with year dummies. Control variables about characteristics of the firm's parent company's city and firm-specific characteristics are identical to those in Table 1. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE E2 Extension: Impact on Performance

Dependent variable:	Profitability		Liquidity & Solvency			Leverage	Growth & Market Performance			
	Gross margin	Operating margin	Total asset turnover	Current ratio	Quick ratio	Debt-equity ratio	Income growth	Net income growth	Cash flow	Net income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. *Investment destination share*										
PRI × Reform exposure	0.0217	-0.0150	-0.2941	0.5450	0.5907	-0.8964	0.2404	1.3726	1.2196	0.9296*
	(0.4348)	(0.2572)	(0.3634)	(0.5244)	(0.5455)	(1.2855)	(0.1657)	(1.1904)	(0.8979)	(0.5336)
Reform exposure	-0.0161	-0.0072	0.0432	-0.0673	-0.0665	0.2848	0.0321	-0.1897	-0.1502	-0.1004
	(0.0609)	(0.0288)	(0.0666)	(0.1337)	(0.1378)	(0.2324)	(0.0665)	(0.1872)	(0.1160)	(0.0767)
Panel B. *Investment amount share*										
PRI × Reform exposure	0.0007	-0.0049	-0.3567	0.5003	0.5564	-0.8968	0.2030	1.1047	1.1410	0.7618
	(0.4287)	(0.2241)	(0.3486)	(0.5025)	(0.5223)	(1.1973)	(0.1474)	(1.0417)	(0.8602)	(0.5210)
Reform exposure	-0.0170	-0.0053	0.0522	-0.0712	-0.0712	0.2638	0.0184	-0.1530	-0.1389	-0.0865
	(0.0583)	(0.0239)	(0.0626)	(0.1238)	(0.1277)	(0.2172)	(0.0588)	(0.1428)	(0.1092)	(0.0735)
Pre-policy city-level control × year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pre-policy firm control × year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province-year trend	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	7,811	7,811	7,796	7,812	7,812	7,812	7,812	7,811	7,812	7,078

Notes: Table E2 presents results on the impact of judicial reform on total investment. The unit of observation is at the firm-year level. Dependent variables are categorized into four groups: Columns (1) and (2) focus on profitability and efficiency metrics, including gross margin and operating margin; Columns (3) and (5) analyze liquidity and solvency, featuring total asset turnover, current ratio, and quick ratio; Columns (6) explore financial leverage through the debt/equity ratio; and Columns (7) to (10) assess growth and market performance, encompassing income growth, net income growth, cash flow, and net income. The key independent variable, $Reform_exposure$ measures the intensity of the judicial reform's impact on firms in local prefectures, as defined in Equation 4. The variable PRI represents the firm's political reliance index. All columns include firm and year fixed effects, province-specific linear time trends , as well as interactions between pre-policy firm location city and firm characteristics with year dummies. Control variables about characteristics of the firm's parent company's city and firm-specific characteristics are identical to those in Table 1. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.