Bankruptcy efficiency, collateral liquidation and capital misallocation

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Abstract

This paper examines how improvements in bankruptcy resolution affect the ex-ante allocation of capital among surviving firms. Using the staggered establishment of bankruptcy court across Chinese municipalities from 2008 to 2020 as a quasi-natural experiment, we find that enhanced judicial efficiency unexpectedly worsens capital misallocation in the manufacturing sector. Specifically, our estimates show that bankruptcy court establishment increases capital misallocation by approximately 7.7% relative to the baseline level. To explain this, we develop a model of bank lending under asymmetric information to identify three transmission channels: pricing, collateral, and bank screening channel. Further mechanism analysis shows that, while the pricing channel is not significant, the aggregate outcome is more likely driven by quantity-side reallocation, where the bank screening channel dominates. Higher expected recovery from bankruptcy weakens banks' incentives to collect information about borrower quality, leading to a reallocation of credit away from high-productivity firms toward less productive firms with abundant collateral. These findings reveal that judicial reforms can generate unexpected aggregate allocation costs in emerging markets where credit rationing operates through quantity constraints.

Keywords: bankruptcy courts, capital misallocation, bank screening, collateral

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1. Introduction

Financial frictions limit firms' access to external finance and, in turn, hinder investment, innovation and economic development (Banerjee and Duflo, 2005). Strong creditor rights and effective bankruptcy institutions are widely viewed as pillars of a well-functioning financial system (La Porta et al., 1997; La Porta et al., 1998). By improving the resolution of corporate distress, efficient bankruptcy procedures can raise asset recovery and reduce the deadweight losses associated with firm failure (Rodano et al., 2016; Iverson, 2018).

Motivated by this, a large literature examines how stronger creditor protections and faster court procedures affect financial markets and firm behavior. These studies fall into two broad groups. One strand studies distressed firms directly, for example their asset recovery, redeployment, and exit (Bernstein et al., 2019). The other examines indirect, general-equilibrium consequences for surviving firms, such as asset prices in the same industry (Benmelech and Bergman, 2011) , firms' financing and investment choices (Ponticelli and Alencar, 2016). Despite this breadth, few studies have directly linked judicial efficiency to macro-level capital misallocation. Furthermore, if we look into the micro-level credit allocation mechanism underlying misallocation, the existing literature focuses mainly on the price channel, while paying relatively little attention to the quantity channel of credit (Rodano et al., 2016; Müller, 2022). Ponticelli and Alencar (2016) are among the earliest to formally investigate how improvements in bankruptcy enforcement affect credit access. They argue that better court enforcement quality facilitates firms' access to credit, and that stronger protection of secured creditors allows industries with high asset pledgeability to benefit more. However, they did not thoroughly examine the specific mechanisms underlying changes in credit access, nor did they fully account for the heterogeneity among credit-receiving firms. Taken together, the impact of improvements in bankruptcy efficiency on capital misallocation remains unclear, and the underlying mechanisms warrant deeper investigation.

Based on the above, this paper asks a broader question: how does improvement in ex-post bankruptcy resolution reshape the ex-ante allocation of capital among surviving firms? Standard reasoning suggests that stronger legal enforcement should expand credit access and improve aggregate efficiency. We present evidence that qualifies this conventional view. Using the staggered establishment of bankruptcy courts across Chinese municipalities, we find that higher

judicial efficiency in corporate default resolution unexpectedly worsens capital misallocation in the manufacturing sector.

To explain this result, we first develop a model of bank lending under asymmetric information, inspired by the lazy-bank framework of Manove et al. (2001). Besides the classic ***pricing channel*** studied in earlier work, an improvement in creditor recovery affects credit supply through another two opposing forces. First, higher expected recovery relaxes collateral requirements. This ***collateral channel*** expands credit access for productive but asset-light firms and tends to reduce misallocation. Second, higher recovery lowers the expected loss from unscreened lending and weakens banks' incentives to collect soft information. This ***bank screening*** channel allows low-productivity but asset-heavy firms to obtain credit and tends to increase misallocation.

Based on the model, our analysis indicates that, in more marketized financial systems, lower borrowing costs may dominate and improve allocation. In China during our sample period, however, loan rates were only partially flexible, so quantity-side reallocation is more likely to drive the aggregate outcome. Moreover, which force dominates depends on local banking structure and the distribution of firm-level productivity and asset pledgeability. It should be noted that different mechanisms exert distinct effects on firms with different combinations of productivity and asset pledgeability. By classifying firms into distinct types, we can cleanly identify the net effect of each mechanism, which also provides a solid foundation for our subsequent mechanism tests.

Next, in the empirical analysis, we exploit the staggered rollout of bankruptcy courts across Chinese prefecture-level cities as a quasi-natural experiment. Drawing on firm-level data from the Chinese Annual Tax Survey for 2008 to 2020, we examine how improvements in bankruptcy efficiency affect capital misallocation among surviving firms. By covering all types and sizes, especially millions of micro and small firms at the margin of formal credit, the dataset enables us to recover shifts in bank screening and capital misallocation, which is an advantage that datasets restricted to large listed firms cannot offer.

To be more specific, we estimate treatment effects using a staggered difference-in-differences design and assess the robustness of our findings through a battery of checks, including event studies, treatment effect heterogeneity analyses, and synthetic difference-in-differences. To measure capital misallocation, we follow Hsieh and Klenow (2009), incorporate heterogeneous

factor output elasticities as in Ackerberg et al. (2015), and recover firm-level markups and productivity shocks using the approach of De Loecker and Warzynski (2012). This framework allows us to separate factor-market distortions from product-market frictions and transitory productivity shocks.

The baseline estimates show the aggregate effect directly. At city-industry level, we find that the establishment of bankruptcy courts significantly worsens capital misallocation in affected regions. Firm-level regressions deliver the same conclusion. This corresponds to an increase of approximately 7.7% relative to the average level of misallocation across the sample.

Explaining the underlying mechanism requires separating supply-side changes in bank behavior from endogenous shifts in firm demand. Stronger creditor enforcement raises the threat of premature liquidation, and high-tangibility firms may deleverage to avoid that risk (Vig, 2013). If this demand-side mechanism were dominant, we would expect a contraction in credit concentrated among asset-rich firms.

Our mechanism tests do not support the prediction. Across subsamples sorted by productivity and tangibility, absolute bank credit volumes do not change materially. We find neither a uniform expansion nor contraction of credit. Instead, the reform changes the composition of credit. Banks shift funding away from high-productivity firms with ample collateral, which had benefited from bank screening before the reform, and toward less productive firms.

After ruling out the competitive hypothesis, we empirically test the three mechanisms separately based on the propositions derived in the model section. The results reveal that the pricing channel is not significant in our sample while quantity channel emerges as the key factor governing the direction of capital misallocation. Furthermore, triple-difference and quadruple-difference specifications in different subsamples separate the collateral channel from the screening channel and indicate that the screening channel dominates. This conclusion remains robust under various tests. Also, local banking conditions matter as well. Differences in bank competition across regions shape how these channels operate.

The impact above also propagates through the court system in a way that highlights the institutional structure of the reform. Bankruptcy courts are usually established within the intermediate courts or operate independently to handle bankruptcy and liquidation cases. When we split regions by the court handling the relevant cases, the baseline effect appears with a delay

for firms registered under district-level courts. This lag is consistent with an institutional spillover. Intermediate courts handle the earliest and most complex default cases through specialized panels. Meanwhile, provincial High People's Courts further contribute to this process by fostering inter-court cooperation and by requiring intermediate courts to guide lower-level courts in applying similar standards, thereby also raising bankruptcy efficiency at the district levels. As a result, the reallocation of credit emerges in larger intermediate jurisdictions and then spreads downward through the local economy.

This paper relates to three strands of research. First, it contributes to the literature on the economic consequences of bankruptcy institutions. On the ex-ante side, reforms that strengthen creditor rights can reduce financing costs by improving expected recovery, whereas debtor-friendly regimes can raise interest rates by increasing strategic default incentives (Rodano et al., 2016). Related work emphasizes that judicial efficiency is a crucial determinant of how financial reforms translate into real outcomes. For example, Ponticelli and Alencar (2016) show that improvements in creditor protection expand secured credit and investment only where courts can enforce contracts effectively. On the ex-post side, bankruptcy procedures also affect how distressed assets are reallocated. Bernstein et al. (2019) highlight that liquidation can be inefficient under search frictions, whereas reorganization may preserve value by avoiding fire sales. Relative to this literature, which primarily evaluates firm-level outcomes such as borrowing terms, investment, or the fate of distressed firms, our paper focuses on a different object: how improvements in ex-post resolution reshape the ex-ante allocation of capital across surviving firms in the aggregate.

Second, the paper adds to the banking literature on credit allocation under judicial frictions. Existing work often emphasizes pricing channel in which better enforcement reduces credit risk and lowers loan rates (Rodano et al., 2016; Müller, 2022). Those studies typically focus on more marketized financial systems. Our setting suggests that in emerging markets, like China during the sample period, the main transmission may operate through credit availability and quantity rationing. Building on the lazy-bank logic of Manove et al. (2001), we argue that higher ex-post recovery makes lending less sensitive to borrower quality, weakens banks' incentives for costly ex-ante screening, and relaxes collateral requirements. Importantly, this does not produce a uniform expansion of credit. Instead, it changes the composition of credit across borrowers. Under the influence of two opposing quantity channels, credit is reallocated away from firms that

combine high collateral capacity with high productivity toward other firms, which finally induce capital misallocation. The quantity channel, especially the bank screening mechanism, provides a micro foundation for why better bankruptcy adjudication can generate aggregate allocation costs.

Finally, the paper contributes to the macro literature on the micro-origins of capital misallocation. That literature measures allocative efficiency through firm-level wedges (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009) and studies mechanisms including financial frictions, firms' balance-sheet dynamics, and product-market distortions (Midrigan and Xu, 2014; Moll, 2014; Uras, 2014; David and Venkateswaran, 2019). The allocative consequences of judicial infrastructure remain underexplored. The closest precedent is Boehm and Oberfield (2020), who show that weak contract enforcement distorts firms' input choices and vertical structure. Based on the exogenous shocks introduced by the bankruptcy court, we identify a distinct channel operating through credit and capital allocation rather than through intermediate-input sourcing.

The rest of the paper proceeds as follows. Section 2 presents a simple conceptual framework that formalizes the mechanisms and guides the empirical tests. Section 3 describes the institutional background of bankruptcy court establishment in China and details our data and variable construction. Section 4 discusses the identification strategy. Sections 5 and 6 present the empirical results and mechanism tests, respectively. Section 7 concludes.

2. A Model of Collateral, Screening, and Credit Misallocation

This section develops a model of bank lending under asymmetric information to show that an exogenous improvement in creditor recovery operates impacts through three channels—affecting credit *access*, banks' *screening incentives*, and loan *pricing*. Effects on capital misallocation through above channels carry opposing signs and the relative magnitudes are governed by institutional features and borrower distribution in the credit market. Below, we describe the model in detail.

2.1 Environment

Agents

The economy contains a unit mass of risk-neutral entrepreneurs and a competitive fringe of risk-neutral banks. The risk-free rate is normalized to zero. Each entrepreneur is endowed with a single indivisible project requiring outside financing of $I = 1$, borrowed in full by limited liability.

Technology and heterogeneity

Entrepreneurs can be classified as High or Low type. A project of type $\tau \in \{H, L\}$ succeeds with probability p_τ and yields $R > 0$. It fails with probability $(1 - p_\tau)$ and yields zero, with $p_H > p_L$. Type is the entrepreneur's private information. The bank observes only the prior $\theta \equiv Pr(\tau = H)$ and the mean success probability $\bar{p} \equiv \theta p_H + (1 - \theta)p_L$. By incurring a fixed cost $s > 0$ per applicant, the bank can perfectly learn the applicant's type.[1] Screening reveals τ but not the realization of project risk.

Collateral

Entrepreneur i owns observable tangible assets $C_i \geq 0$ whose value is realized independently of the project outcome. Departing from Manove et al. (2001), who treat collateral as an endogenous signaling instrument, we model C_i as an exogenous characteristic, taking the borrower's asset position as given and focusing on the bank's contract-design problem.

Interest-rate compression

The bank's contractual repayment in the success state cannot exceed φR with $\varphi \in (0,1)$, which therefore, imposing an interest-rate ceiling of $\varphi R - 1$. In other words, banks cannot fully price default risk through the lending rate and must rely on collateral recovery. This compression reflects China's interest-rate regulation over the sample period from 2008 to 2020, under which lending rate was subject to binding administrative constraint.[2]

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[1] We model screening as ex-ante lender-side verification of borrower type. We retain the term screening for consistency with Manove et al. (2001) and the lazy-bank literature. The qualitative results require only that screening be costly and that higher recovery reduce its private return. Therefore, perfection of the screening technology is not essential.

[2] China's interest-rate liberalization proceeded in stages. Prior to 2013, the People's Bank of China imposed a statutory ceiling on commercial bank lending rates, set as a fixed multiple of the benchmark rate. The cap was formally abolished in July 2013. From 2015 onward, following the removal of deposit-rate controls, new loan pricing shifted to a Loan Prime Rate (LPR) framework anchored to the Medium-term Lending Facility rate plus a bank-specific spread. Over 2015 to 2020, the LPR was held persistently below market-clearing levels, sustaining effective compression of the feasible lending-rate schedule. The parameter φ captures above soft ceiling.

Bankruptcy and the institutional shock

Upon default the bank seizes and liquidates the borrower's pledged assets, recovering λC_i with $\lambda \in [0,1]$. We model the establishment of bankruptcy courts as a one-time, permanent, exogenous increase in λ.

Capital misallocation

Following Hsieh and Klenow (2009), misallocation is measured through the dispersion of the marginal revenue product of capital, $MRPK_\tau = p_\tau R$, relative to the social opportunity cost of capital.

Assumption 1. $\boldsymbol{p_H R > 1 > p_L R}$

Under Assumption 1, only high-type projects are socially productive: $MRPK_H = p_H R > 1 > p_L R = MRPK_L$. In the fixed-scale environment, the social optimum funds only high-type borrowers while credit extended to low types constitutes misallocation.

2.2 The Bank's Contracting Problem

For each applicant with collateral C_i, the bank's contracting problem proceeds in three steps. Bertrand competition first pins down break-even repayment rates under each contract as functions of collateral and recovery. Imposing the interest-rate ceiling then determines the minimum collateral required for each contract to be feasible. Next, the bank compares the collateral provided by the borrower with the minimum collateral requirements for both types of contracts. If the borrower's collateral fails to meet the minimum requirements for any of the contracts, the loan will be denied. If it only exceeds the minimum collateral requirement of one type of contract, the bank will offer that type of contract to the borrower. Finally, when both contracts are feasible, the bank selects the contract that minimizes the high-type repayment rate, trading off the explicit cost of information production against the implicit cost of cross-subsidizing low types under pooling.

Step 1: Equilibrium repayment rates. Bertrand competition drives expected profits to zero on each contract type. Under the screening contract (S contract), the bank lends exclusively to high types and amortizes the screening cost at s/θ per funded loan. Under the pooling contract (NS contract), the bank lends to all applicants at a uniform rate. Zero-profit conditions yield break-even repayment rates as functions of collateral:

$$p_H R_S + (1 - p_H)\lambda C_i = 1 + \frac{s}{\theta}, \quad R_S(\lambda, C_i) = \frac{1+s/\theta-(1-p_H)\lambda C_i}{p_H} \tag{1a}$$

$$\bar{p}\, R_{NS} + (1 - \bar{p})\lambda C_i = 1, \quad R_{NS}(\lambda, C_i) = \frac{1-(1-\bar{p})\lambda C_i}{\bar{p}} \tag{1b}$$

Step 2: Feasibility thresholds. Imposing the interest-rate ceiling $\varphi R - 1$ on each contract in (1) and yielding the minimum pledge below which the break-even rate would violate the ceiling and the contract cannot be offered:[3]

$$C_S^{min}(\lambda) \equiv \frac{1+s/\theta-p_H\varphi R}{(1-p_H)\lambda}, \quad C_{NS}^{min}(\lambda) \equiv \frac{1-\bar{p}\varphi R}{(1-\bar{p})\lambda} \tag{2}$$

We maintain $1 + s/\theta > p_H\varphi R$ and $1 > \bar{p}\varphi R$ throughout, so both thresholds are strictly positive.[4] An applicant with $C_i < min\{C_S^{min}, C_{NS}^{min}\}$ is denied credit under either contract.

Step 3: Mechanism-selection threshold. When $C_i \geq max\{C_S^{min}, C_{NS}^{min}\}$, both contracts are feasible. The bank selects the contract that minimizes the high-type repayment rate. Under the screening contract, the bank eliminates the cross-subsidy that high types would bear under pooling, but incurs the explicit cost s/θ. Under the pooling contract, the bank saves the fixed cost but requires high types to subsidize low-type default losses through a higher pooling rate. When collateral is low, default losses are large and the cross-subsidy exceeds the screening cost, making screening optimal. When collateral is high, collateral recovery compresses default losses under both contracts and renders pooling optimal. Equating R_S and R_{NS} in (1) yields the mechanism-selection threshold:

$$C^*(\lambda) \equiv \frac{1}{\lambda}\left[1 - \frac{s\bar{p}}{\theta(1-\theta)(p_H-p_L)}\right] \tag{3}$$

The screening contract obtains if and only if $C_i \leq C^*(\lambda)$.

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[3] Actually, throughout, our break-even repayment rates R_S and R_{NS} here denote the gross repayment in the success state, i.e., principal plus interest, not the interest component alone. Therefore, the interest-rate ceiling $\varphi R - 1$ is equivalent to the gross-repayment cap $R. < \varphi R$ we used here.

[4] This once again demonstrates the necessity of $\varphi \in (0,1)$: Taking the screening contract as an example, we consider the high-level participation constraints as follows: $p_H R \geq 1 + s/\theta + (1 - p_H)(1 - \lambda)C$. Under limited liability, a screened high type earns $E[\pi_H] = p_H(R - R_S) - (1 - p_H)C$, where the wedge $(1 - \lambda)C$ is a deadweight liquidation loss. Substituting the break-even rate (1a) gives $p_H R > (1 + s/\theta) + (1 - p_H)(1 - \lambda)C$. Hence the high type accepts the screened contract iff the project's net surplus cover both the amortized screening cost and the expected deadweight liquidation loss. The condition is compatible with $1 + s/\theta > p_H\varphi R$ in (2) only if $\varphi \in (0,1)$ as interest-rate compression preserves borrower surplus.

Figure 1 summarizes the above decision-making process and the resulting equilibrium partition of the collateral distribution.

[Figure 1 about here]

2.3 Three Channels: Comparative Statics

Lemma 1 (Credit access). *Both feasibility thresholds in (2) are strictly decreasing in* λ.

$$\frac{\partial C_S^{min}}{\partial \lambda} = -\frac{1+s/\theta-p_H\varphi R}{(1-p_H)\lambda^2} < 0, \quad \frac{\partial C_{NS}^{min}}{\partial \lambda} = -\frac{1-\bar{p}\varphi R}{(1-\bar{p})\lambda^2} < 0 \tag{4}$$

A higher recovery rate λ raises the default-state value of any given pledge, λC_i. Holding the rate cap φR fixed, the bank can therefore break even on borrowers who post less collateral than before: the minimum pledge at which expected repayment under the cap covers the bank's cost falls. The extensive margin of credit shifts downward, toward firms with lower collateral than the previous cutoff.

Lemma 2 (Screening incentive). *The mechanism-selection threshold (3) is strictly decreasing in* λ*, provided* $C^* > 0$:

$$\frac{\partial C^*}{\partial \lambda} = -\frac{1}{\lambda^2}\left[1 - \frac{s\bar{p}}{\theta(1-\theta)(p_H-p_L)}\right] < 0 \tag{5}$$

Higher recovery reduces the loss a bank incurs on a funded low type, and hence the cross-subsidy high types bear under pooling. Once this implicit cost falls below the explicit screening cost s/θ, the bank optimally abandons type verification and lends against collateral alone, which is called *lazy banking* in Manove et al. (2001). Better creditor protection renders information production less valuable and therefore affect the equilibrium mechanism.

Lemma 3 (Loan pricing). *Both equilibrium rates in (1) are strictly decreasing in* λ*, and the pooling rate falls strictly faster:*

$$\frac{\partial R_S}{\partial \lambda} = -\frac{(1-p_H)C_i}{p_H}, \quad \frac{\partial R_{NS}}{\partial \lambda} = -\frac{(1-\bar{p})C_i}{\bar{p}}, \quad \left|\frac{\partial R_{NS}}{\partial \lambda}\right| > \left|\frac{\partial R_S}{\partial \lambda}\right| \tag{6}$$

Under perfect competition the gain from higher recovery is passed through entirely to borrowers. The pooling contract bears a higher blended default rate ($\bar{p} < p_H$), so each unit increase in λ generates greater loss savings under NS equilibrium, producing a steeper rate decline.

Lemmas 1–3 act on different margins, including extensive, screening, and pricing, respectively. Therefore they will carry different implications for misallocation depending on the composition of borrowers at each margin.

2.4 Impacts on Capital Misallocation through Different Channels

An increase in recovery from λ_0 to $\lambda_1 > \lambda_0$ shifts both C^{min} and C^* leftward based on Lemmas 1-2 above. This partitions the collateral support into five regions illustrated in Figure 2:

[Figure 2 about here]

Proposition 1 (Collateral channel). *In Region II, the credit-access expansion induced by Lemma 1 reduces capital misallocation.*

Intuitively, this proposition describes a collateral channel that operates on the extensive margin of lending. Improved bankruptcy efficiency increases the recovery value of collateral, making banks more willing to lend. This is analogous to a shock that expands the set of pledgeable assets, such as allowing patents to be used as collateral or enhancement of creditor protection for movable collateral holders. The reform effectively lowers the minimum collateral requirement for obtaining a loan.

In Figure 2, Region II contains borrowers newly admitted to credit. Under the configuration $C_S^{min} < C_{NS}^{min}$, crossing C_S^{min} from below is a transition from no credit to screened credit, under which only high types are funded.[5] Since $MRPK_H = p_H R > 1$, newly funded loan is socially productive and aggregate misallocation falls.

Therefore, credit expansion above primarily benefits previously constrained firms with productive projects but insufficient collateral. As these high-productivity firms gain access to credit, capital flows to more efficient uses, thus reducing aggregate misallocation. This implies that the effect of the collateral channel should be most pronounced for firms with low asset pledgeability. We test this prediction in our mechanism analysis by examining the net effect of the

[5] When $C_S^{min} > C_{NS}^{min}$, the entry contract is pooling and Region II admits both types. Misallocation nonetheless falls, because low-collateral entrants are disproportionately high-productivity: the correlation between pledgeable assets and log TFP in our sample is −0.028, significant at the 1% level. The collateral channel is therefore misallocation-reducing under either configuration.

reform on the low-collateral subsample.

Proposition 2 (Bank screening channel). *In Region IV, the reduction in screening incentives induced by Lemma 2 increases capital misallocation.*

Intuitively, this proposition describes a bank screening channel. An increase in bankruptcy recovery rates reduces the bank's expected loss from lending to a low-type firm without screening. This lowers the implicit cross-subsidy that high-type firms pay for low-type firms within a non-screened loan pool. As the explicit cost of screening remains unchanged, banks become rationally "lazy" and have a greater incentive to switch from screening to pooling.

To be more specific, in Figure 2, borrowers in Region IV received screened credit before the shock and low-type firms were rejected. Following the shock, the bank pools this interval and extends credit to the previously rejected low types. Since $MRPK_L < 1$ by Assumption 1, these newly funded loans are socially wasteful and misallocation rises strictly. The negative correlation between collateral and TFP amplifies this channel: high-collateral borrowers, who populate Region IV, are disproportionately low-productivity, so the mass of low types absorbed into credit when the bank ceases screening exceeds what independence between pledgeability and productivity would imply.

Empirically, this shift in bank behavior allows previously rejected low-productivity firms to obtain credit. This interpretation is consistent with Qian and Strahan (2007), who show that the collateral effectiveness depends on the legal system's ability to enforce creditor rights and make pledged assets truly recoverable. When creditor protection is weak, collateral loses much of its disciplinary force, so banks cannot rely on it to offset credit risk and instead adjust other contract terms.

Since these new loans are socially inefficient, aggregate misallocation increases. The effect is particularly strong for high-collateral firms, as banks have less incentive to screen them. This implies that the bank screening channel should dominate in the high-collateral subsample. We test this prediction by examining the net effect of the reform on high-collateral firms in our mechanism analysis.

Proposition 3 (Pricing channel). *The rate reductions induced by Lemma 3 lower misallocation in Regions II–III and raise it in Region IV. The net effect depends on the distribution of firms across regions and is theoretically ambiguous.*

Intuitively, this proposition describes a pricing channel that operates when banks pass on the gains from improved bankruptcy efficiency to borrowers by lowering interest rates. This is most likely to occur in competitive and market-oriented banking sectors.

The impact of this channel on misallocation is driven by two opposing effects. On one hand, lower interest rates allow some high-productivity firms, previously denied credit, to become profitable and enter the market (Region II), which reduces misallocation. On the other hand, lower rates can also make it profitable for banks to lend to low-productivity firms that were previously screened out (Region IV), which increases misallocation.[6] The net effect on aggregate misallocation is therefore theoretically ambiguous, depending on the relative strength of these two quantity margins.[7] We will also test for the presence of this pricing channel in our mechanism analysis.

2.5 Aggregate Response of Misallocation

Combining Propositions 1–3, the aggregate response of misallocation to the shock can be decomposed as

$$\Delta\text{Misallocation} = \underbrace{\Delta_1}_{(<0,\ \textit{Prop. 1})} + \underbrace{\Delta_2}_{(>0,\ \textit{Prop. 2})} + \underbrace{\Delta_3}_{(\textit{ambiguous, Prop. 3})} \tag{7}$$

The net effect of establishing bankruptcy courts on capital misallocation depends on the cross-sectional distribution of borrowers across regions and on the institutional parameters governing each channel. When the rate cap is slack, Δ_3 dominates. This case accords with evidence from

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[6] In Region II, a small mass of low types may also enter wherever the entry contract is pooling rather than screening, raising misallocation. Also, within Region IV, there are also firms of high types reclassified from screening to pooling and faced a higher rate, again raising misallocation. However, considering the distribution of productivity among the enterprises in our empirical sample, neither of the above two effects is the dominant factor for the corresponding regions.

[7] The rate reduction also affects firms whose borrowing status does not change. In Region III the funded set is unchanged and contains only high types, so the rate reduction lowers the financing cost of efficient borrowers alone and misallocation falls. Similarly, in Region V the pooling regime is unchanged: both types remain funded under the same contract at the identical rate R^{NS}, which falls equally for high and low types. Here the funded composition is undisturbed, so Region V contributes no direct effect. However, the ultimate impact of firms in above regions on aggregate misallocation still depends on the distribution of collateral and productivity of different enterprises and exogenous parameters.

U.S. and European insolvency reforms, where the improvement in bankruptcy efficiency leads to a decrease in interest rates, and enterprises with higher risk of failure benefit more (Müller, 2022). Totally, credit access expands and investment efficiency rises (Ponticelli and Alencar, 2016; Rodano et al., 2016). However, in economies with binding interest rate caps and a heavy reliance on collateral, the price channel may not be dominant, and the ultimate effect on misallocation depends on the quantity channel.

3. Background and Data

3.1 Institutional Background

Prior to 2007, China's insolvency system was underdeveloped, resting on an outdated 1986 statute. Formal bankruptcy was rare, leaving a large stock of zombie firms trapping capital in unproductive uses (Li and Ponticelli, 2022). In addition to the fragmented legal framework, the inefficiency also stemmed from significant judicial frictions. Bankruptcy cases were handled by generalist civil court judges who lacked specialized expertise. This, combined with judicial performance metrics that discouraged complex bankruptcy cases and local government intervention aimed at protecting employment, led to a severe backlog and impeded efficient resolution.[8]

China addressed these weaknesses in two main phases. First, the legal framework was overhauled with the passage of a new Enterprise Bankruptcy Law in 2006, which took effect in June 2007. Modeled on Western insolvency regimes, it provided a modern, unified national framework.

Second, a series of judicial reforms followed to improve implementation. This rollout occurred in a staggered fashion across cities and over time, which provides the quasi-natural experiment for our study. The reform began with the establishment of specialized liquidation and bankruptcy tribunals within existing intermediate courts, a process concentrated between 2015 and 2017.[9] In

[8] See https://www.shszfy.gov.cn/detail.jhtml?id=20250519090806343 61c3203ac33c279.

[9] This significant step in judicial reform was closely linked to the policies issued by the Supreme People's Court. From August 2016 to early 2019, a key institutional push reshaped the landscape of bankruptcy adjudication. In August 2016, the Supreme People's Court issued *The Work Plan on Establishing Liquidation and Bankruptcy Tribunals in Intermediate People's Courts*, which explicitly required that one intermediate court in each centrally-administered municipality, and the intermediate courts located in provincial capitals and sub-provincial cities, set up specialized liquidation and bankruptcy tribunals to take charge of corporate liquidation and bankruptcy adjudication. In 2018, the Supreme People's Court issued the *Minutes of the National Courts' Bankruptcy Adjudication Work Conference*, again emphasizing the establishment of dedicated liquidation and bankruptcy tribunals.

general, these tribunals were specialized units operating inside the civil courts rather than freestanding institutions. By the end of 2017, 63 intermediate people's courts had created such tribunals and the number reached 85 by the end of 2022 (INSOL, 2018). Subsequently, beginning in 2019, some of these tribunals were elevated to fully independent bankruptcy courts, while a few other cities established such courts directly. By 2020, 13 specialized bankruptcy courts were in operation. By the end of our sample period in 2020, this new judicial infrastructure was in place in many, but not all, major cities.

These specialized tribunals and courts substantially improved the efficiency of bankruptcy proceedings. Staffed with professional judges and following standardized procedures, they limited the influence of local protectionism and accelerated case resolution (Huang et al., 2022). The effect is also apparent in case statistics, which show a marked acceleration in bankruptcy filings and resolutions. Of the 59,604 cases accepted between 2007 and 2020, 54 percent were filed in the four years from 2017 to 2020, coinciding with the most active phase of the judicial reforms we study.[10] Resolution also became faster. Li and Ponticelli (2022) find that cities with specialized courts resolved bankruptcy cases about 36 percent faster than those without. Consequently, the introduction of bankruptcy courts raises the recovery rate on assets. This staggered and targeted enhancement of judicial capacity provides us an ideal setting to identify its economic consequences.

3.2 Data and Variable Construction

Our firm-level analysis utilizes the National Tax Survey spanning 2008 to 2020. We retain the complete sample of manufacturing firms.

The Tax Survey provides detailed firm-level accounting and operational information with broad coverage across all ownership types and size classes, comprehensively capturing the true underlying distribution of Chinese manufacturing firms. Its compilation is strictly supervised by the State Taxation Administration, and automated system checks combined with local agency reviews ensure data reliability and representativeness (Liu and Mao, 2019). This coverage offers a crucial empirical advantage over datasets restricted to listed companies or above-scale industrial

As a result, specialized bankruptcy tribunals in intermediate courts across the country came into being.

[10] The improvement is visible internationally as well, with China's standing on the World Bank's resolving-insolvency indicator climbing from 78th in 2014 to 51st among 190 economies by 2020. See the Report of the Law Enforcement Inspection Team of the Standing Committee of the National People's Congress on the Inspection of the Implementation of the Enterprise Bankruptcy Law.

enterprises, making the survey well suited to measuring capital allocation across the full distribution of surviving enterprises. Importantly, the survey includes smaller firms that historically face severe credit constraints and inhabit the precise margin of active bank screening. Their inclusion is therefore absolutely essential for cleanly identifying our proposed credit supply mechanisms and evaluating structural misallocation.

We also complement these firm-level records with city macroeconomic variables from the City Statistical Yearbooks and merge relevant two-digit industry characteristics.

3.2.1 Capital misallocation in factor market

Our dependent variable is the degree of capital misallocation, measured by the dispersion in the marginal revenue product of capital across firms following Hsieh and Klenow (2009). Relative to their framework, we allow factor output elasticities to vary across firms and over time. Also, we account for heterogeneous markups to isolate distortions from product markets. A further issue is that theoretical revenue is unobserved, as observed revenue is contaminated by unanticipated productivity shocks and measurement error. We therefore remove these contaminants before computing marginal revenue products. To be more specific, we estimate the output elasticities following Ackerberg et al. (2015) and recover firm-level markups and productivity shocks following De Loecker and Warzynski (2012), with the detailed estimation procedure provided in Appendix A. The specific measurement method for capital misallocation is as follows.

Consider firms within a two-digit industry that produce differentiated goods with firm- and time-specific Cobb-Douglas technologies,

$$Y_{ijt} = A_{ijt} {K_{ijt}}^{\alpha_{ijt}^K} {L_{ijt}}^{\alpha_{ijt}^L} \tag{8}$$

where the output elasticities α_{ijt}^K and α_{ijt}^L vary across firms and over time. Each firm faces a labor wedge τ_{si}^L and a capital wedge τ_{si}^K when accessing production factors and charges a markup μ_{ijt} over marginal cost. Profit maximization equates the markup-adjusted marginal revenue product of each factor to its distorted price,

$$MRPK_{ijt} = \frac{1}{\mu_{ijt}} \alpha_{ijt}^K \frac{P_{ijt} Y_{ijt}}{K_{ijt}} = \left(1 + \tau_{ijt}^K\right) R \tag{9}$$

$$MRPL_{ijt} = \frac{1}{\mu_{ijt}} \alpha_{ijt}^L \frac{P_{ijt} Y_{ijt}}{L_{ijt}} = \left(1 + \tau_{ijt}^L\right) W \tag{10}$$

where *R* and *W* are the distortion-free wage and rental rate.

In the absence of wedges, MRPs would be equalized across firms within an industry. Therefore, deviations from a common benchmark measure misallocation. We take the benchmark for industry *j* in year *t* to be the value-added-weighted average of firm-level log marginal revenue products of capital (Hsieh and Klenow, 2009),

$$\overline{mrpk}_{jt} = \sum_{i \in jt} \omega_{ijt} mrpk_{ijt}, \quad \omega_{ijt} = \frac{VA_{ijt}}{\sum_{i \in jt} VA_{ijt}} \tag{11}$$

where $mrpk_{ijt} \equiv ln\, MRPK_{ijt}$. Therefore, firm-level capital misallocation can be defined as the absolute deviation from this benchmark (Micco and Muñoz, 2024),

$$Mis_MRPK_{ijt} = \left| mrpk_{ijt} - \overline{mrpk}_{jt} \right| \tag{12}$$

with larger values indicating a more severe distortion. The dispersion of $mrpk_{ijt}$ across different enterprises can be also used as a misallocation indicator at the aggregated level.

Based on the above, we can now begin to estimate the production function and calculate the level of capital misallocation. Constructing these measures requires real value added, intermediate inputs, capital, and labor, which we obtain from the Tax Survey as follows.

Real value added. The Tax Survey reports nominal value added at the firm level. When it is missing in a given year, we compute it as nominal output less nominal intermediate inputs. We then deflate nominal value added by the two-digit manufacturing producer price index from the National Bureau of Statistics, which yields real value added that is comparable across years.

Intermediate inputs. Nominal intermediate inputs are recorded in the dataset. Using the producer price index for each industry in combination with input-output tables, a weighted average output deflator is constructed by applying weights derived from each supplying sector's share in the total intermediate output used by a given industry.[11] We then deflate them by the above industry-specific output deflator for real intermediate inputs.

Physical capital. Firms report the book value of fixed assets at original purchase prices. Considering that these values sum nominal amounts recorded in different years, they are not comparable across firms or over time. We estimate each firm's real capital stock in its first sample year from its founding information and the corresponding investment price deflator. Then, the

[11] The National Bureau of Statistics compiles input-output tables in years ending in 2 and 7. We compute the weights from the basic flow table of intermediate inputs by industry and weight the industry output price indices accordingly.

stock in later years can be built by the perpetual inventory method, following Brandt et al. (2012).

Labor input. The HK method proxies labor input by the wage bill. We avoid this proxy and instead measure labor input by the number of employees at year-end to avoid embedding distortions in the price of labor into the measured quantity of labor. Estimating the production function also requires labor compensation, which we measure as the sum of wages, salaries, and social insurance contributions in the dataset.

Finally, we drop observations with missing or non-positive values for any key variable, namely real value added, employment, labor compensation, real capital stock, and intermediate inputs. We winsorize all key real variables and their logarithms at the 1st and 99th percentiles annually. The final sample contains 1,323,025 firm-year observations for misallocation estimation.

3.2.2 Bankruptcy court information

Our key explanatory variable, *Treat*, is an indicator equal to one if prefecture-level city *c* has established a specialized bankruptcy institution, either a liquidation and bankruptcy tribunal within its intermediate court or an independent bankruptcy court, by year *t*, and zero otherwise.[12] Treatment varies at the prefecture-city level. We hand-collect the location and exact establishment dates of these institutions from the websites of the intermediate courts of individual prefecture-level cities and from related media and newspaper coverage.[13] Figure 3 maps the reform across prefecture-level cities at the end of the sample period. The left marks the cities that operate a fully independent specialized bankruptcy court, and the right marks those whose intermediate court

[12] In the baseline specification, a specialized bankruptcy tribunal or independent court is treated as having jurisdiction over all firms in the prefecture-level city in which it sits, so Treat switches on for every firm in the city upon establishment. The institutions differ, however, in the precise scope of cases they hear, which is delimited by the administrative rank of the market-regulation authority that registered the firm. The Tianjin tribunal, for instance, hears compulsory liquidation and bankruptcy cases for firms registered with district-level or higher market-regulation authorities within the municipality, whereas the Chongqing court hears cases for firms registered at the county level or above. In our heterogeneity analysis we exploit this variation, combining each firm's registering authority with the jurisdictional scope of the relevant court, to refine the treated sample.
According to data from the Ministry of Justice, by the end of 2017, there were 97 liquidation and bankruptcy tribunals across courts nationwide, comprising 3 at high courts, 63 at intermediate courts, and 31 at primary courts (which we called district-level court here). By the end of our sample period, there were over 400 intermediate courts and over 3,000 primary courts nationwide. Primary courts with such specialized tribunals accounted for less than 2% of all primary courts, and the data quality for these courts was relatively poor. We therefore focus our analysis solely on intermediate courts.

[13] We cross-checked our hand-collected dates against the data disclosed at the Ministry of Justice press conferences and against related news reports across provinces and municipalities. The dates are fully consistent across these sources.

houses a specialized bankruptcy tribunal. Figure 4 plots the introduction of specialized bankruptcy institutions over the sample period, reporting the number of cities that first adopt the reform in each year.[14] The rollout generates the staggered variation in treatment timing that identifies the difference-in-differences design developed below.

[Figure 3 about here]

[Figure 4 about here]

3.2.3 Control variables and mechanism variables

Our regressions include control variables at the firm, city, and industry levels, defined in Table 1. At the firm level, we control for firm productivity, age, size, and ownership type. At the city level, we control for the level and structure of economic development, government intervention, local financial development, and openness. At the industry level, we take two-digit manufacturing industry characteristics measured in the initial sample year 2008 as predetermined variables and interact them with a linear time trend, which controls for industry heterogeneity while limiting endogeneity concerns. In robustness checks, we replace these predetermined interactions with time-varying industry-level controls.

The mechanism analysis in Section 6 uses three key mechanism variables, *bank loan ratio*, *loan rate*, and *tangibility*, together with two additional control variables, *ROA* and *sales growth*. All five are constructed from the tax-survey balance sheets and income statements and are also defined in Table 1. For the control variables, namely ROA and sales growth, for the samples with missing values, we replace them with the corresponding industry average values for that year. Finally, all variables are winsorized at the 2.5th and 97.5th percentiles annually.

[Table 1 about here]

[14] If a city first established a liquidation and bankruptcy tribunal and later set up an independent specialized bankruptcy court, both are included when we tally the number of specialized bankruptcy institutions establishments in a given year.

3.2.4 Summary statistics

Table 2 reports summary statistics for the key variables and control variables used in our analysis.

[Table 2 about here]

4. Empirical Strategy

To estimate the effect of bankruptcy efficiency on capital misallocation, we adopt a staggered difference-in-differences design that exploits variation in the timing of bankruptcy court establishment across prefecture-level cities. The specification is as follows:

$$Mis_MRPK_{i,c,j,t} = \beta \cdot Treat_{c,t} + X_{i,c,j,t}{}'\gamma + \alpha_i + \mu_{c\times j} + \lambda_t + \varepsilon_{i,c,j,t} \quad (13)$$

The subscripts *i*, *c*, *j*, and *t* index the firm, city, industry, and year, respectively. Mis_MRPK is our firm-level measure of capital misallocation. *Treat* is an indicator variable that equals one for all years after a bankruptcy court is established in city *c*, and zero otherwise. The coefficient of interest, β, thus captures the average change in a firm's capital misallocation following the reform.

The matrix X incorporates a comprehensive set of time-varying controls spanning firm, city, and industry aggregates. We include firm fixed effects α_i to absorb time-invariant individual unobservable. Industry-city fixed effects $\mu_{c\times j}$ are also incorporated to strictly control for regional industrial structure and firm relocation dynamics. Year fixed effects λ_t eliminate aggregate macroeconomic shocks. In addition, industry-year fixed effects are also considered in our robustness check. We consistently cluster standard errors at the city level to match the primary level of treatment assignment.

While our core identification targets firm-level dynamics to align with the mechanism tests, individual misallocation structurally aggregates to generate regional efficiency losses. We therefore complement our primary micro-level design with a supplementary aggregate estimation in the next section. We compute the total dispersion of marginal revenue products specifically at the city-industry-year level to validate the transmission from micro-level distortions to aggregate economic consequences.

5. Empirical Results: Bankruptcy Court and Capital Misallocation

5.1 Baseline

We first test our hypothesis at the city-industry-year level to examine the aggregate-level impact of bankruptcy court establishment on capital misallocation. Table 3 reports the results. In column (1), we include city-industry and year two-way fixed effects. Columns (2) and (3) subsequently incorporate city-level control variables and industry-year interaction fixed effects, respectively, with the results remaining statistically significant at the 1% level. In column 3, after including a full set of fixed effects, we find a significant positive relationship between the establishment of bankruptcy courts and capital misallocation.

[Table 3 about here]

The aggregate findings establish the macroeconomic distortion. We now transition to our primary firm-level analysis to evaluate individual misalignment. Table 4 reports the baseline firm-level staggered estimates. In column 1, controlling for firm and year fixed effects, we find a positive and significant association. In columns 2 through 4, we progressively add industry-city fixed effects and a comprehensive set of firm, city, and industry controls. The result remains robust. Specifically, the coefficient in our preferred specification in column 4 is 0.1766, positive and significant at the 1% level, which implies that the reform increases capital misallocation by approximately 7.7% relative to the sample mean.

[Table 4 about here]

This finding runs counter to the conventional view, drawn largely from advanced economies, that bankruptcy reform improves resource allocation. We will examine the mechanisms in Section 6, where we show that the net deterioration reflects the dominance of a bank-screening channel over a countervailing collateral channel.

5.2 Identification Concerns

In the following section, we show that our results are robust to allowing for treatment effect heterogeneity and using synthetic controls. In addition, we show that our baseline results are not driven by differential pre-trends in the treated and control groups, as shown by dynamic plots as

well as analyses using synthetic controls and entropy balance methods. These results lend support to a causal interpretation of our results. Furthermore, we conduct several other robustness tests, which also further strengthen this argument.

5.2.1 Parallel trends and dynamic treatment effects

We first examine the dynamic effect of the reform and test for pre-existing trends. For this purpose, we modify our baseline model by incorporating a full set of time indicators relative to the year a bankruptcy court is established. The event window spans from five years before to five years after the reform, with endpoints binned. The year immediately preceding the reform is the omitted base period.

Figure 5 plots the resulting point estimates and their 95% confidence intervals. There is no pre-trend in capital misallocation prior to the establishment of bankruptcy courts and a jump emerges in misallocation ex post. Specifically, we observe a significant increase in misallocation starting from the effective year of the reform. This pattern reinforces a causal interpretation of our main findings.

[Figure 5 about here]

5.2.2 Treatment effect heterogeneity

Recent econometric work shows that the two-way fixed-effects estimator can be biased in the presence of heterogeneous treatment effects across adoption cohorts, because already-treated units may serve as "bad controls" for later-treated units, generating negatively weighted comparisons (de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021). We address this concern in several ways.

First, we implement the Goodman-Bacon decomposition to assess the sources of identifying variation in our baseline estimate. Figure 6 displays the decomposition. The weighted estimate of 0.2729 is close to the corresponding coefficient in Section 5.1, and comparisons of treated units to never-treated units account for 81.08% of the total weight. To sum up, bias arising from heterogeneous treatment effects across cohorts is unlikely to drive our result because the potentially problematic comparisons using already-treated units as controls receive only a small share of the weight.

[Figure 6 about here]

Secondly, we re-estimate the dynamic treatment effects using heterogeneity-robust estimators that are valid under arbitrary treatment-effect heterogeneity, and again test for parallel pre-trends between treated and control groups (Callaway and Sant'Anna, 2021; Gardner, 2022; Borusyak et al., 2024). Figure 7 reports the results. The dynamic effects satisfy parallel trends prior to the reform, and the treatment effect remains positive and significant in the later post-reform periods, consistent with the baseline estimates.

[Figure 7 about here]

5.2.3 Alternative control group construction

We further address concerns about treatment effect heterogeneity and pre-existing trends using the synthetic difference-in-differences (SDID) estimator from Arkhangelsky et al. (2021). This method re-weights both control units and pre-treatment periods to construct a more suitable counterfactual.

The estimated average treatment effect on the treated (ATT) is 0.2812, which is significant at the 1% level and closely aligns with our baseline estimate. Figure 8 plots misallocation trends for the 2016 and 2017 reform cohorts against their synthetic controls. The adjusted pre-treatment trends are nearly parallel. After the reform, a clear divergence emerges as misallocation rises in the treated cities relative to their counterfactuals.

[Figure 8 about here]

In Appendix B, we also plot the corresponding control-group weights and treatment effects: the SDID weights avoid excessive concentration, and the estimated treatment effects cluster above zero, with the red reference line denoting the average treatment effect. Therefore, the estimated treatment effects are consistently positive.

Also, we use entropy balancing to reweight the sample, ensuring the control variables are balanced on their means between the treated and control groups. We then re-estimate our baseline

specification on this balanced sample. Table 5 shows that the coefficient on the treatment variable is 0.1782 and significant at the 1% level, consistent with our main findings. Taken together, the evidence from above robustness checks indicates that our result is not an artifact of the staggered research design or the choice of a particular control group.

[Table 5 about here]

5.3 Further Robustness Checks

We next conduct a series of additional robustness checks. We begin with two placebo tests.

First, we test for confounding trends by creating a series of counterfactual treatment dates. We artificially advance the timing of the reform by one, two, or even five years, then re-estimate our baseline model. Figure 9 shows that these placebo treatments yield statistically insignificant coefficients, mitigating concerns that our results are driven by other contemporaneous policies.

[Figure 9 about here]

Second, we test whether our findings could be driven by chance. We randomly reassign the reform status across all cities in our sample and plot the distribution of placebo coefficients against our actual baseline estimate in Figure 10. The placebo coefficients are centered around zero while our true estimate lies in the extreme right tail, confirming that the effect is unlikely to arise by chance.

[Figure 10 about here]

We further verify that our main finding is robust to several alternative specifications. Table 6 presents these results. In column 1, we replace the industry-city fixed effects with industry-year fixed effects. In column 2, we include a one-period lag of the dependent variable as an additional control. In column 3, we cluster the standard errors at the city-industry level. Finally, in column 4, we replace the industry-level control variables with time-varying indicators calculated on an annual basis. The estimated treatment effect remains positive, significant, and similar in magnitude

across all these specifications, confirming the robustness of our baseline result.

[Table 6 about here]

5.4 Jurisdictional Scope and the Propagation of Effects

In this section, we exploit the institutional design of the bankruptcy court system to examine how the reform's impact propagates. Our baseline analysis assumes the reform applies uniformly to all firms within a treated city. The actual jurisdictional scope, however, is more nuanced and often depends on the administrative level of a firm's registration authority. This allows us to distinguish between firms directly under the purview of bankruptcy courts and those remaining under district-level court jurisdiction.

Figure 11 presents separate event studies for these two groups of firms. A distinct, staggered pattern emerges. For firms directly under intermediate-court jurisdiction, we observe an immediate and significant increase in capital misallocation in the year of the reform. In contrast, for firms under the jurisdiction of district-level courts, although the policy also exacerbated capital misallocation, the 95% confidence interval for the estimated coefficient in the implementation year narrowly includes zero. It becomes more pronounced in the subsequent year, suggesting a lagged effect.

[Figure 11 about here]

This delayed onset for district-level firms is consistent with a spillover effect. The reform's impact appears to originate with the larger, more complex cases handled by the specialized bankruptcy courts and then spreads throughout the local economy. This propagation was formalized by the courts themselves. For instance, after establishing their own bankruptcy courts, the intermediate courts in cities like Zhengzhou and Kunming explicitly guided their subordinate district-level courts to form specialized collegial panels, enhancing the professionalization of bankruptcy proceedings at all levels.[15] Furthermore, the increasingly close cooperation between

[15] For example, the *White Paper on Bankruptcy Trials of Zhengzhou Courts (2018 - 2022)* released by the Zhengzhou Intermediate People's Court clearly states: "In August 2017, the Liquidation and Bankruptcy Trial Division of the Zhengzhou Intermediate Court

intermediate courts and district-level courts also contributed to the improvement of the bankruptcy efficiency at county level. This suggests the lagged effect we identify is a plausible consequence of the broader, systemic adjustments triggered by the initial high-level reform.

6. Mechanisms

This section explores the underlying mechanisms driving the observed increase in capital misallocation. Besides the pricing channel, we evaluate two competing theoretical forces regarding bank credit supply. The collateral channel implies that improved bankruptcy efficiency relaxes lending constraints for highly productive firms. The bank screening channel posits that reduced potential losses disincentivize costly screening and allow low-productivity firms to obtain credit. Firstly, we examine the aggregate effects on credit quantity and pricing in the full sample. We then rule out a demand-driven liquidation bias by showing no absolute credit contraction among collateral-rich firms. Because the absolute credit volume remains unchanged, we use a sequence of subsample and higher-order difference designs to document the relative reallocation of credit across firms. This approach isolates the net effects of the competing channels. Finally, we explore how local banking competition shapes these dynamics and provide a series of robustness checks.

6.1 Specification and Variable Construction

All mechanism regressions follow the baseline specification in (13). We include the same set of fixed effects and control variables, plus two additional firm-level controls: *ROA* and *sales growth*. Standard errors are clustered at the city level. All variables are defined in Section 3.2.3.

$$y_{i,c,j,t} = \beta_1\left(Treat_{c,t} \times G_{i,c,j,t}\right) + \beta_2 \cdot Treat_{c,t} + \beta_3 \cdot G_{i,c,j,t} + X_{i,c,j,t}{}'\gamma + \alpha_i + \mu_{c,j} + \lambda_t + \varepsilon_{i,c,j,t} \quad (14)$$

where y is *bank loan ratio* or *loan rate*. $G_{i,c,j,t}$ is the set of {*High-TFP, High-Tangibility*}. To test our model's predictions, we interact the treatment indicator with dummy variables for different firm characteristics. For example, we define *High-TFP* as an indicator for firms with total factor productivity above the industry-year median. We then examine the coefficient on the interaction

was officially established, and it guided all grassroots courts to set up bankruptcy trial divisions or specialized collegial panels to exclusively handle bankruptcy cases." See https://zzfy.hncourt.gov.cn/upload/file/20230418/20230418104328_50800.pdf.

term *Treat* × *High-TFP* to identify the heterogeneous effect of the reform on high-productivity firms. We apply a similar approach for asset tangibility.

According to Hsieh and Klenow (2009), when resources are limited and there are no frictions, resources should flow to firms with higher productivity until the marginal revenue products of factors are equalized across firms, thereby achieving the socially optimal allocation. In this process, if resources flow disproportionately to *low-TFP* firms, resource misallocation will arise. Therefore, in the subsequent empirical analysis, when examining how different channels affect capital misallocation, we focus on whether high-productivity firms obtain more credit resources under each channel. If so, misallocation is mitigated. Otherwise, it is aggravated.

6.2 Aggregate Effects: Credit Quantity and Pricing

Table 7 reports the baseline tests of the three channels. Column (1) partitions firms by tangibility to capture the collateral channel; columns (2) and (3) partition firms by productivity to capture the screening and pricing channels, respectively, with both columns additionally controlling for tangibility to net out the collateral force.

We first test the collateral channel. In column (1), we use the *bank loan ratio* as dependent variable and interact the treatment indicator with a dummy for high-tangibility firms. The coefficient on this interaction term is negative and significant at 5% level. This shows that low-tangibility firms gain more access to bank credit after the reform. The result is consistent with Proposition 1. Improved bankruptcy efficiency relaxes credit constraints for previously rationed, low-collateral borrowers.

Column (2) turns to the screening channel. The coefficient on *Treat* × *High-TFP* is negative and significant. This indicates that credit is reallocated from high-productivity firms to low-productivity firms. This finding supports Proposition 2. Higher recovery prospects reduce the bank's incentive for costly screening, leading them to fund more low-productivity borrowers. We have to point that, the key coefficient here is only statistically significant at 10% level. This is because, at this point, we have not fully identified the net effect of this channel. In the subsequent sub-sample results, we will find that this significance has been significantly enhanced in the high-tangibility sub-sample.

Finally, column 3 investigates the pricing channel using the *loan rate* as the outcome. The estimated coefficient is statistically insignificant. This suggests the pass-through from improved

recovery rates to relative loan prices is muted in our setting. The reform's impact on misallocation therefore operates primarily through credit quantities rather than prices.

[Table 7 about here]

6.3 Demand-Side Responses and Structural Reallocation

Although we have conducted a preliminary examination of the theoretical mechanism in the previous section, the identification results are still affected by some alternative hypotheses. Among them, isolating credit supply from unobserved demand shocks poses a primary empirical challenge. Actually, our identification strategy treats the establishment of bankruptcy courts as a supply-side shock to bank risk preferences. We also absorb general demand shifts using firm fixed effects and time-varying proxies for investment opportunities, for example *ROA* and *sales growth*. However, ideal demand isolation typically still relies on micro-level proxies such as trade credit payment delays (Petersen and Rajan, 1994). Data constraints prevent us from measuring these specific payment behaviors. Therefore, we choose to rely on a cross-sectional falsification test to evaluate the demand-side response.

We consider whether specific demand-side behavioral changes drive our findings. Stronger creditor rights can introduce a premature liquidation threat. To mitigate this risk, firms with abundant tangible assets may voluntarily reduce their reliance on secured bank debt (Vig, 2013). If this demand-side liquidation bias dominates our setting, we should observe an absolute reduction in credit for high-tangibility firms.

To test this, we sort firms into four distinct cells based on the industry-year medians of productivity and tangibility. We estimate the treatment effect on the *bank loan ratio* within each specific cell. Table 8 shows that none of the treatment coefficients are statistically significant. Crucially, high-productivity and high-tangibility firms experience no significant credit contraction. This specific lack of absolute decline theoretically rules out the demand-side mechanism.

Furthermore, the overall lack of expansion eliminates a uniform credit boom. The absolute volume of bank credit remains unchanged across all subgroups. This consistent evidence indicates that the reform may induce a structural and relative reallocation of credit.

[Table 8 about here]

Our model predicts exactly this type of relative adjustment. The central prediction is not an aggregate expansion or contraction. Instead, the reform triggers a structural reallocation away from high-productivity and high-tangibility borrowers toward the rest of the economy. We test this core prediction by evaluating the credit reallocation between the four subsample cells.

We estimate a triple-difference regression across the four cells. We designate the high-productivity and high-tangibility cell as the reference group. This group represents the constrained efficient firms that predominantly received screened credit prior to the reform. Table 9 reports the results. Relative to this reference group, the treatment significantly raises the *bank loan ratio* in each of the other three cells.

[Table 9 about here]

This relative increase demonstrates a clear diversion of credit. Banks reallocate funds out of the most efficient and asset-rich firms, shifting them into less productive or asset-light competitors. This movement constitutes the key structural misallocation in our sample. The reform incentivizes a recovery-driven contraction in costly bank screening alongside a mechanical relaxation of collateral requirements. These dual supply-side forces systematically shift credit down the productivity distribution. Consequently, this targeted diversion widens the dispersion of the marginal revenue product of capital and severely degrades aggregate allocative efficiency.

6.4 The Reallocation of Credit: Screening vs. Collateral

After ruling out the competitive hypothesis, following Section 6.3, we can now adopt similar multiple difference design to further analyze the credit reallocation in subsamples and test the two quantitative mechanisms. More specifically, having established the two quantity channels separately, we now examine them jointly and then isolate the net effect of each.

6.4.1 Joint test of the quantity channels

We first construct a triple-difference specification for the *bank loan ratio*, including both interaction terms: the treatment dummy interacted with the high-productivity indicator and the treatment dummy interacted with the high-tangibility indicator. Table 10 reports this joint

estimation. Both interaction coefficients remain significant and carry opposite implications. Therefore, credit concurrently expands toward asset-light firms along the collateral margin and shifts toward low-productivity firms along the screening margin.

[Table 10 about here]

6.4.2 Net screening effect across the tangibility distribution

Our theoretical framework implies that the bank screening channel can be identified within the subset of high-collateral firms. Post-reform reallocation toward low-productivity borrowers within this group reflects the pure screening channel.

Table 11 re-estimates the impact of the treatment on high-productivity firms across different samples. Column 1 reports the full sample baseline. Columns 2 and 3 split the sample at the industry-year median of tangibility. The high-tangibility subsample in column 2 captures the isolated net screening effect.

[Table 11 about here]

Column 2 shows that the diversion of credit toward low-productivity firms is substantially stronger within the high-tangibility group. This finding is consistent with our theoretical predictions. When collateral is plentiful, banks entirely dispense with costly screening. Low-productivity firms with high tangibility consequently become the primary marginal recipients of bank credit once active screening is abandoned. Furthermore, the treatment coefficient in column 3 is positive. This indicates that, in the sub-sample with low collateralization, enterprises with higher productivity tend to obtain more credit resources. This positive sign aligns theoretically with the collateral channel despite lacking statistical significance. A further examination of this mechanism will be provided in the next subsection. Moreover, to alleviate potential concerns regarding artificial division of the subsample threshold, subsection 6.6 demonstrates the robustness of these results across alternative sample definitions.

6.4.3 Isolating the net collateral channel: a quadruple difference

The preceding subsample analysis generates a theoretically consistent but statistically

insignificant coefficient for the net collateral effect. We therefore deploy a supplementary quadruple-difference specification for more definitive identifications of this mechanism. We interact the staggered treatment indicator with both the low-tangibility and low-productivity indicators. The specification is as follows:

$$bank\ loan\ ratio_{i,c,j,t} = \delta\left(Treat_{c,t} \times Low - Tang_{i,c,j,t} \times Low - TFP_{i,c,j,t}\right) + \Theta_{i,c,j,t} + X_{i,c,j,t}{}'\gamma + \alpha_i + \mu_{c\times j} + \lambda_t + \varepsilon_{i,c,j,t} \quad (15)$$

Where the coefficient δ captures the core specific four-way interaction effect and vector $\Theta_{i,c,j,t}$ absorbs all corresponding exhaustive lower-order interaction terms. Other settings are consistent with the baseline regression. Table 12 reports these estimation results.

[Table 12 about here]

The coefficient δ is negative and statistically significant at 1% level across three distinct sample specifications, including the median-split, mean-split, and prefecture-registered restricted samples in column 1-3. This regression result demonstrates that newly available credit systematically flows toward higher-productivity borrowers within the subset of asset-light firms. The collateral channel consequently reduces resource misallocation.

Totally, the opposing empirical signs of the isolated collateral effect in Table 12 and screening effect in Table 11 mirror our theoretical channel decomposition. The observed aggregate increase in structural misallocation reflects the overwhelming dominance of the bank screening channel in our specific institutional setting.

6.5 Role of Local Banking Competition

Considering that the level of competition among banks may impact their behavioral decisions, we exploit cross-sectional spatial variation to understand how banking market structure regulates the above channels. We use the number of commercial banks in a prefecture to proxy for local banking competition and evaluate the heterogeneity of different channels. Results show that the pricing channel is insignificant in both groups. However, in terms of quantity channel, we draw

some interesting conclusions.

First, we explore the screening channel. We partition the high-tangibility subsample into high-competition and low-competition environments in Table 13. The active screening channel operates exclusively within high-competition markets. Low-competition markets display no significant screening relaxation. This pattern is consistent with the joint operation of incentives and constraints. On the incentive side, intense competition compresses bank margins. However, compressed lending rates foreclose price-cutting as a competitive instrument (Cetorelli and Strahan, 2006). Therefore, this compression finally drives banks to pursue lending volume among lower-quality borrowers once improved bankruptcy efficiency provides a liquidation backstop. On the constraint side, banks in competitive markets face higher costs of soft-information production, which makes the abandonment of screening more likely (Petersen and Rajan, 1995; Boot and Thakor, 2000). Based on the above, the deliberate relaxation of screening protocols therefore emerges as the optimal and dominant competitive strategy.

[Table 13 about here]

Second, we evaluate the collateral channel. We replicate our core quadruple-difference design across the competitive split in Table 14. The collateral channel operates significantly in both market structures. Crucially, the magnitude of the collateral effect is substantially larger in low-competition environments. This amplification effect reflects the overall severity of credit discrimination and corroborates the conclusion of Hainz et al. (2013). Banks wielding strong market power stringently discriminate against asset-light firms. When the legal reform relaxes collateral constraints, the pent-up efficient credit demand among these previously excluded high-productivity firms triggers the strongest credit reallocation exactly where rigid rationing historically dominated.

[Table 14 about here]

In summary, competitive markets overwhelmingly drive the destructive screening channel, whereas monopoly markets amplify the constructive collateral channel. This intersection of legal infrastructure and market structure is crucial to the final distribution of capital.

6.6 Robustness of the Reallocation Results

We conduct four robustness checks to validate the identified quantity channels. First, we replicate the analysis by splitting the sample using the industry-year mean of tangibility rather than the median. Second, we restrict our analysis strictly to firms with prefecture-level registration. Third, we retain only the top and bottom quartiles of the tangibility distribution. This approach curtails potential estimation noise from firms concentrated near the threshold. Fourth, we classify firms into sub-samples based entirely on their characteristics in the year prior to the policy. This pre-treatment sorting eliminates concerns regarding endogenous firm reactions to the reform.

Table 15 reports the relevant mechanism re-estimations organized across separate panels. Each panel presents the full sample alongside the designated high-tangibility and low-tangibility sub-samples. The empirical patterns remain highly stable. In every alternative specification, the structural reallocation toward low-productivity borrowers dominates within the high-tangibility group. These consistent findings confirm the robustness of our proposed supply-side transmission mechanisms.

[Table 15 about here]

Building on all the foregoing analysis, we empirically examine the three channels through which capital misallocation is affected, as proposed in Section 2. We establish that the quantity channel constitutes a significant source of capital misallocation, while the collateral channel and the bank screening channel mitigate and exacerbate capital misallocation, respectively. In this process, the rational abandonment of screening by banks serves as the key mechanism through which improvements in bankruptcy efficiency ultimately lead to an increase in capital misallocation.

7. Conclusion

This paper investigates how improvements in bankruptcy resolution efficiency reshape capital allocation among surviving firms. Our empirical analysis, grounded in a quasi-natural experiment using the staggered rollout of specialized bankruptcy courts across Chinese cities, reveals that stronger judicial enforcement in corporate default cases unexpectedly worsens capital misallocation.

Our theoretical framework identifies three transmission mechanisms through which improved

bankruptcy resolution affects credit supply and capital allocation. First, the pricing channel operates through reductions in loan rates. Second, the collateral channel works by relaxing collateral requirements, thereby expanding credit access for productive but asset-light firms. Third, the bank screening channel operates by weakening banks' incentives to collect information about borrower quality when expected recovery is higher. This mechanism generates a reallocation of credit toward less productive firms, thereby worsening the capital misallocation.

Based on the propositions derived in the model section, we empirically test the three mechanisms separately. Results show that, in our setting, the bank screening channel is the dominant force.

The key insight is that institutional improvements can have unintended consequences when the financial system operates under specific constraints. In more marketized systems with flexible pricing mechanisms, stronger creditor enforcement would reduce costs through lower interest rates. However, in emerging markets where loan rates are partially regulated and credit rationing operates through quantity constraints in the sample period, the same institutional reforms shift the composition of credit in a way that amplifies misallocation. Banks respond to improved recovery prospects not primarily by lowering rates or expanding credit uniformly, but by economizing on costly screening activities. This behavioral adaptation redirects credit flows toward firms with higher asset pledgeability but lower productivity, reversing the allocation gains that would occur in frictionless markets. For policymakers, the results suggest that institutional reforms aimed at strengthening creditor protections must be calibrated with attention to the underlying financial system structure and constraints. When credit allocation operates primarily through quantity mechanisms, standalone improvements in bankruptcy efficiency may not deliver expected efficiency gains and could even raise aggregate misallocation costs.

Our paper also documents that the reform's impact operates through institutional channels. The effect emerges first in regions where intermediate courts establish specialized courts, and subsequently propagates to lower-level jurisdictions through inter-court coordination and guidance for the work of primary courts from intermediate courts. This institutional spillover mechanism underscores that the consequences of legal reform depend not only on formal rules but also on how judicial actors implement and coordinate enforcement across hierarchical levels.

Future research could extend this framework in several directions. First, our theoretical model primarily emphasizes banks' contract selection, while the firm side remains only partially modeled.

Allowing firms to endogenously choose collateral and contract terms may generate additional implications for the interaction between bank screening, collateralization, and credit allocation. Second, because of data limitations, we are unable to observe precise firm-level bank loan quantities and interest rates. The proxy measures constructed from financial statements, although adjusted using accounts such as accounts payable, may still capture trade credit and other non-bank financing components. Future work with more direct measures of bank lending would therefore be valuable for validating the mechanism more cleanly and for separating bank credit from other financing channels. Third, examining whether these mechanisms hold in other institutional contexts and under different legal reforms would strengthen the external validity of our findings. Despite these open questions, our paper shows that the economic consequences of bankruptcy institutions are more nuanced than existing literature has recognized, and that careful empirical investigation of transmission mechanisms is essential for understanding the real effects of legal and institutional reforms.

Declaration of interests

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Tables and Figures

Table 1

Variable Definitions.

Variable	Definition
Panel A: Firm level	
Ln_TFP	Natural log of firm's tfp (measured following Ackerberg et al., 2015)
lnage	Natural log of the observation year minus the registration year.
lnsize	Natural log of total assets.
private	Indicator equal to one if the firm is privately or collectively owned.
state_owned	Indicator equal to one if the firm is state-owned.
hmt	Indicator equal to one if the firm is owned by Hong Kong, Macao, or Taiwan investors. Foreign-owned firms are the omitted category.
Bank loan ratio	Total liabilities net of accounts payable, divided by year-end total assets.
Loan rate	Financial expenses divided by year-end total liabilities net of accounts payable.
Tangibility	Net fixed assets divided by year-end total assets.
ROA	Operating profit divided by year-end total assets.
Sales growth	Natural log of operating revenue divided by one-year-lagged operating revenue.
Panel B: City level	
Log_GDP	Natural log of city GDP per capita.
Secondary Share	Share of the secondary sector in city GDP.
Fiscal	Ratio of fiscal expenditure to GDP.
Loan	Ratio of year-end outstanding bank loans of financial institutions to GDP.
Trade	Ratio of total imports and exports to GDP.
Panel C: Industry level, predetermined as of 2008	
HHI_{2008}	Revenue-based Herfindahl–Hirschman index of the firm's two-digit industry in 2008.
$Capital_{2008}$	Capital intensity (Assets at year-end / Number of workers) of the two-digit industry in 2008, taken as the industry median.
$Labor_{2008}$	Labor income share of the two-digit industry in 2008, taken as the industry median.

Table 2

Descriptive statistics of key variables.

	Observations	Mean	Sd	Min	Max
Variables at firm level					
Mis_MRPK	1,323,025	2.2933	1.5008	1.31e-06	14.9106
Treat	1,323,025	0.1424	0.3494	0	1
Ln_TFP	1,323,025	3.9102	1.1782	2.19e-06	13.2776
lnage	1,323,025	2.1058	0.6970	0	3.5264
lnsize	1,323,025	10.2600	2.0038	0	19.6464
private	1,323,025	0.7777	0.4158	0	1
state_owned	1,323,025	0.0221	0.1469	0	1
hmt	1,323,025	0.0802	0.2716	0	1
Bank loan ratio	1,259,784	0.4158	0.2911	0	1.1272
Loan rate	1,200,710	0.0234	0.0474	-0.1940	0.2862
Tangibility	1,294,381	0.2359	0.1817	0.0015	0.7440
ROA	1,173,833	0.0589	0.3026	-1.6401	1.9982
Sales growth	1,303,025	0.0265	0.0744	-0.2061	0.2719
Variables at city-industry level					
Mis_MRPK	78,177	2.2096	1.3694	0	9.0787
Control variables at city level					
Log_GDP	3,540	10.8284	0.6031	9.2762	12.0756
Trade	3,540	0.3836	0.5363	0.0055	3.2786
Fiscal	3,540	0.1722	0.0845	0.0440	0.5304
Loan	3,540	1.2790	0.6853	0.2954	3.7086
Secondary Share	3,540	0.4750	0.1192	0.1698	0.7564
Variables at two-digit industry level					
HHI_{2008}	31	0.1297	0.1527	0.0023	0.5271
$Capital_{2008}$	31	584.8847	454.3185	83.3046	2318.9490
$Labor_{2008}$	31	0.4359	0.0776	0.2943	0.5974

Notes: All continuous firm-level control variables are winsorized at the 1st and 99th percentiles within each year. The variables used in mechanism section, namely the bank loan ratio, loan rate, tangibility, ROA, and sales growth, are winsorized at the 2.5th and 97.5th percentiles within each year.

Table 3

Baseline regression results for the impact of bankruptcy court establishment on capital misallocation.

	(1)	(2)	(3)
Treat	0.2533***	0.2533***	0.2655***
	(0.0171)	(0.0171)	(0.0169)
City Controls	NO	YES	YES
Year FE	YES	YES	NO
Industry-City FE	YES	YES	YES
Industry-Year FE	NO	NO	YES
Observations	78177	78177	78177
Adjusted_R^2	0.235	0.235	0.722

Notes:

*This table reports the results at the city-industry-year level. Column (1) includes city-industry and year two-way fixed effects. Column (2) adds city-level control variables. Column (3) further adds industry-year interaction fixed effects. Standard errors are clustered at the city level to match the primary level of treatment assignment. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 4

Firm-level estimates for the impact of bankruptcy court establishment on capital misallocation.

	(1)	(2)	(3)	(4)
Treat	0.1843***	0.1828***	0.1746***	0.1763***
	(0.0361)	(0.0362)	(0.0376)	(0.0380)
Firm FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry-City FE	NO	YES	YES	YES
Firm Controls	NO	NO	YES	YES
City Controls	NO	NO	YES	YES
Industry Controls	NO	NO	NO	YES
Observations	1323025	1323025	1323025	1323025
Adjusted_R^2	0.3361	0.3636	0.3640	0.3667

Notes:

*This table evaluates the impact of bankruptcy court establishment on firm-level capital misallocation. In column 1, controlling for firm and year fixed effects, we find a positive and significant association. Considering that some enterprises changed their industry/city affiliation during the sample period, we add industry-city fixed effects in column 2. In columns 3 through 4, we progressively add a comprehensive set of firm, city, and industry controls. In Table 6, we replace the industry-city fixed effects with industry-year fixed effects. The result remains robust and magnitudes of the coefficients are basically similar. Standard errors are clustered at the city level to match the primary level of treatment assignment. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 5

Robustness check: Baseline specification re-estimated on an entropy-balanced sample.

	Entropy-balanced sample
Treat	0.1782***
	(0.0406)
Firm FE	YES
Year FE	YES
Industry-City FE	YES
Firm Controls	YES
City Controls	YES
Industry Controls	YES
Observations	776656
Adjusted R^2	0.2541

Notes:

*This table reports the results after reweighting the sample with entropy balancing. The control variables are balanced on their means between the treated and control groups. Standard errors are clustered at the city level to match the primary level of treatment assignment. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 6

Robustness check: Alternative specifications.

	(1) Fixed effect at industry-year level	(2) Delayed dependent variable	(3) Change the clustering level	(4) Control variable substitution
Treat	0.1317*** (0.2462)	0.0948* (0.0532)	0.1763*** (0.02428)	0.1747*** (0.0376)
Firm FE	YES	YES	YES	YES
Year FE	NO	YES	YES	YES
Industry-City FE	YES	YES	YES	YES
Industry-Year FE	YES	NO	NO	NO
Firm Controls	YES	YES	YES	YES
City Controls	YES	YES	YES	YES
Industry Controls	YES	YES	YES	YES
Observations	1323025	1323025	1323025	1323025
Adjusted_R^2	0.7214	0.3485	0.3667	0.3725

Notes:

*This table reports the results from four alternative specifications. Column 1 replaces industry-city and year fixed effects with industry-year fixed effects. Column 2 includes a one-period lag of the dependent variable as an additional control. Column 3 clusters standard errors at the city*industry level instead of the city level. In column 4, we replace the industry-level control variables with time-varying indicators calculated on an annual basis.. The estimated treatment effect remains positive, significant, and similar in magnitude across all specifications. These results confirm the robustness of the baseline finding. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 7

Baseline mechanism tests: Collateral, screening, and the pricing channel.

	(1)	(2)	(3)
	Bank loan ratio		*Loan rate*
Treat	-0.0007	-0.0033	-0.5162
	(0.0051)	(0.0055)	(0.5028)
High-TFP		0.0011	-0.1519
		(0.0024)	(0.1046)
High-Tangibility	-0.0046		
	(0.0063)		
Treat × High-TFP		-0.0043*	0.3513
		(0.0026)	(0.3466)
Treat × High-Tangibility	-0.0079**		
	(0.0038)		
Observations	1134805	1134805	1128298
Adjusted_R^2	0.4463	0.4475	-0.3019

Notes:

*This table reports the baseline tests of the collateral channel, the screening channel, and the pricing channel. Column (1) partitions firms by tangibility to capture the collateral channel. Columns (2) and (3) partition firms by productivity to capture the screening channel and the pricing channel respectively. Both columns additionally control for tangibility to net out the collateral force. The key coefficient in the second column is only statistically significant at the 10% level. This is because, at this point, we have not fully identified the net effect of this channel. In the subsequent sub-sample results, we will find that the significance has been significantly enhanced in the high-tangibility sub-sample. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 8

Competitive mechanism: demand-side or uniform credit boom.

	(1) HH	(2) HL	(3) LH	(4) LL
Treat	0.0031 (0.0056)	-0.0142 (0.0194)	-0.0014 (0.0078)	-0.0024 (0.0098)
Observations	251827	283751	315977	283250
Adjusted_R^2	0.8343	0.3764	0.2041	0.4467

Notes:

*This table presents the impact of the establishment of the bankruptcy court on the bank loan ratio in four different cells. Firms are sorted based on the industry-year medians of productivity and tangibility. None of the treatment coefficients are statistically significant. High-productivity and high-tangibility firms experience no significant credit contraction. This finding theoretically rules out the demand-side mechanism. The overall lack of expansion also eliminates a uniform credit boom. The absolute volume of bank credit remains unchanged across all subgroups. The evidence demonstrates that the reform induces a structural and relative reallocation of credit. Therefore, our subsequent analysis will adopt a relative difference design to evaluate the competitive supply-side mechanism. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 9

Structural reallocation of credit.

	(1) *Bank loan ratio*
Treat	-0.0146**
	(0.0064)
Treat × (Group type=HL)	0.0153***
	(0.0047)
Treat × (Group type=LH)	0.0121***
	(0.0042)
Treat × (Group type=LL)	0.0121**
	(0.0050)
Observations	1134805
Adjusted R^2	0.4463

Notes:

*This table reports the results from a triple-difference regression across the four cells. The high-productivity and high-tangibility cell serves as the reference group. This group represents the constrained efficient firms that predominantly received screened credit prior to the reform. Relative to this reference group, the treatment significantly raises the bank loan ratio in each of the other three cells. This indicates that there has indeed been a structural misallocation in credit allocation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 10

Joint test of the quantity Channels.

	(1) *Bank loan ratio*
Treat	0.0020
	(0.0053)
High-TFP	0.0014
	(0.0024)
Treat × High-TFP	-0.0047*
	(0.0026)
High-Tangibility	-0.0046
	(0.0063)
Treat × High-Tangibility	-0.0082**
	(0.0039)
Observations	1134805
Adjusted_R^2	0.4463

Notes:

*This table reports a single triple-difference specification that includes both interaction terms. The first term interacts the treatment dummy with the high-productivity indicator. The second term interacts the treatment dummy with the high-tangibility indicator. Both interaction coefficients remain significant. The two coefficients carry opposite implications. Credit expands toward asset-light firms along the collateral margin. Credit shifts toward low-productivity firms along the screening margin. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 11

Testing the net screening effect: Subsamples by asset tangibility.

	(1) Full-sample	(2) High-Tangibility subsample	(3) Low-Tangibility subsample
Treat	-0.0033 (0.0055)	0.0033 (0.0065)	-0.0097 (0.0090)
High-TFP	0.0011 (0.0024)	0.0026 (0.0027)	0.0008 (0.0043)
Treat × *High-TFP*	-0.0043* (0.0026)	-0.0085** (0.0041)	0.0032 (0.0043)
Observations	1134805	567804	567001
Adjusted R^2	0.4475	0.4598	0.4410

Notes:

*This table re-estimates the treatment effect on high-productivity firms. Column 1 reports the full sample baseline. Columns 2 and 3 split the sample at the industry-year median of tangibility. The high tangibility subsample in column 2 captures the isolated net screening effect. The low tangibility subsample in column 3 captures the collateral channel. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 12

Testing the Collateral Channel: A quadruple difference.

	Bank loan ratio		
	(1)	(2)	(3)
	Median-Split	Mean-Split	Intermediate court
Treat	-0.0146**	-0.0173**	-0.0129**
	(0.0064)	(0.0070)	(0.0061)
Low-Tangibility	0.0028	0.0008	-0.0026
	(0.0069)	(0.0067)	(0.0030)
Treat × Low-Tangibility	0.0153***	0.0165***	0.0152***
	(0.0047)	(0.0053)	(0.0051)
Low-TFP	-0.0030	-0.0055**	-0.0034
	(0.0027)	(0.0025)	(0.0025)
Treat × Low-TFP	0.0121***	0.0144***	0.0129***
	(0.0042)	(0.0046)	(0.0040)
Low-Tangibility × Low-TFP	0.0035	0.0038	0.0060
	(0.0039)	(0.0032)	(0.0038)
Treat × Low-Tangibility × Low-TFP	-0.0152***	-0.0162***	-0.0148***
	(0.0055)	(0.0056)	(0.0056)
Observations	1134805	1134805	917374
Adjusted_R^2	0.4463	0.4463	0.5078

Notes:

*We interact the staggered treatment indicator with both the low-tangibility and low-productivity indicators. We focus on the coefficient of Treat × Low-Tangibility × Low-TFP across three distinct sample specifications. These include the median-split subsamples in column 1, the mean-split subsamples in column 2, and the prefecture-registered restricted sample in column 3. This regression result demonstrates that newly available credit systematically flows toward higher-productivity borrowers within the subset of asset-light firms. The collateral channel consequently reduces resource misallocation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Table 13

The screening channel by local bank competition.

	High-Tangibility subsample	
	(1) Intense Competition	(2) Unintense Competition
Treat	-0.0039	0.0244**
	(0.0082)	(0.0108)
High-TFP	0.0048	0.0131
	(0.0036)	(0.0132)
Treat × *High-TFP*	-0.0096*	-0.0099
	(0.0049)	(0.0115)
Observations	283146	284627
Adjusted_R^2	0.4957	0.3997

Notes:

This table explores the screening channel by partitioning the high-tangibility subsample. Firms are divided into high-competition and low-competition environments. The estimates reveal how the screening channel varies with the level of competition.

Table 14

The collateral channel by local bank competition.

	(1) Intense Competition	(2) Unintense Competition
Treat	-0.0235**	-0.0002
	(0.0097)	(0.0106)
Low-Tangibility	0.0076	-0.0030
	(0.0150)	(0.0052)
Treat × *Low-Tangibility*	0.0166***	0.0118
	(0.0060)	(0.0101)
Low-TFP	-0.0057*	-0.0080
	(0.0034)	(0.0087)
Treat × *Low-TFP*	0.0119**	0.0120
	(0.0050)	(0.0094)
Low-Tangibility × *Low-TFP*	0.0035	0.0102
	(0.0049)	(0.0082)
Treat × *Low-Tangibility* × *Low-TFP*	-0.0130**	-0.0225**
	(0.0055)	(0.0107)
Observations	566010	568795
Adjusted_R^2	0.4473	0.4151

Notes:

This table evaluates the collateral channel by replicating the core quadruple-difference design across the competitive split. Firms are divided into high-competition and low-competition environments. The estimates show how the collateral channel varies with the level of competition.

Table 15

Robustness checks of mechanisms across alternative specifications.

	(1) Full sample	(2) High-tangibility	(3) Low-tangibility
Baseline			
Treat × *High-TFP*	-0.0043*	-0.0085**	0.0032
	(0.0026)	(0.0041)	(0.0043)
Observations	1134805	567804	567001
Adjusted_R^2	0.4475	0.4598	0.4410
Panel A: Mean-split tangibility subsamples			
Treat × *High-TFP*	-0.0043*	-0.0124***	0.0023
	(0.0026)	(0.0043)	(0.0038)
Observations	1134805	467031	667774
Adjusted_R^2	0.4475	0.4121	0.4576
Panel B: Prefecture-registered firms only			
Treat × *High-TFP*	-0.0054*	-0.0075*	0.0010
	(0.0028)	(0.0039)	(0.0040)
Observations	917374	458727	458647
Adjusted_R^2	0.5099	0.4454	0.7522
Panel C: Top and bottom tangibility quartiles (Col. 2 = top quartile; Col. 3 = bottom quartile)			
Treat × *High-TFP*	-0.0043*	-0.0150**	0.0075
	(0.0026)	(0.0065)	(0.0089)
Observations	1134805	282628	282443
Adjusted_R^2	0.4475	0.4554	0.2714
Panel D: Subsample split based on year prior to policy			
Treat × *High-TFP*	-0.0116**	-0.0182***	-0.0034
	(0.0058)	(0.0056)	(0.0078)
Observations	1134805	35012	1099793
Adjusted_R^2	0.4475	0.6132	0.4405

*Notes: We conduct four robustness checks to validate the identified quantity. First, we replicate the analysis by splitting the sample using the industry-year mean of tangibility rather than the median. Second, we restrict our analysis strictly to firms with prefecture-level registration. Third, we retain only the top and bottom quartiles of the tangibility distribution. This approach curtails potential estimation noise from firms concentrated near the threshold. Fourth, we classify firms into sub-samples based entirely on their characteristics in the year prior to the policy. This pre-treatment sorting eliminates concerns regarding endogenous firm reactions to the reform. Each cell reports the coefficient on Treat × High-TFP with firm and year fixed effects. The empirical patterns remain highly stable. Standard errors clustered at the city level in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.*

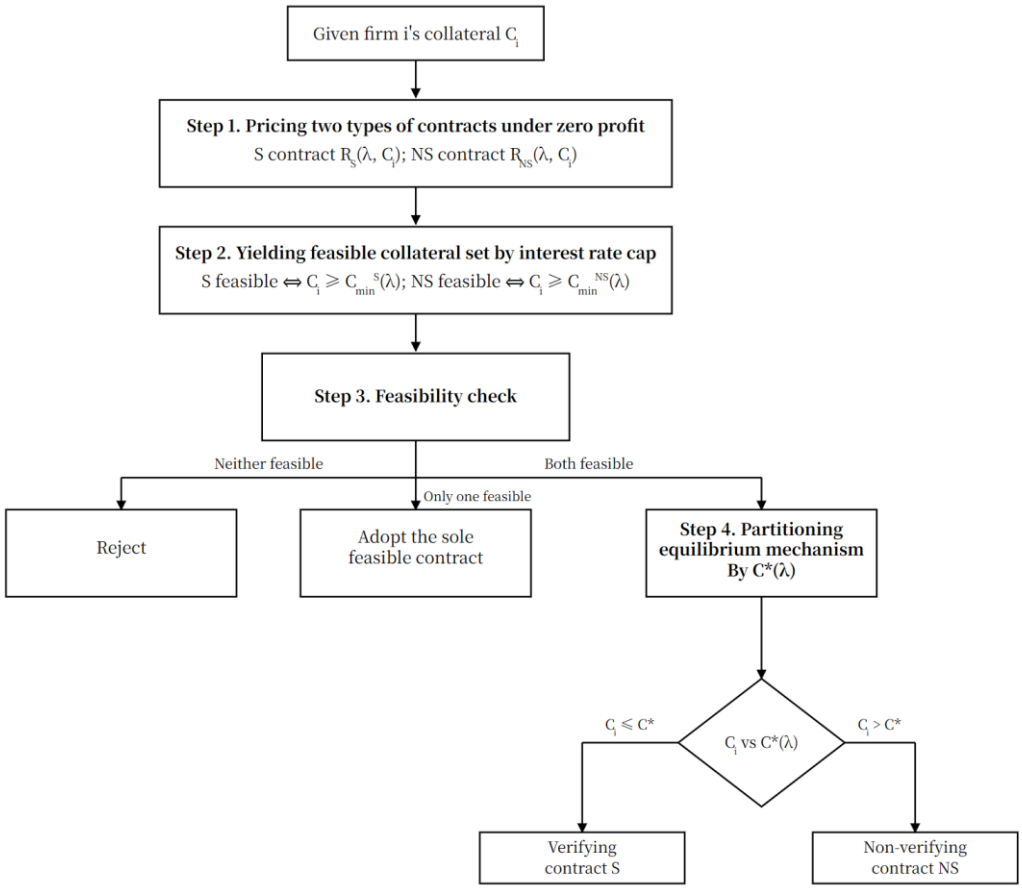


Fig. 1. The bank's contracting decision process and the equilibrium partition of the collateral distribution.

Notes:

This figure illustrates the three-step decision process. For each applicant with collateral C_i*, the bank's contracting problem proceeds in three steps. Bertrand competition first pins down break-even repayment rates under each contract as functions of collateral and recovery. Imposing the interest-rate ceiling then determines the minimum collateral required for each contract to be feasible. Next, the bank compares the collateral provided by the borrower with the minimum collateral requirements for both types of contracts. If the borrower's collateral fails to meet the minimum requirements for any of the contracts, the loan will be denied. If it only exceeds the minimum collateral requirement of one type of contract, the bank will offer that type of contract to the borrower. Finally, when both contracts are feasible, the bank selects the contract that minimizes the high-type repayment rate, trading off the explicit cost of information production against the implicit cost of cross-subsidizing low types under pooling.*

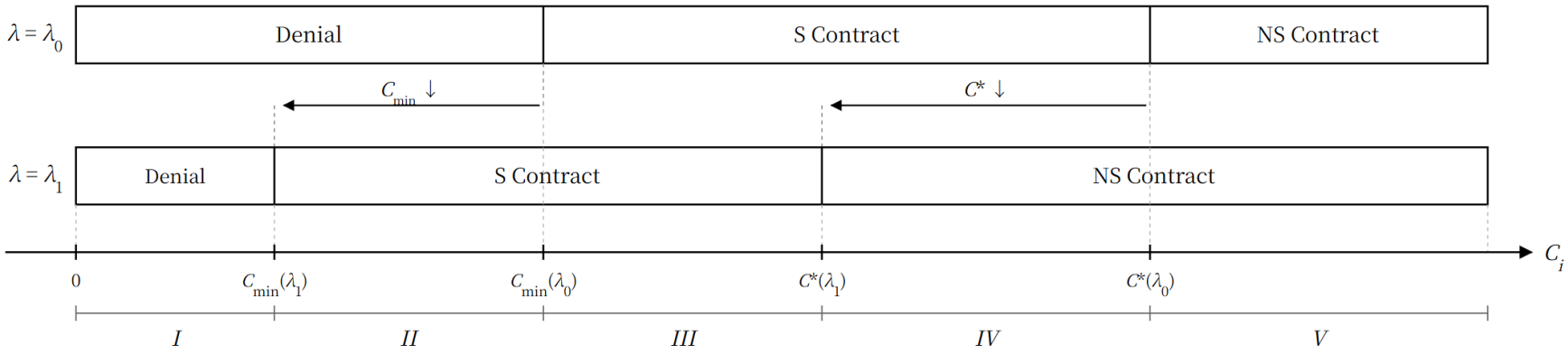


Fig. 2. Impact of an increase in recovery on the equilibrium partition of the collateral distribution.

Notes: An increase in recovery from λ_0 to $\lambda_1 > \lambda_0$ shifts both C^{min} and C^* leftward based on Lemmas 1-2. The corresponding core mechanisms for each region above are shown in Proposition 1-3. The specific theorem content can be found in Section 2.4. We have provided the economic intuition and corresponding empirical testing methods for the impact of bankruptcy court establishment on credit allocation in each channel.

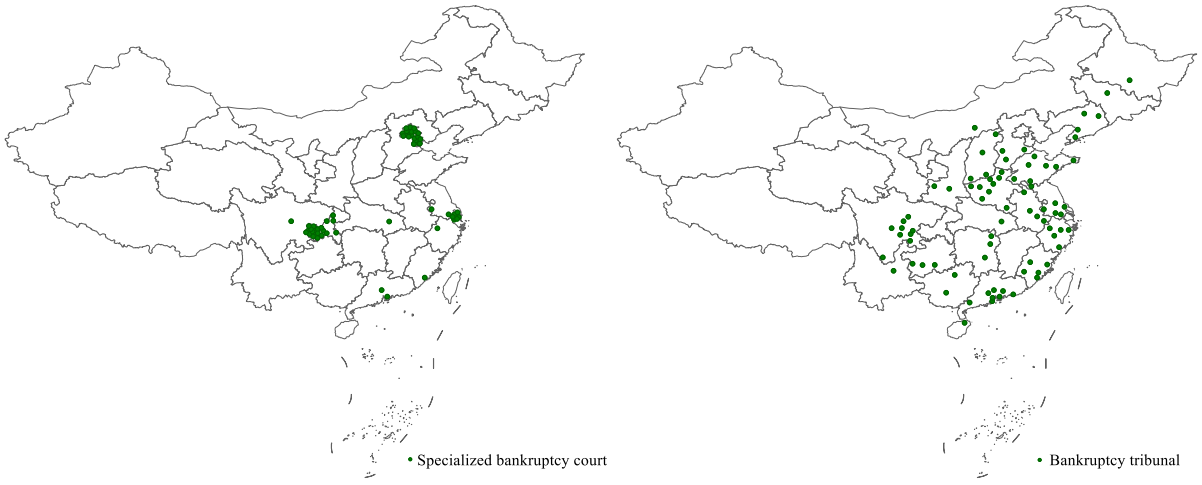


Fig. 3. Spatial distribution of specialized bankruptcy institutions across prefecture-level cities at the end of the sample period.

Notes:

The left identifies cities with a fully independent specialized bankruptcy court. The right identifies cities where the intermediate court houses a specialized bankruptcy tribunal. This map shows the cross-sectional variation in reform adoption by the end of the sample period.

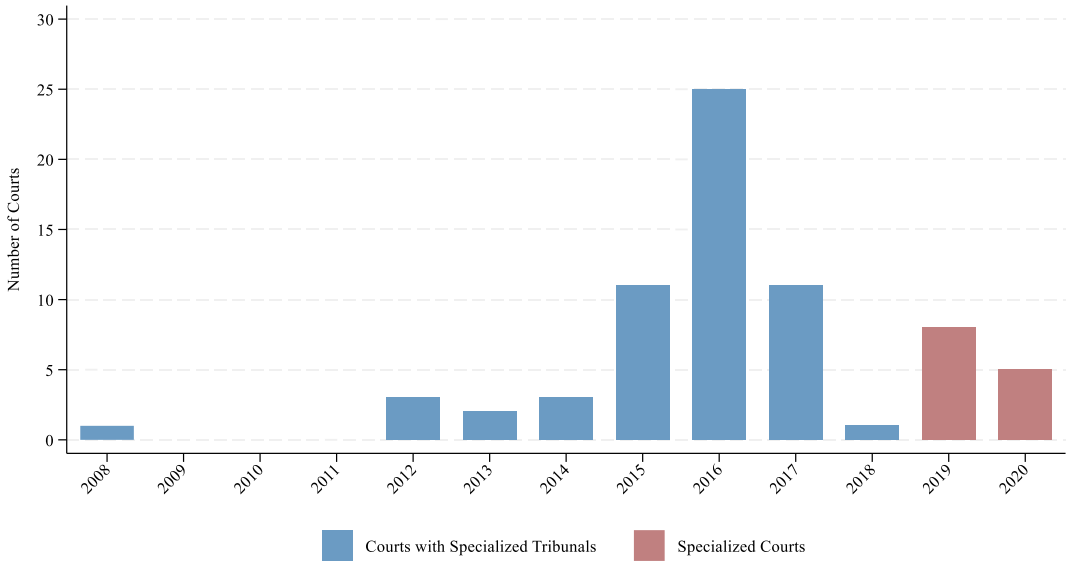


Fig. 4. Annual first-time adoption of specialized bankruptcy institutions across cities.

Notes:

This figure plots the number of cities that first introduced a specialized bankruptcy institution in each year. If a city first established a liquidation and bankruptcy tribunal and later set up an independent specialized bankruptcy court, both are included when we tally the number of specialized bankruptcy institutions establishments in a given year. The staggered rollout of the reform across cities generates the variation in treatment timing that identifies the difference-in-differences design.

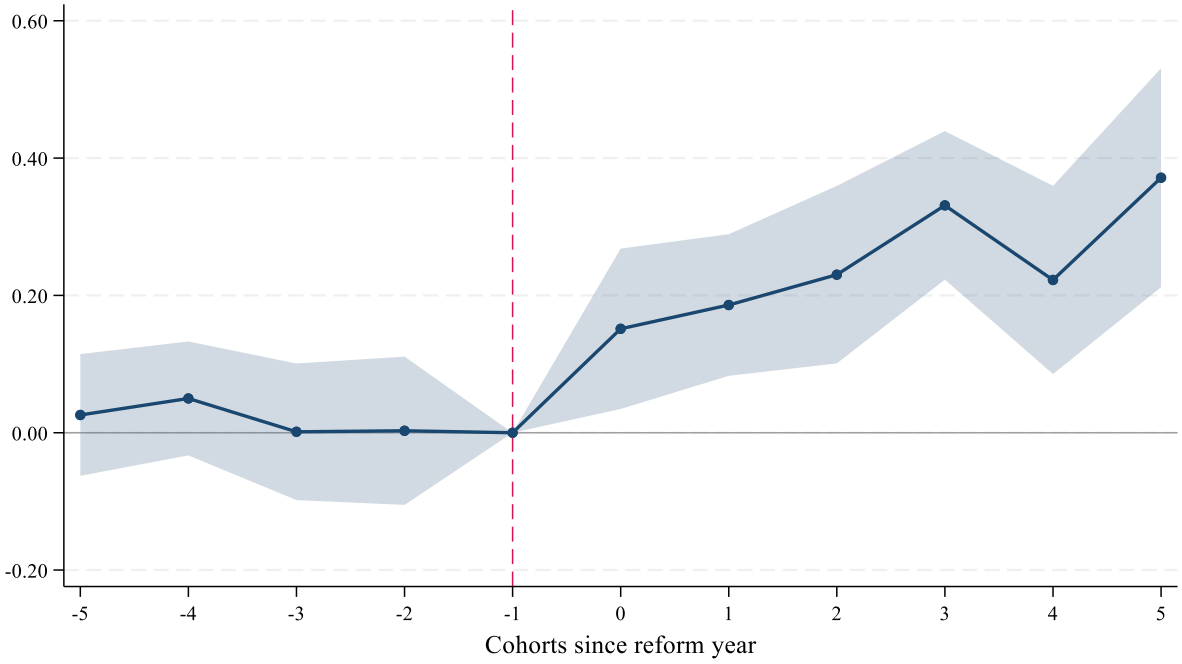


Fig. 5. Dynamic effects of bankruptcy court establishment on capital misallocation.

Notes:

This figure plots the point estimates and the 95% confidence intervals from the dynamic difference-in-differences specification. There is no pre-trend in capital misallocation before the establishment of bankruptcy courts. A significant increase in misallocation emerges in the effective year of the reform. This pattern supports a causal interpretation of the main findings.

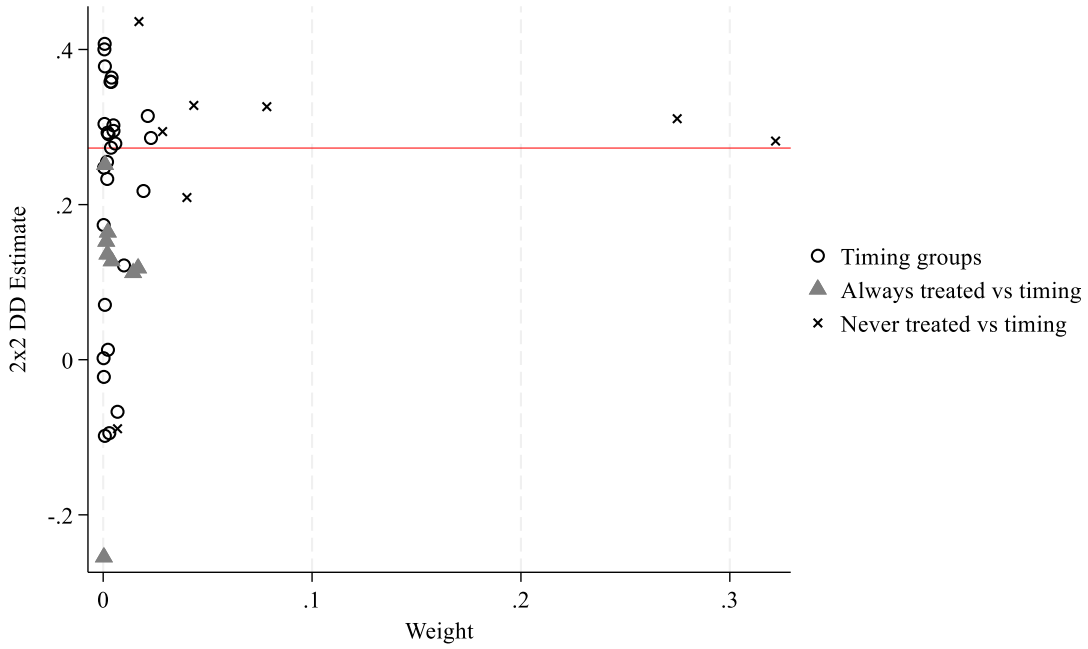


Fig. 6. Goodman-Bacon decomposition.

Notes:

This figure displays the Goodman-Bacon decomposition. The weighted estimate is 0.2729. This value is close to the corresponding two-way fixed-effects coefficient. Comparisons of treated units to never-treated units account for 81.08% of the total weight. Comparisons that use already-treated units as controls receive only a small share of the weight. Bias from heterogeneous treatment effects across cohorts is therefore unlikely to drive the result.

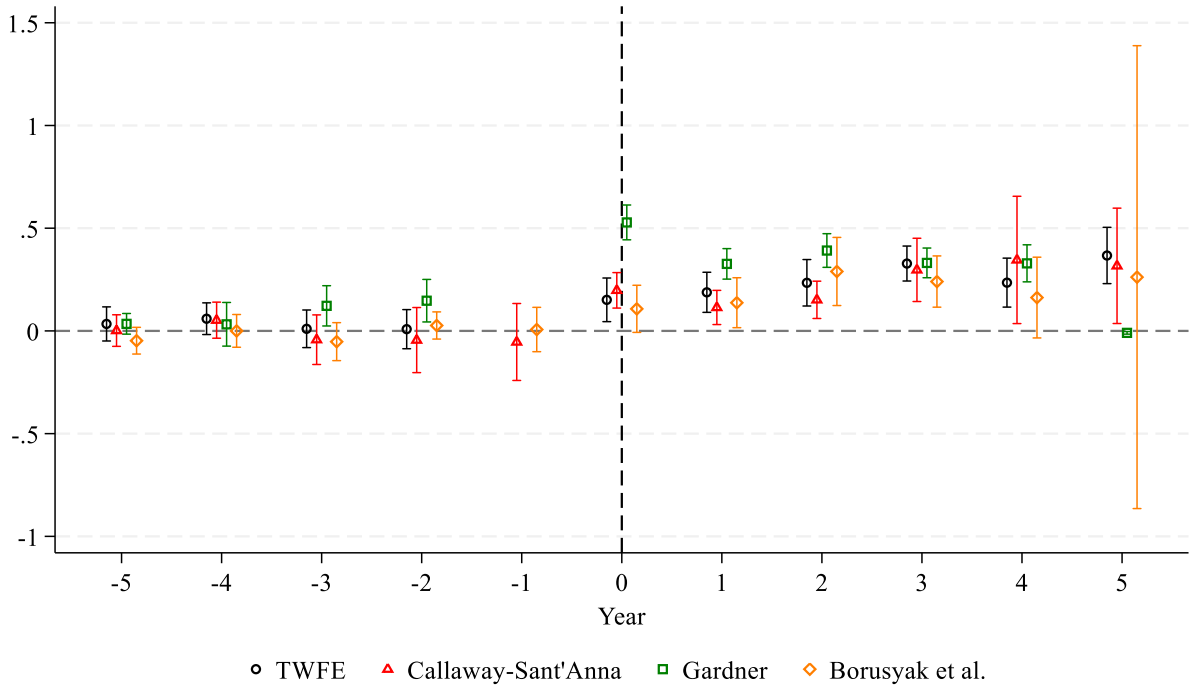


Fig. 7. Dynamic treatment effects estimated with heterogeneity-robust estimators.

Notes:

This figure reports dynamic treatment effects estimated using estimators that are valid under arbitrary treatment-effect heterogeneity. The estimates satisfy the parallel trends assumption prior to the reform. The treatment effect remains positive and significant in the later post-reform periods. These results are consistent with the baseline estimates.



Fig. 8. Misallocation trends for the 2016 and 2017 reform cohorts against synthetic controls.

Notes:

This figure plots misallocation trends for the 2016 and 2017 reform cohorts and their synthetic controls. We take the years 2016 and 2017 as examples, as they were the two years during which the bankruptcy courts were established the most times within the sample period. The average treatment effect on the treated (ATT) is 0.2812. This estimate is significant at the 1% level and closely aligns with the baseline estimate. The adjusted pre-treatment trends are nearly parallel. A clear divergence emerges after the reform. Misallocation rises in the treated cities relative to their counterfactuals.

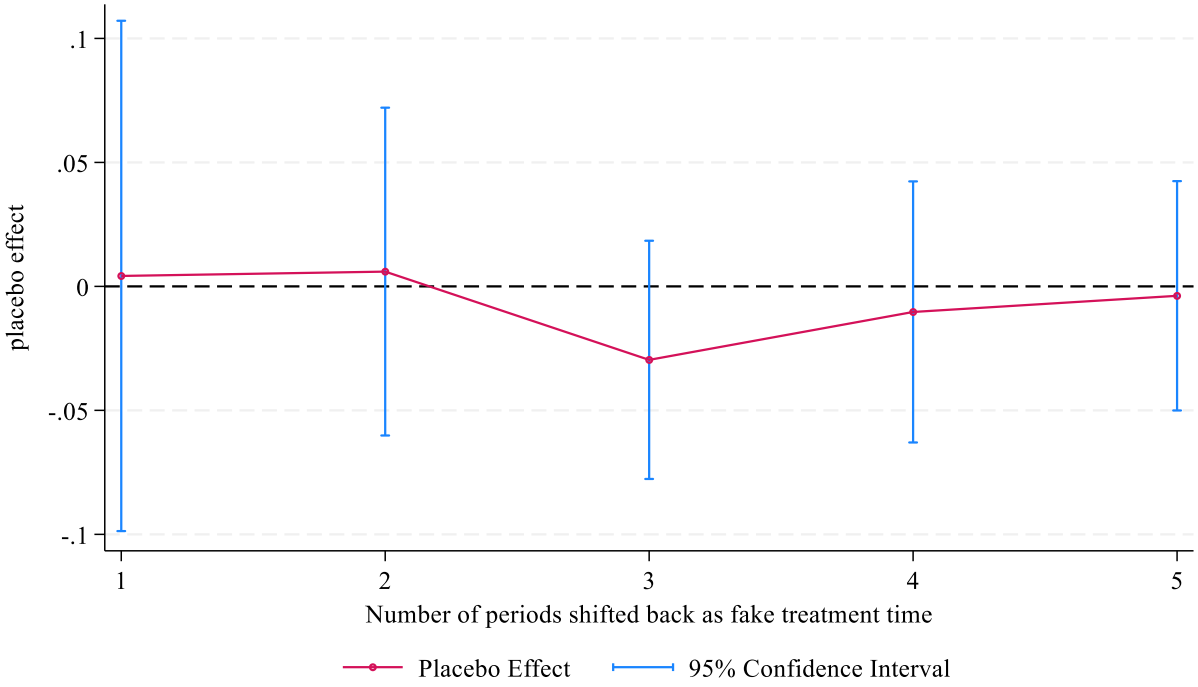


Fig. 9. Placebo test with counterfactual treatment dates.

Notes:

This figure reports the results of re-estimating the baseline model with counterfactual treatment dates. The reform timing is artificially advanced by one year, two years, or even five years. These placebo treatments yield statistically insignificant coefficients. This finding mitigates concerns that the results are driven by other contemporaneous policies.

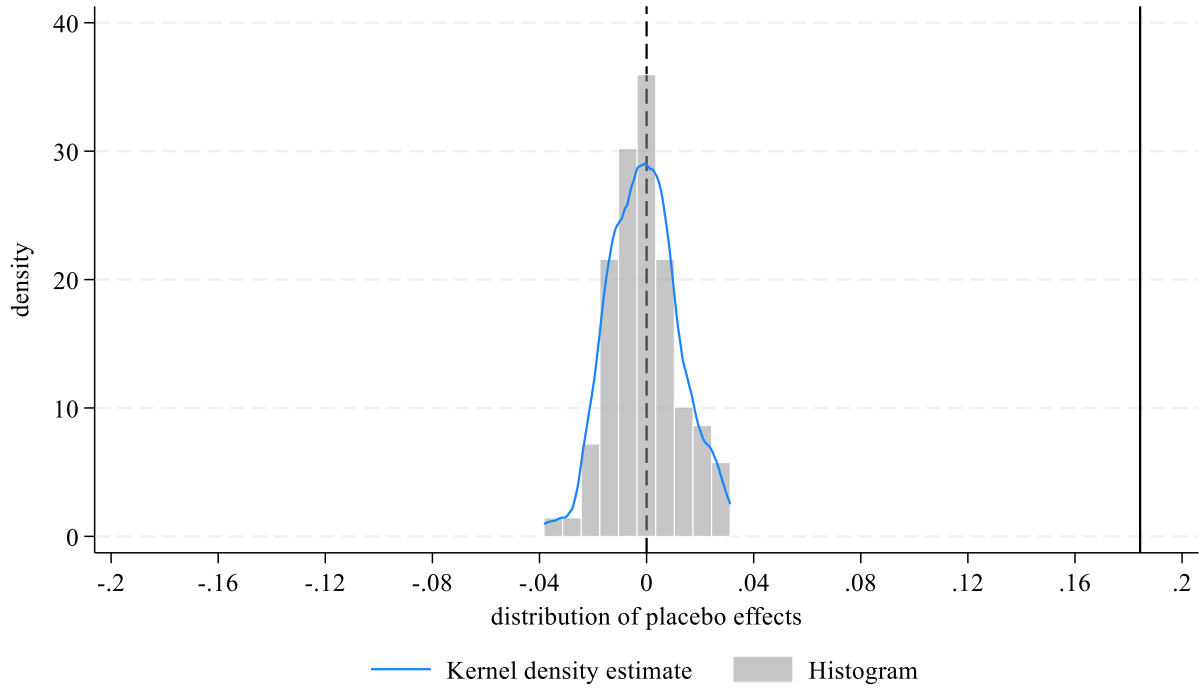


Fig. 10. Distribution of placebo coefficients from randomization inference.

Notes:

This figure plots the distribution of placebo coefficients against the actual baseline estimate. The reform status is randomly reassigned across all cities. The placebo coefficients are centered around zero. The true estimate lies in the extreme right tail. This result confirms that the effect is unlikely to arise by chance.

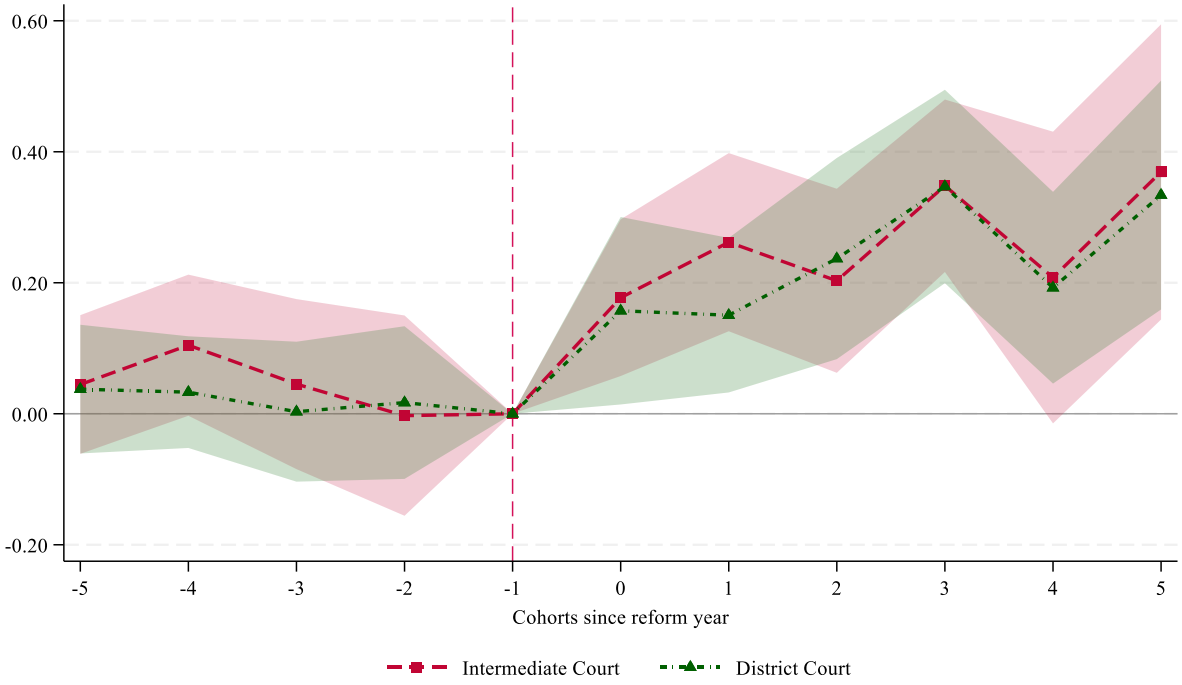


Fig. 11. Dynamic effects of bankruptcy court establishment on capital misallocation by firm jurisdiction.

Notes:

This figure presents separate event studies for two groups of firms to test spillover effect. Firms directly under intermediate-court jurisdiction show an immediate and significant increase in capital misallocation in the reform year. Firms under the jurisdiction of district-level courts show a slight lagged effect. The 95% confidence interval in the implementation year narrowly includes zero. The increase becomes more pronounced in the subsequent year.

Appendix A

1. The roadmap of our estimation procedure

We follow the method proposed by Ackerberg et al. (2015) and De Loecker and Warzynski (2012) to estimate firm-specific productivity shocks, measurement errors and markups.

First, for productivity shocks and measurement errors, we restrict our attention to production functions with a scalar Hicks-neutral productivity term ω_{sit} and common technology parameters β_s across a set of producers within industry *s* as follows,

$$Y_{sit} = F(X_{sit}^1, \ldots, X_{sit}^V, K_{sit}; \beta_s) exp(\omega_{sit}) \tag{A1}$$

where it relies on variable inputs X_{sit} such as labor and intermediate inputs and capital stock K_{sit} for production. For simplicity, we omit the industry subscript *s*, though all estimations are conducted separately by industry.

Next, we account for unanticipated production shocks, such as fluctuations in demand elasticity and income levels. These factors are aggregated into ε_{it}, which can be treated as a composite of productivity shocks and measurement error following De Loecker and Warzynski (2012). Firms do not observe ε_{it} when making their optimal input decisions, the observed log output can be decomposed as follows:

$$\tilde{y}_{it} = lnY_{it} + \varepsilon_{it} = f(x_{it}, k_{it}; \beta) + \omega_{it} + \varepsilon_{it} \tag{A2}$$

By definition, markups μ_{it} can be expressed as the ratio of the labor factor's output elasticity to its income share. Taking labor as an example: under cost minimization, the marginal cost of labor is equal to its marginal benefit, which means:

$$\frac{\partial Y_{it}}{\partial L_{it}} \cdot P_{it} = \mu_{it} \cdot W_{it} \tag{A3}$$

$$\underbrace{\left(\frac{\partial Y_{it}}{\partial L_{it}} \cdot \frac{L_{it}}{Y_{it}}\right)}_{\alpha_{it}^L(\beta)} = \mu_{it} \cdot \underbrace{\frac{W_{it}}{P_{it}} \cdot \frac{L_{it}}{Y_{it}}}_{\theta_{it}^L} \tag{A4}$$

where $\alpha_{it}^L(\beta)$ represents the output elasticity of labor, which is a function of β, the parameters of production function. $\theta_{it}^L = \frac{W_{it}L_{it}}{P_{it}Y_{it}}$ is the expenditure share of labor. This gives the calculation of markups:

$$\mu_{it} = \frac{\alpha_{it}^L}{\theta_{it}^L} \tag{A5}$$

Therefore, by estimating the production function, we get (1) the parameters of production function $\widehat{\beta_s}$; (2) the unanticipated productivity shocks and measurement errors $exp(\hat{\varepsilon}_{it})$ and (3) the heterogeneous markups. The $\widehat{\beta_s}$ will be used to calculate output elasticity of labor $\hat{\alpha}_{it}^L(\hat{\beta})$, and the productivity shocks and measurement errors will be used to get $P_{it}Y_{it} = P_{it}\tilde{Y}_{it}/\, exp(\hat{\varepsilon}_{si})$, which is crucial for calculating θ_{it}^L and markups.

2. The procedure of estimating production function

We estimate the production function following the procedure below.[16] First of all, we present a specific form of the production function in Eq. (A2). For example, a value-added translog production function,

$$\tilde{y}_{it} = \beta_l l_{it} + \beta_k k_{it} + \beta_{ll} l_{it}^2 + \beta_{kk} k_{it}^2 + \beta_{lk} l_{it} k_{it} + \omega_{it} + \varepsilon_{it} \tag{A6}$$

Following Ackerberg et al. (2015), we use material m_t to proxy for productivity ω_{it}, since the choice of material depends on firms' productivity

$$m_{it} = m_t(k_{it}, \omega_{it}, z_{it}) \tag{A7}$$

in which z_{it} denotes additional variables which potentially affect optimal material demand choices. For example, we control for individual and year fixed effects in estimation.

By inverting $m_t(\cdot)$, we can deduce the expression of productivity ω_{it}:

$$\omega_{it} = h_t(k_{it}, m_{it}, z_{it}) \tag{A8}$$

Firstly, we substitute Eq. (A8) into the production function expression and the observed logged output can be written as:

$$\tilde{y}_{it} = \phi_t(l_{it}, k_{it}, m_{it}, z_{it}) + \varepsilon_{it} \tag{A9}$$

Then we try to approximate ϕ_t in some nonparametric forms to estimate Eq. (A9). Here, we adopt the combinations of first and second terms of l_{it}, k_{it}, m_{it}, and z_{it} as well as their all cross-multiplication terms. This gives the consistent estimators of expected output ($\hat{\phi}_{it}$) and demand

[16] For identification assumptions, please refer to Ackerberg et al. (2015) and De Loecker and Warzynski (2012).

shock $\hat{\varepsilon}_{it}$.

With translog production function, the expected output is given by,

$$\hat{\phi}_{it} = \beta_l l_{it} + \beta_k k_{it} + \beta_{ll} l_{it}^2 + \beta_{kk} k_{it}^2 + \beta_{lk} l_{it} k_{it} + h_t(k_{it}, m_{it}, z_{it}) \tag{A10}$$

Next, we give estimates for all parameters in production function by defining the law of motion for productivity:

$$\omega_{it} = \rho \omega_{it-1} + \xi_{it} \tag{A11}$$

Since we regard labor as a flexible input, $E(l_{it}\xi_{it})$ is expected to be nonzero. Thus, our GMM estimates can be obtained,

$$E\left(\xi_{it}(\beta) \begin{pmatrix} l_{it-1} \\ k_{it} \\ l_{it-1}^2 \\ k_{it}^2 \\ l_{it-1}k_{it} \\ \cdots \end{pmatrix} \right) = 0 \tag{A12}$$

where the factor output elasticities can be given for markup measurement:

$$\hat{\alpha}_{it}^L = \hat{\beta}_l + 2\hat{\beta}_{ll} l_{it} + \hat{\beta}_{lk} k_{it} \tag{A13}$$

$$\hat{\alpha}_{it}^K = \hat{\beta}_k + 2\hat{\beta}_{kk} k_{it} + \hat{\beta}_{lk} l_{it} \tag{A14}$$

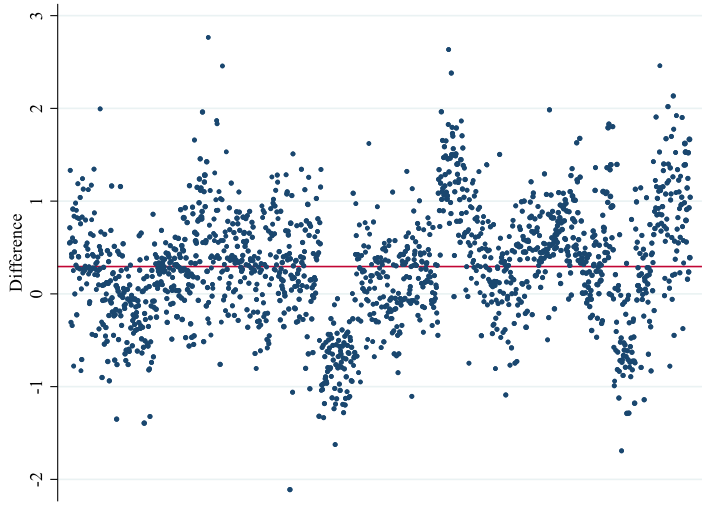
We conduct the above production function estimation by every two-digit industry and similarly exclude the interference of unanticipated productivity shocks and measurement errors to production. The estimation of markups can be obtained as follows:

$$\hat{\mu}_{it} = \frac{\hat{\alpha}_{it}^L}{\hat{\theta}_{it}} \tag{A15}$$

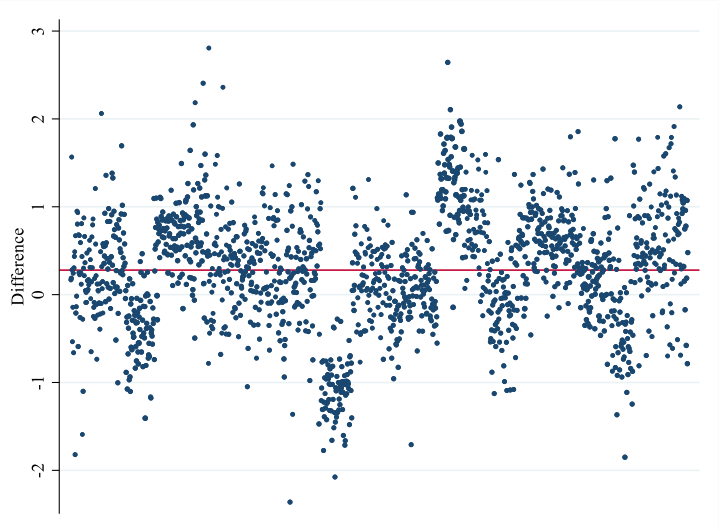
Based on the above estimation results, we can calculate each firm's marginal revenue products of factors and the indicators of misallocation in factor markets following the approach described in Section 3.

Appendix B

The corresponding control-group weights and treatment effects indicate that the SDID weights do not exhibit excessive concentration, while the estimated treatment effects are clustered above zero, with the red reference line marking the average treatment effect. Accordingly, the estimated treatment effects remain consistently positive. Panel (a) reports the results for 2016, and panel (b) reports the results for 2017.



(a)



(b)