Empowering through Courts: Judicial Centralization and Municipal Financing in China

Jiayin Hu, Wenwei Peng, Yang Su*

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Abstract

This study finds that reducing local political influence over courts through judicial centralization weakens local government debt capacity. We establish this result by exploiting the staggered roll-out of a judicial centralization reform in China. We confirm the reform has reduced judicial favoritism towards local governments. The majority of local government lawsuits are with contractors over government payment delays. We show the reform increases government lawsuit losses, discourages their payment delays to contractors, and exposes their credit risk as payment delays without court support signal government financial distress. Investors respond by tightening lending and increasing interest rates, which curbs government spending.

Keywords: judicial centralization, court capture, lawsuit, public finance, information asymmetry, municipal corporate bond
JEL classifications: G12, G30, H70, K40

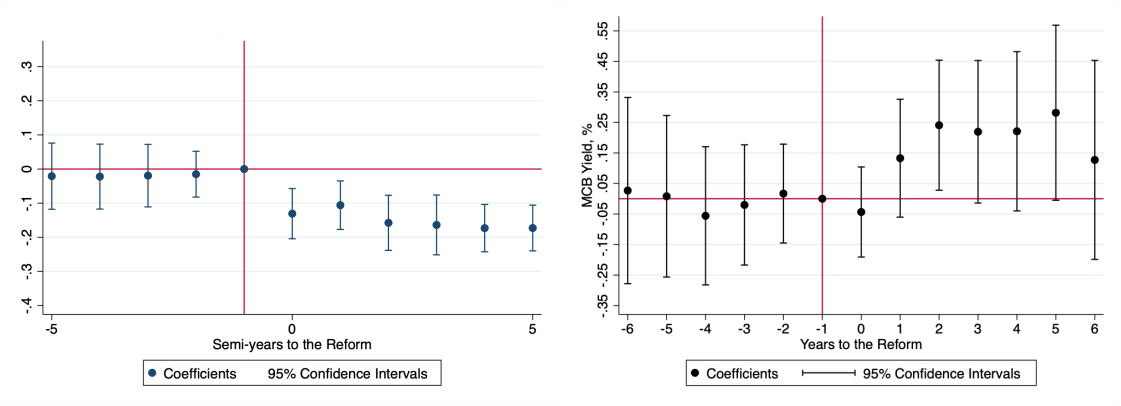
*Hu: Peking University, jyhu@nsd.pku.edu.cn; Peng: Harvard University, wenwei_peng@fas.harvard.edu; and Yang: Chinese University of Hong Kong, yangsu@cuhk.edu.hk. We thank Patrick Bolton, Zefeng Chen (discussant), Haoyu Gao, Ming Gao, Zhiguo He, Yi Huang (discussant), Ruixue Jia, Kai Li, Tony Baixiao Liu, Fangyuan Ma, Linlin Ma, Jacopo Ponticelli (discussant), Zheng (Michael) Song, Kedi Wang, Shaoda Wang, Xiaoyan Zhang, Naide Ye, Bernard Yeung, and participants at seminars in Peking University HSBC Business School and School of Economics, ABFER Capital Market Development: China and Aisa Webinar, the China Financial Research Conference (CFRC 2024) at Tsinghua PBCSF, the 17th Chinese Economy Summer Institute at Peking University, CUHK Business School Brown Bag Seminar, the Sixth Annual Conference of Government and Economics at Tsinghua University, and the 2024 CIRF-CFRI Joint Conference for their helpful comments. Yanting Chen, Haoting Dong, Ruoyao Liu, and Yilin Wu provided excellent research assistance. All errors are our own.

1 Introduction

The relationship between central and local governments has been a classic topic in the research of political economy and public finance, especially for transition economies (Qian and Weingast, 1997).[1] One renowned strand of literature has examined the issue of fiscal (de)centralization (e.g., Qian and Roland, 1998; Zhuravskaya, 2000; Enikolopov and Zhuravskaya, 2007) and the impact on fiscal capacity and state building (Cantoni et al., 2024). Another important issue is judicial (de)centralization. A centralized judicial system can safeguard the independence of local courts from local political interference, which is crucial for the functioning of the judicial system in addition to its efficiency and specialization (e.g., Lambert-Mogiliansky et al., 2007; Ponticelli and Alencar, 2016; Li and Ponticelli, 2022; Araujo et al., 2023). While some research has examined the economic consequences of local judicial independence (Feld and Voigt, 2003; Voigt et al., 2015; Liu et al., 2022), few studies have investigated the impact of local judicial independence on public finance.

This paper fills the gap by analyzing the impact of judicial centralization on local governments' borrowing capacity. The independence of high-level courts, which enjoys substantial power in judicial interpretation, is predicted to improve government or sovereign debt capacity because an independent judiciary can enforce governmental compliance with debt contract obligations (Dove, 2018). Interestingly, we find opposite effect of judicial independence at the grassroots level – local governments are empowered to increase their debt capacity through locally captured courts. Figure 1 shows the event study results based on a reform that centralizes local courts' budgetary and personnel management in China. We confirm the effectiveness of the reform in reducing court favoritism toward local governments, as reflected by the decrease in local governments' win rates in lawsuits post-reform. Notably, the majority of these lawsuits are against contractors and suppliers rather than creditors. We find that as the reform benefits these contractors and suppliers, it has increased local governments' borrowing costs as measured by their bond yields. Moreover, local governments also experience an increase in debt default and a decrease in borrowing and spending.

[1] An efficient hierarchical allocation of power hinges on the delicate balance between incentivizing local governments to develop the economy and containing bailout costs associated with soft budget constraints (Kornai, 1986; Kornai et al., 2003; Maskin, 1999).



(a) Local Government Win Rates (b) Municipal Corporate Bond Yield, %

Figure 1: Local Court Capture and Municipal Financing Cost

Note: This figure plots the 95% confidence interval of the effect of the judicial centralization reform on local government win rates in commercial lawsuits and their bond yield. Standard errors are clustered by cities.

To the best of our knowledge, this paper provides the first empirical evidence about the effect of judicial independence on government debt capacity at the grassroots level. To explain why the prediction regarding high-level court independence does not apply for grassroots-level courts, we conjecture that unlike high-level courts, grassroots-level courts have limited power in judicial interpretation and cannot affect government compliance with debt contract obligations. In contrast to simple and standard debt contracts, procurement contracts are often intricate and incomplete, allowing for significant court discretion in their rulings. This is probably why legal lawsuits of local governments are much more common with contractors and suppliers than with creditors. It is contractors and suppliers that are directly subject to local court protectionism, with creditors indirectly affected.

We propose three mechanisms to explain the effect of the reform on local government debt capacity. Alleviating local court protectionism increases local governments' lawsuit losses with business partners, discourages them from delaying payment to business partners and holding the money to pay creditors, and produces information about LGFVs' credit risk. The last mechanism occurs because most of these lawsuits revolve around government payment delays and those that delay payment with defense not supported by the court will then be perceived by the creditors to be liquidity constrained. Consistent with the in-

formation production mechanism, we find that in lawsuits around government payment delays, their credit rating is more likely to be downgraded following a defeat, especially in less fiscal transparent cities. The adverse effect of judicial independence disappears for local governments without lawsuits around their payment delays, in which case the court information production role is muted.

China provides an ideal setting for studying the effect of local judicial independence on municipal debt capacity.[2] First, given China's high level of economic decentralization (Xu, 2011), the judicial system used to be decentralized and judicial local protectionism in commercial lawsuits used to be ubiquitous (Gong, 2004).[3] Since 2014, the Supreme People's Court of China (SPC) has been gradually rolling out a high-profile judicial centralization reform that aims to alleviate local court capture by moving the financial and personnel controls over local courts from prefecture-level city governments to provincial governments. This reform has been widely recognized by judges, legal scholars, and economists as having significantly transformed China's judicial system (Zhang and Ginsburg, 2019; Liu et al., 2022). The reform is rolled out in a staggered way and provides an opportunity for us to study the impact of local court capture. Second, since the Great Financial Crisis of 2007-2008, local governments have increasingly expanded their debt usage via entities known as Local Government Financing Vehicles (LGFVs) through the issuance of municipal corporate bonds (MCBs), bank loans, and other financing channels (Chen et al., 2020). With the rapid rise of local government debt, the fear of government default risks has intensified, and a few LGFVs have recently defaulted on debt with creditors such as trust companies.[4]

We build a unique dataset combining several distinct data sources to draw a comprehensive picture of the effects of alleviating local court capture on municipal debt capacity. We start from the universe of MCBs issued by LGFVs until 2023. For all these bond-issuing LGFVs, we download their financial statements between 2010 and 2023 from WIND (a leading financial database in China) and all court verdicts in which these LGFVs were either plaintiffs or defendants from the official court filing disclosure website. In total, 2,144 out of the 3,201 LGFVs are matched

[2]Following the convention, we refer to the prefecture-level city governments, also known as municipal governments, as local governments in this paper.

[3]The degree of political capture of judiciaries is more common in developing economies and civil law countries than in other countries (Glaeser and Shleifer, 2002; Porta et al., 2008).

[4]See several reports from Financial Times, Wall Street Journal and Reuters.

to at least one court verdict, and the average number of verdicts involving these matched LGFVs is 33. This number is much higher than the average number of verdicts involving non-LGFV firms, which is roughly 0.1, suggesting important roles of courts in the operation of LGFVs. Next, we merge these LGFVs with a sample of value-added tax (VAT) invoices from a private data vendor, which covers the universe of VAT invoices of 849 contractors and suppliers between 2016 and 2023. In total, 1,475 of the 3,201 LGFVs are matched with at least one VAT invoice. Finally, we obtain default and rating downgrading events of LGFVs between 2017 and 2023 from the Enterprise Alert System, a firm monitoring database widely used by financial institutions and government think tanks in China.

In the first part of the paper, we show that the judicial centralization reform has successfully mitigated court favoritism towards LGFVs. By exploiting the staggered roll-out of the reform using difference-in-differences (DID) estimation and controlling for heterogeneous treatment effect following Sun and Abraham (2021), we find that following the reform in the LGFVs' cities, the win rate of LGFVs decreased by 17.2% when against non-local plaintiffs and 11.6% when against local plaintiffs. In comparison, LGFVs' average win rate is 56.6% as plaintiffs and 50.1% as defendants in our data. The magnitude of the effect is greater for non-local than for local counterparties, consistent with previous findings that non-local firms suffered from additional local protectionism bias before the reform (Liu et al., 2022). We also find that the decline in LGFVs' win rates is quantitatively larger when the lawsuits are against contractors with a larger contract value. In the appendix, we conduct two placebo tests to show that the results are not driven by some economic shocks correlated with the timing of the reform. We also find that on the extensive margin, the reform has encouraged less powerful plaintiffs to file lawsuits against LGFVs, and on the intensive margin, the effect after controlling for case composition is quantitatively larger than the baseline estimates.

In the data, more than 93.7% of the lawsuits involving LGFVs are filed by or against their business partners, including contractors and intermediate input suppliers. We can think of the effect of the reform as increasing transfers from LGFVs to their business partners in case of lawsuits. We find that the increased lawsuit losses of LGFVs post the reform can be partially mitigated by lower prices charged by contractors and suppliers ex ante. Specifically, we use the value-added tax (VAT) invoice data and find that after the reform, the prices paid by LGFVs

relative to non-LGFVs for the same product from the same seller in the same year decrease significantly by approximately 0.1%. Moreover, LGFVs purchase more from external contractors and suppliers by about 3.66%. This is because external contractors and suppliers suffer from additional local protectionism bias than local ones and hence benefit more from the reform. These results further support our argument that the reform reduces court bias in favor of local governments.

In the second part of the paper, we investigate how the reform has affected the default risk and debt capacity of LGFVs. There is barely any direct effect as very few lawsuits are between LGFVs and creditors (e.g., banks and bond investors). One concern for interpreting the effect of the reform on municipal financing as causal is reverse causality, i.e., the spatial variation of the reform timing may result from the local governments' financial conditions in the first place. In particular, local governments with more financial constraints may be more willing to conduct the reform to save the money allocated to local courts. However, this is unlikely to be true because court expenditures constitute a negligible part of local governments' total expenditures. In 2014 and 2021, total local court and procuratorate expenditure is 1.4% of total city government budgetary expenditure. Moreover, there is no significant correlation between the cities' reform status by 2020 and all the outcome variables related to local governments' financial conditions and examined in this paper in 2013. We find no evidence that the reform reflects decreased political support from upper-level governments either.

We first present the adverse effect of the reform on LGFVs' debt capacity and default rate. We first look at municipal corporate bonds (MCBs) for which we have detailed information on the issuance, maturity, and yields. We aggregate all the MCBs to construct the annual net issuance and average yield of MCBs for each city in each year and scale the net issuance by the city government's fiscal revenue in 2013, the year before the reform. We find that reform and control cities experienced parallel trends in the net MCB issuance and average MCB yield before the reform. After the implementation of the reform, the net MCB issuance decreases significantly in reform cities as compared to non-reform cities, and the magnitude of the effect stabilizes at approximately 15% in the third year following the reform. Meanwhile, the average MCB yield increases significantly and stabilizes at approximately 0.25% since the second year following the reform. The decrease in net issuance and increase in yields suggest that the judicial reform

has weakened local government debt capacity.

As the issuance of MCBs is only part of the LGFVs' financing sources, we then use LGFVs' financial statements to study the overall impact of the reform at the firm level. We find that after the judicial reform, LGFVs' total assets decrease by about 9.34% on average, and the effect keeps increasing at least five years after the reform. Their average borrowing rate also increases by 0.33%, similar to the effect on MCB yields, suggesting that other credit sources, such as bank loans, are also adversely affected by the judicial reform. Unlike other credit sources, trade credit, measured by accounts payable over cost-of-goods-sold, increases by approximately 2.37%. The increase in trade credit is likely a joint outcome of LGFVs' tightened financial constraints and a fairer legal environment that makes business partners more willing to extend trade credit.

Regarding LGFVs' debt default, despite no formal default of MCBs by the end of 2023, LGFVs have defaulted on non-standard debt obligations, such as trust products, in recent years. Using firm-level DID estimation, we find that the reform has increased LGFVs' default rate by about 0.51%, which is both statistically significant and economically important compared to the mean of 0.3%.

We propose three channels through which the reform affects municipal financing. All channels highlight the fact that it is the contractors and suppliers that are directly affected by the reform, with creditors indirectly affected. First, the reform increases lawsuit losses incurred by the LGFVs. Lawsuits often involve some cost that the two parties disagree on how to split. The reform increases the share borne by LGFVs, leaving them with less resource to meet their debt obligations.

Second, the reform decreases the de facto seniority of creditors. Many of LGFVs' lawsuits revolve around their payment delays with business partners, while very few with creditors. LGFVs may have opted to delay payment to less powerful business partners so as to meet debt obligations with more powerful creditors. The reform increases the legal constraint of such payment delays, which decreases creditors' seniority and makes them unwilling to extend credit ex ante. We find that after the reform, not only is the court more likely to refute the LGFVs' defense for payment delays and force them to pay immediately, but the LGFVs are also less likely to delay payment in the first places.

Lastly, and most importantly, is the information production role of the court. Court rulings contain valuable firm information and alert to firms' lawsuits is

highlighted on many popular websites widely used by financial analysts. The most common lawsuits of LGFVs revolve around their payment delays with business partners, which can stem from either liquidity constraint of LGFVs or misconduct of contractors and suppliers. When the LGFVs' defense for payment delays is not supported by the court, investors will perceive the LGFVs as more likely to be liquidity constrained and thereby cut lending to these LGFVs. This negative effect of information disclosure on credit supply is known as the Hirshleifer effect (Hirshleifer, 1971).

As direct evidence for the information production role of the court, we find that LGFVs are more likely to experience credit rating downgrading within one year after losing lawsuits featuring their payment delays. The relationship holds for both OLS and IV estimation when we instrument the LGFV's win rate using the city's judicial reform status. The IV estimation result means that a defeat in lawsuit induced by the reform, holding information collection from other sources constant, can significantly trigger credit rating downgrading. In terms of the magnitude of the effect, the reform has increased the probability of downgrading by 2.38%, which is economically important compared to the mean of 2.1%. Moreover, the effect does not hold for cases that do not involve LGFV payment delays as outcomes of such cases cannot inform the LGFVs' financial conditions. We also find the effect to be much stronger in cities with less self information disclosure based on two measures of the city government fiscal transparency. Furthermore, when we mute the court information production by selecting jurisdictions without any LGFV lawsuits around their payment delays, we do not find any significant effect of the reform on the LGFVs' borrowing and spending.

With the information production role of the court, the reform reduces municipal debt capacity by shifting more LGFVs from the lawsuit-winning to the lawsuit-losing pool, and the difficulty in debt rollover will contribute to more debt default. We highlight that the reform does not necessarily improve court information production as the marginally affected LGFVs also include those that are not liquidity constrained. It is thereby unclear how the average credit risk of LGFVs conditional on winning or losing lawsuits changes after the reform.

Finally, we show that the reform has real impacts by weakening LGFVs' debt capacity. We find that after the reform, LGFVs' total spending on goods and services, fixed asset, intangible and other long-term asset decreases by 22.2%.

The decrease in spending has undermined LGFVs' production of residential land inventories for local governments to sell. We find that after the judicial reform, the annual residential land supply at the city level decreases by approximately 22.8 square meters per 100 urban residents, compared to the mean of 147.2 square meters per 100 urban residents. Average residential land prices also drops by about 10%. The simultaneous reduction in quantity and price suggests that with reduced spending induced by the reform, LGFVs not only cut the production of land inventories but also reduce investment on facilities for each land parcel and lower land quality. The decrease in residential land sale may have, in turn, further worsened the LGFVs' financial conditions because land sale is an important source of revenue for LGFVs.

Literature Review. This paper is related to four strands of literature. First and foremost, we highlight the critical role of local judicial independence under the centralized judiciary system, which is relatively understudied compared to other characteristics of a well-functioning judicial system such as efficiency (Ponticelli and Alencar, 2016) and specialization (Li and Ponticelli, 2022; Hotchkiss et al., 2022). Although an extensive literature has argued the economic advantages of an independent judiciary (Glaeser and Shleifer, 2002; Porta et al., 2008; Acemoglu and Robinson, 2013), few rigorous studies examine how judicial independence affects judicial outcomes and shapes the behaviors of firms and governments, especially at the local court level. Lambert-Mogiliansky et al. (2007) find that regional political characteristics affected judicial decisions regarding bankruptcy proceedings, suggesting that politically strong governors subverted enforcement of the bankruptcy law. Most related to this paper is Liu et al. (2022), who exploit the same judicial reform in China and find that alleviating court capture reduces protectionism against nonlocal firms and encourage external investment.[5] Rather than studying private-sector disputes, our paper focuses lawsuits of local governments against all counterparties, regardless local or external, and the subsequent effect on local government debt capacity.

Second, we provide novel empirical evidence of the nexus between the legal

[5]Some earlier research includes Tabarrok and Helland (1999), which uses data from the United States and find that compared to appointed judges, elected state judges favor in-state plaintiffs by issuing larger trial awards to redistribute wealth from out-of-state businesses to in-state plaintiffs. Mehmood (2022) shows that in Pakistan, the change in the selection procedure of judges from presidential to peer appointment reduces the number of pro-governmental rulings.

system and municipal financing. In the United States, elected judges are typically believed to be more subject to local popular wills and less independent than appointed judges. Dove (2018) finds a negative correlation between judges being appointed, as opposed to elected, and municipal defaults. Dove (2017) finds that across different states, the extent of judiciary independence is positively correlated with bond ratings and negatively correlated with municipal borrowing costs. Using municipal financial data from 2009 to 2016, Mughan (2021) finds evidence suggesting that cities use municipal courts to fund governments by increasing fines and fee revenues. Our paper differs from previous studies in two ways. First, we provide micro-level evidence on how court capture affects rulings involving local governments. Second, we introduce both business partners and creditors to the analysis and highlight different mechanisms from previous studies.

Third, the paper offers new insights to the vast literature on debt capacity and borrowing constraints by emphasizing the real and informational impact of judicial decisions. In our paper, the judicial reform plays three roles in affecting local government debt capacity: lower profitability on LGFVs' balance sheets, less seniority of creditors over business partners, and more information production of LGFVs' fundamentals. The first echoes Bernanke and Gertler (1989) in showing the financial accelerator role played by firms' net worth. The second links to the literature on creditor conflicts and financial constraints (Bao and Hou, 2017). The last relates to the classic concern over the adverse impact of information disclosure, as indicated in Hirshleifer (1971). While previous studies mainly focus on the disclosure of bank-specific information in the context of financial crises (Allen and Gale, 2000) and stress tests (e.g., Bouvard et al., 2015; Faria-E-Castro et al., 2017; Goldstein and Leitner, 2018; Dai et al., 2024), we apply the Hirshleifer effect to the local court context.

Finally, we contribute to the literature on municipal financing, particularly the local government financing in China. Chen et al. (2020) show that the rise of the municipal corporate bond market in China can be linked to the four-trillion RMB fiscal stimulus following the global financial crisis (Acharya et al., 2024). As local government debt has soared since 2010, concerns about local government default risk (Gao et al., 2021) and potential knock-on effects on credit access in the private sector (Becker and Ivashina, 2018; Demirci et al., 2019; Huang et al., 2020; Zhang et al., 2022), innovation and growth (Croce et al., 2019), land zoning (He et al., 2023),

factor allocation (Wu et al., 2018), and liquidity squeeze of government contractors (Hu et al., 2022) have been increasing. Most related to our paper is Gao et al. (2021), who show that local governments in China selectively default on banks with weaker political power. Our paper echoes Gao et al. (2021) by demonstrating local governments' selective defaults on counterparties with less power and more incomplete contracts (e.g., contractors and trust companies). We emphasize that judicial reforms, by reducing local governments' ability to intervene, facilitates more information production about government credit risk and enhances the market forces to discipline government debt issuance.

The rest of the paper is organized as follows. Section 2 introduces the institutional background. Section 3 describes our data. Section 4 documents the effect of the judicial reform on the LGFVs' lawsuit outcomes. Section 5 presents the effect on local government debt capacity and spending. Section 6 concludes.

2 Institutional Background

2.1 Judicial Centralization Reform

Before 2013, China had a decentralized judicial system where local governments managed courts and procuratorates within their jurisdiction. While a decentralized fiscal system can ensure local government incentive to develop local economy, there is no obvious economic rationale for a decentralized judicial system. The personnel and budgetary reliance on local governments weakens judicial independence, making court decisions prone to interference by local officials.

In November 2013, China's top-level authority outlined a comprehensive reform at the Third Plenary Session of the 18th Central Committee of the Communist Party of China (CPC). One objective of this reform is to ensure the independence and impartiality of the judicial system. Specifically, the CPC Central Committee calls for reforming the judicial system by "promoting the unified management of personnel and budgets in local courts and procuratorates below the provincial level and exploring establishing a judicial jurisdiction system that is appropriately separated from administrative divisions."[6] The reform has two components: personnel and

[6]See The Decision of the Central Committee of the Communist Party of China on Several Major Issues Concerning Comprehensively Deepening Reform, approved by the Third Plenary Session of

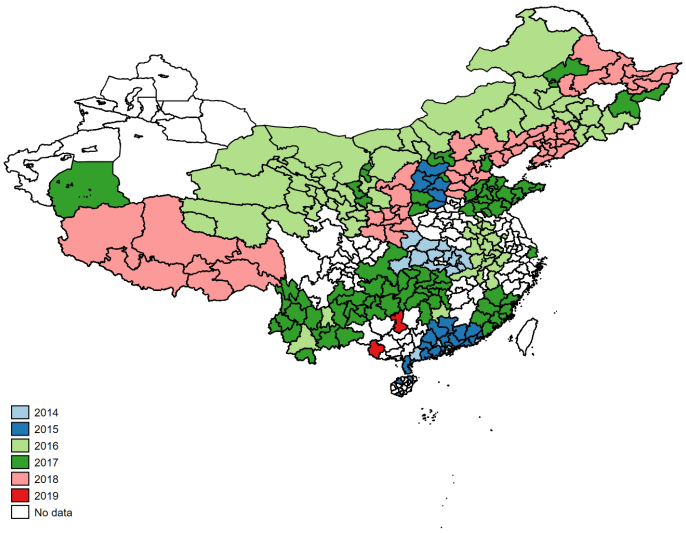


Figure 2: Timeline of the Reform

Note: In this figure, we plot the year of the judicial centralization reform across different cities. "No data" means that by the end of 2020, the city has not finished the reform yet.

budgetary. The personnel reform requires the upper-level provincial governments to manage the appointment of local judges and procurators, and the budgetary reform aims to secure funds for local courts and procuratorates from, again, provincial governments. It should be emphasized that the judicial reform does not entirely insulate judicial decisions from political influence; rather, it centralizes judicial authority at higher government levels. The reform is part of the increased centralization efforts since 2013 to rectify China's formerly fragmented mode of governance.

The reform has been steadily rolled out since June 2014. The personnel reform and the budgetary reform usually occur simultaneously, although in a few cities, one reform can precede the other by as long as one year. Following Liu et al. (2022), we pick the earliest date when either of the two reforms is finished as the treatment time. Figure 2 shows the reform years for different cities until the end of 2020. There are 17 cities that finished the reform in 2014, followed by 27 cities in 2015, 65 cities in 2016, 77 cities in 2017, 50 cities in 2018, and 2 cities in 2019. By the end of 2020, 98 out of the 336 cities had not finished the reform. We discuss the determinants of the reform timing in Section 5.1.

the 18th Central Committee of the Communist Party of China, November 12, 2013.

2.2 Local Government Debt in China

The Budget Law of the People's Republic of China was promulgated in 1994, which prohibited local governments from financing fiscal deficits through bank loans or bonds. Lacking the ability to use debt, many local governments relied on transfers from the central government to finance their expenditures.

The tension between local government spending and financing restrictions heightened in 2008. In response to the global financial crisis, the Chinese government rolled out a 4 trillion fiscal stimulus plan to stabilize the economy, requiring local governments to provide the majority of the funding (Acharya et al., 2024). To finance this stimulus plan without violating the Budget Law, the central government allowed local governments to establish legal entities known as Local Government Financing Vehicles (LGFVs), which can obtain funds through bonds, banks loans and other credit sources to finance public investment projects. The size of local government debt took off ever since (Bai et al., 2016; Chen et al., 2018).

LGFVs are essentially state-owned enterprises that raise funding for local governments from the financial markets. Different from typical municipal bonds, bonds issued by LGFVs are de jure corporate bonds but are backed by local governments, either explicitly or implicitly. Therefore, these bonds are commonly referred to as municipal corporate bonds (MCBs) (e.g., Chen et al., 2020).

In 2015, the Budget Law was revised and authorized local governments to issue municipal bonds, with the intention to wind down the rapid growth of the informal government financing through LGFVs. However, the outstanding debt of LGFVs kept growing after 2015 and the fear for default risks intensified in recent years. In December 2023, Moody's downgraded the credit rating outlook of 22 LGFVs from stable to negative, reflecting growing concerns over "local governments' ability to deal with mounting hidden debt amid a protracted property slump."[7]

2.3 Judicial System and Municipal Finance: The U.S. Experiences

The interaction between judicial independence and municipal financing has drawn much attention in the history of the U.S. state judiciary system. Early state courts in the United States were typically selected through legislative appointment. Due to concerns regarding undue influence from a state legislature and outright judiciary

[7] See the Wall Street Journal report.

corruption, many states shifted toward popular elections since the 1840s. New York set a precedent in 1846, and by 2021, approximately three-fourths of the states selected their judges through elections. Nonetheless, anecdotal evidence suggests that popularly elected state judiciaries are influenced by popular wills when addressing local public indebtedness.

One illustrating example is from Iowa. In 1850, the Iowa state legislature passed legislation granting county indebtedness with the support from the state Supreme Court. While municipal indebtedness grew as a result, the panic of 1857 severely crippled the industries within Iowa. Meanwhile, the state Supreme Court was transformed into a popularly elected body in 1858. Faced with growing popular agitation against debt obligations, the newly elected Iowa Supreme Court nullified all outstanding local debt. The Iowa court's decision had a snowball effect, with the high courts of Wisconsin, Michigan, and Missouri overturning prior decisions favoring creditors. These overturns led to significant federal litigation. Fairman (1971) indicates that the United States Supreme Court heard over 200 municipal bond cases throughout the latter half of the nineteenth century, most of which came from states with elected state courts.[8]

In the example above, the state Supreme Court has substantial power in judiciary interpretation and hence the authority to nullify outstanding debt obligations. In contrast, grassroots-level courts have to respect the contract terms. The intervention of local governments can at best affect to what extent the courts favor local governments in unspecified situations, which is conceptually different from affecting government compliance with debt contractual obligations.

3 Data

3.1 LGFVs and MCBs

We focus on bond-issuing LGFVs as only these LGFVs disclose financial statements for our analysis. We start by downloading data on all bonds issued before the end of 2023 and identified as MCBs by both WIND and Rating Dog. We further restrict

[8]In several instances, the U.S. Supreme Court reprimanded some of these high courts. When referencing the Iowa decision, Justice Noah Swayne of the U.S. Supreme Court suggested that "[w]e shall never immolate truth, justice, and the law because a State tribunal has erected the altar and decreed the sacrifice." (As quoted in Fairman (1971), p. 936).

our sample to the MCBs issued by LGFVs that are controlled by the prefecture-level or lower-level government units. In the Supplemental Appendix we also use MCBs issued by provincial or central government financing vehicles for placebo tests.

Panel A of Table 1 shows some summary statistics for these MCBs. There are 3,201 unique LGFVs who issued approximately 36,000 MCBs. The average issuance amount is 0.73 billion RMB and the average yield is 5.11%. There is considerable variation in the bond yield, as reflected by a standard deviation of 1.55%, which is consistent with the fact that despite no formal MCB defaults as of 2023, investors do not consider all MCBs equally safe, and the underlying credit risk as priced by investors varies across different cities.

Panel B shows some key summary statistics for the LGFVs between 2010 and 2023. The average asset size is 26.3 billion RMB. On average, 18.1% of their assets are financed by loans, 8.1% by bonds, and 50.5% by all kinds of liabilities. To calculate the extent of trade credit that these LGFVs receive from both input suppliers and project contractors, we divide their accounts payable by the sum of accounts payable, cash paid for goods and services, and cash paid for fixed asset, intangible asset and other long-term investment. On average, LGFVs receive 17.4% of trade credit from suppliers and contractors. We also estimate the firm-level borrowing rate as the interest expense scaled by the sum of short- and long-term loans and bonds. The average borrowing rate is approximately 2.3% with a standard deviation of 2.6%. Finally, the annual cash paid is about 2.0 billion RMB for goods and services and 1.4 billion RMB for fixed, intangible and other long-term investment.

Panel C shows the average default rate of our sample LGFVs. Despite no formal MCB defaults by 2023, there have been defaults by LGFVs on "non-standardized" debt obligations such as loans and trust products.[9] We obtain default events between 2016 and 2023 from the Enterprise Alert System and match them with our sample LGFVs.[10] In total, there are 139 default events involving 56 LGFVs.

[9]In 2013, the China Banking Regulatory Committee (CBRC) issued regulations on bank wealth-management products, in which it defined "non-standardized" (*feibiao* in Chinese) debt as any debt assets not traded on exchanges or in the interbank market, such as bank loans, trust products, entrusted loans, warrants, accounts receivable, and equity products with repurchase agreements.

[10]The Enterprise Alert System collects default events from various public sources, including the China Securities Regulatory Commission, China Banking and Insurance Regulatory Commission, National Enterprise Credit Information Publicity System, China Judgments Online (CJO), China Enforcement Information Disclosure Website, and major financial news websites. The Enterprise Alert

Unfortunately, the dollar amount at default is mostly missing in the data. Instead, we define a dummy *default* indicating that the LGFV has at least one default event in that year. During 2016-2023, the average default rate is 0.3%, increasing from 0 in 2016 to 1.1% in 2023. The average number of default events per LGFV-year, *defaultn*, is 0.005, increasing from 0 in 2016 to 0.02 in 2023.

3.2 Court Verdicts

We collect all court verdicts in China between 2014 and 2021 from the China Judgment Online (CJO), totaling over 144 million judgment files.[11] For each judgment file, we extract information such as the court in charge, trial and ruling dates, names of the judges, names of the plaintiff and defendant, basic facts about the case, claims made by the plaintiff, whether the court supported the plaintiff's claims, the judicial reasoning provided by the judge, etc. From this dataset, we identify 70,364 civil lawsuits involving LGFVs. Figure 3 plots the time trend of the number of cases involving LGFV defendants and plaintiffs, respectively.

A key variable for our analysis is the win rate of each party in a lawsuit. In civil practice, court fees are typically paid by the losing party. Therefore, we use the share of fees paid by the opposing side to measure the win rate of LGFVs: $WinRate_j = \frac{CourtFee_i}{CourtFee_i + CourtFee_j}$. Panel D of Table 1 shows that the average win rate of LGFVs is 0.566 for cases in which LGFVs are plaintiffs and 0.500 for cases in which LGFVs are defendants.

3.3 Value-Added Tax (VAT) Invoices

We obtain a sample of VAT invoices issued between 2016 and 2023 from a leading VAT invoice management platform, covering approximately 400 different conglomerates with 27,000 different subsidiaries that use the platform to issue VAT invoices

System has been widely used by financial institutions, government think tanks, tech companies, and universities in China.

[11] CJO is the official website established by the Supreme People's Court in 2013 to disclose court verdicts with the intention of enhancing judicial transparency and providing precedents for judges. Courts at all levels are required to publish all contemporary judgment files within seven days of trial completion, with exceptions granted for special cases such as those involving national security or juvenile delinquency. Due to judge complaints of increased workload and misuse of the verdicts, the number of verdicts disclosed on CJO has declined significantly after 2021, and hence we use the sample until 2021 (See the China File report).

Table 1: Data Summary

	Mean	Median	St. Dev.	Obs
A: MCB				
MCB issuance, RMB Billion	0.730	0.600	0.444	36365
MCB yield, %	5.108	4.950	1.555	36390
B: LGFV				
Asset, RMB Billion	26.325	16.205	33.236	29145
Loan/Asset	0.181	0.169	0.112	29061
Bond/Asset	0.081	0.069	0.081	29061
leverage	0.505	0.526	0.159	29061
Account payable/COGS	0.174	0.116	0.182	28802
borrowing rate, %	2.319	1.326	2.613	22805
Cash paid for goods&service, RMB Billion	2.0097	1.0223	4.4821	29066
Cash paid for investment, RMB Billion	1.4557	0.3991	4.6217	29064
C: Non-Standard Debt Defaults				
default	0.003	0.000	0.058	25784
defaultn	0.005	0.000	0.117	25784
D: Court Verdicts				
LGFV's win rate as plaintiffs	0.566	0.911	0.488	21,800
LGFV's win rate as defendants	0.501	0.468	0.469	48,564
E: VAT Invoices				
Price, RMB Thousand	69.112	0.064	2040.000	2.69E+07
Contract Value, RMB Thousand	55.867	1.367	594.000	2.85E+07
relative price, %	102.887	98.274	27.691	2640
supexternal	0.613	1.000	0.473	4940
supregistercap, RMB Million	413.728	146.621	655.938	4922
supage	23.631	22.980	9.789	4940
F: Residential Land Supply				
Land Supply/Pop, sqm per 100 people	147.151	114.689	137.290	5008
Land Price, RMB per sqm	2860.725	1529.078	5163.871	4996

Note: Panel A is for all MCBs issued during 2011-2023. Panel B is based on LGFVs' financial statements during 2010-2023. Panel C presents the dummy indicating LGFVs' default (`default`) and the number of default events (`defaultn`) during 2016-2023. Panel D presents the lawsuit win rate of LGFVs during 2014-2021. Panel E is based on our sample VAT invoices during 2016-2023 – the first two rows report LGFVs' purchase invoices and the remaining report aggregated values at the LGFV-year level. Panel F reports the residential land supply and land price at the city-year level.

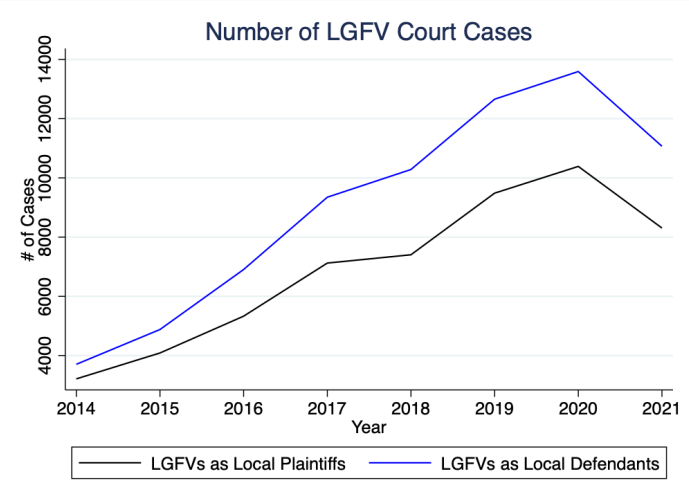


Figure 3: Number of Court Cases Involving LGFVs Overtime

to 4.7 million different customers.[12] The data covers approximately 400 million VAT invoices. The VAT invoice records detailed information for each transaction, including the name and tax ID of the seller and the buyer, the transaction date, the transacted goods and services, the unit price, and quantity.

We focus on VAT invoices for which LGFVs are the customers and the sellers are contractors and suppliers.[13] As sample selection is done on the sellers' side, our analysis at the LGFVs' level does not suffer from sample selection bias. In total, 1,688 out of the 3,201 LGFVs are matched with at least one VAT invoice with the selected contractors and suppliers.

Panel E of Table 1 provides summary statistics for these matched VAT invoices. The average unit price is 69.1 thousand RMB, and the average value is 55.8 thousand RMB. The relative price paid by LGFVs as compared to non-LGFVs for the same product from the same seller in the same year is 1.029 (See Section 4.3 for the definition of "relative price"). On average, LGFVs purchase 61.3% from external sellers. The average size of their sellers, measured by contributed capital, is 413.7 million RMB, and the average age is 23 years.

[12]The VAT is paid by all firms in China since 2016. Before 2016, firms in certain industries paid business taxes and switched to paying the VAT after the Business-to-Value-added Tax reform in 2016. For more details, see He et al. (2023).

[13]We exclude other sellers such as grocery stores, hotels, and restaurants by dropping sellers whose average selling VAT invoice value is below 50,000 RMB.

3.4 Land Auction Data

We download all the land sale data from landchina.com, the official website established by the Ministry of Natural Resources to disclose the universe of land sales by local governments in China since 2007 (He et al., 2023). We focus on land parcels sold through market-based mechanisms (e.g., agreement, tender, auction, and listing) and aggregate transactions to the city-year level. Panel F of Table 1 shows that the annual residential land supply during 2007-2023 is about 147.1 sqm for every 100 urban residents and the average land price is 2,860 RMB per sqm.

4 Judicial Reform and LGFVs' Lawsuits

In this section, we establish the first stage of the empirical results, i.e., whether the judicial centralization reform has mitigated the extent of judicial favoritism towards LGFVs. We lay out our empirical strategy in Section 4.1 and present the empirical results in Section 4.2. In Section 4.3 we complement the main results by showing that changes in ex-post lawsuit outcomes also affect ex-ante contracts.

4.1 Empirical Strategy

For the baseline analysis, we employ case-level data and exploit the staggered roll-out of the reform to estimate the following difference-in-differences (DID) model:

$$winrate_{icnt} = \beta \cdot Reform_{ct} + \alpha_n + \theta_t + \epsilon_{icnt}, \tag{1}$$

where $winrate_{icnt}$ is the win rate of LGFVs in case i tried in court n and semi-year t; $Reform_{ct}$ is a dummy variable that equals one if the city c where the LGFV is located had gone through the reform at time t and zero otherwise; and α_n and θ_t represent court and semi-year fixed effects, respectively. We cluster standard errors at the prefecture level since the reform is rolled out at this level.

To test for parallel trends of our DID design and understand the dynamic effect of the reform, we also estimate the following event study model:

$$winrate_{icnt} = \sum_{\tau \neq -1} \beta_\tau \cdot \mathbf{1}_{t-T_c=\tau} \cdot Reform_{ct} + \alpha_n + \theta_t + \epsilon_{icnt}, \tag{2}$$

where T_c represents the semi-year when city c completed the reform and β_τ represents the treatment effect τ periods away from the reform.

Throughout this paper, we account for heterogeneous treatment effects following Sun and Abraham (2021). The patterns remain the same for the conventional unadjusted event study estimates and for estimates based on other methods proposed in the recent literature (e.g., Borusyak et al., 2021; and Callaway and Sant'Anna, 2021). We present different versions of event studies when discussing the robustness of our findings.

4.2 Empirical Results

Table 2 presents the baseline DID results obtained from estimating Equation (1) using case-level data. As the cases are handled by courts in defendants' cities, Columns (1)-(5) focus on cases with LGFVs as defendants to ensure that it is courts in the LGFVs' cities that handle the cases. Column (1) reveals a notable post-reform shift: local courts demonstrate a significant decrease in favorable rulings toward LGFV defendants, with the average win rate of LGFV defendants dropping by 14.0% – a 25.4% decline from the pre-reform baseline win rate.

To exploit heterogeneity of the effect, we split the sample into cases involving external and local plaintiffs in Columns (2) and (3). Given that pre-reform local court protectionism disadvantages external plaintiffs more than local plaintiffs, we anticipate that the reduction in LGFVs' win rates against external plaintiffs is greater than that against local plaintiffs. The coefficient estimates shown in Column (2) and (3) support this hypothesis. Furthermore, Columns (4) and (5) show that the effect of the reform is more pronounced in high-stakes cases (i.e., with disputes over construction contracts) than in other low-stakes cases.[14]

Column (6) investigates changes in LGFVs' win rates in cases against local defendants, which are still handled by courts in the LGFVs' cities. Here, the estimated effect is negative but statistically insignificant. This weaker significance may be because LGFVs become more selective in pursuing litigation in response to the reform, favoring cases with stronger legal grounds and opting for alternative dispute resolution methods, such as settlements, for weaker cases.

[14]Disputes over construction contracts represent the most frequently observed cases with higher economic stakes, featuring an average monetary damage claims of 5 million RMB.

Table 2: Judicial Centralization Reform and LGFVs' Win Rate

	(1)	(2)	(3)	(4)	(5)	(6)
LGFVs as	Defendants					Local PLTF
	All	External Plaintiffs	Local Plaintiffs	High-stakes	Low-stakes	All Cases
Reform	-0.140***	-0.172***	-0.116***	-0.268***	-0.084**	-0.078
	(-4.67)	(-3.74)	(-3.52)	(-8.93)	(-2.33)	(-1.39)
Mean of Outcome	0.55	0.55	0.55	0.55	0.55	0.60
Court FE	Y	Y	Y	Y	Y	Y
Semi-year FE	Y	Y	Y	Y	Y	Y
Observations	45,681	14,389	31,292	14,355	31,326	16,951
R-Squared	0.231	0.322	0.254	0.263	0.301	0.279

Notes: This table reports the baseline DID estimates on judicial outcomes in commercial lawsuits involving LGFVs. Columns (1)-(5) focus on the win rates of LGFVs as defendants. Column (6) investigates changes in the LGFVs' win rates as plaintiffs against local defendants. Standard errors clustered by cities. Robust t-statistics in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 4 shows the dynamics of the treatment effect by plotting the event study coefficients obtained from estimating Equation (2). We observe a flat pre-trend before the reform, validating the parallel trend assumptions for the DID specification. We observe a salient reduction in local defendants' win rate immediately after the reform. The reform's impact has been persistent for more than three years since the reform. Similarly, the event study estimates for cases in which LGFVs act as plaintiffs filing lawsuits against local defendants show consistent patterns. Although the point estimates in the DID regression are not statistically significant, there is indeed a clear downward trend in win rates of LGFV plaintiffs after the reform. For robustness, Appendix Figure A.1 presents alternative event study estimates following Borusyak et al. (2021), and our results remain robust.

More supportive evidence. We provide more evidence in Section A of the appendix to show that the decrease in LGFVs' win rates is due to reduced court favoritism after the reform. First, to address the concern that the results may be driven by some local shocks correlated with the reform, we conduct placebo tests using two subsamples: a) cases involving financing vehicles controlled by a provincial or the central government and b) cases in which LGFVs are plaintiffs filing lawsuits against external defendants (and hence are handled by external

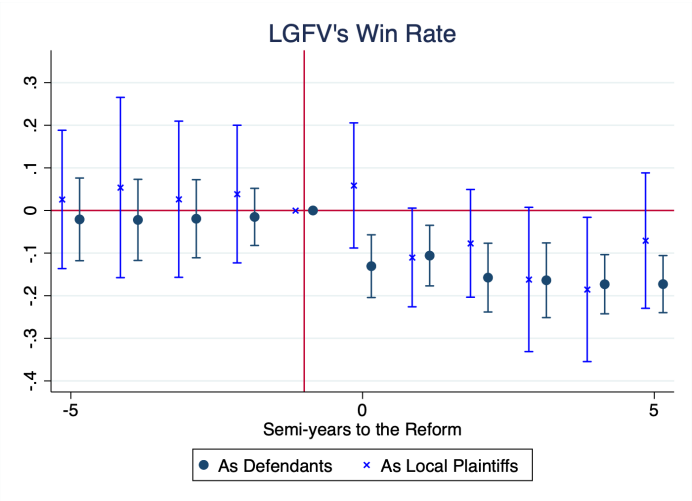


Figure 4: Judicial Centralization Reform and LGFV's Win Rate: Event Study

Note: In this figure, we plot the event study coefficients and the corresponding 95% confidence intervals for the baseline results in Columns (1) and (6) of Table 2, respectively. All event studies are estimated following Sun and Abraham (2021).

courts). The local shocks should lead to similar results on these two subsamples, yet we do not find any significant changes in win rates after the reform.

Second, the baseline results using case-level data represent a mixture of intensive and extensive margin adjustments. The intensive margin concerns changes in LGFVs' win rates in identical cases, and the extensive margin concerns changes in case composition. To examine the effect on the intensive margin, we focus on a subset of lawsuits initiated within six months prior to the reform and compare the outcomes of these cases based on whether the rulings were issued before or after the reform. This approach effectively controls for case composition – the characteristics of plaintiffs and defendants are similar for cases adjudicated before and after the reform. We find a quantitatively larger effect of the reform on LGFVs' win rates on the intensive margin than the baseline estimates. To study the extensive margin effect, we find that LGFVs are more likely to be sued after the reform, and the plaintiffs are less capitalized, younger and have fewer employees. These findings suggest that the reform has encouraged more small plaintiffs, who were previously deterred from suing LGFVs, to initiate lawsuits against LGFVs. This is consistent with the finding that the intensive margin effect, which is not influenced by the shift in case composition, is more pronounced than the baseline estimates.

Judges' Discretion. The magnitude of the effect as reported in Table 2 suggests substantial discretion of judges in rulings in cases involving LGFVs, especially over construction contracts. As an example to illustrate judges' substantial discretion, contractors often subcontract part of the project to subcontractors and the subcontracts may stipulate that "all payments (progress payments, final payments) that Party A shall pay to Party B under this subcontract can only be made after the contractee of the project pays the corresponding amount to Party A." When Party B sues party A as well as the LGFV, who is the contractee, for payment delays, some judges would refuse to support party B's claims based on the term above, while some judges believe Party B should be fairly compensated for the work they have done and choose to support Party B's claims. A concluding remark was made in 2024 in the judges' meeting summary issued by the Fifth Circuit Court of the Supreme People's Court, which indicates that the settlement of a subcontract shall not be made contingent upon the settlement of the main contract.[15]

4.3 Responses of Contracting Prices and Contractors

The change in win rates shifts transfers from LGFVs to counterparties. If both parties anticipate this effect of the reform in case of lawsuits, contractors and suppliers may be willing to offer lower prices ex ante, and those previously unwilling to do business with LGFVs may be open to it post-reform. As supplementary to the lawsuit results, we investigate whether, and to what extent, the adverse ex-post effect of the reform on LGFVs is mitigated by adjustment of ex-ante contracts.

To investigate the price effect and control for all factors on the sellers' side that may affect contracting prices, we use prices paid by non-LGFVs for the same products to the same sellers in the same year as the benchmark and calculate the relative price paid by LGFVs.[16] We then investigate whether the relative price decreases by more for LGFVs in cities with the judicial reform than for LGFVs in cities without the reform.

[15] See https://www.thepaper.cn/newsDetail_forward_29385573.

[16] The sample only includes manufacture products because there are no well-defined prices for construction projects and services.

Formally, we define the relative price paid by LGFVs as follows:

$$rp_{i,j,k,t} = P_{i,j,k,t} \Big/ \sum_{i' \notin \mathbf{L}} P_{i',j,k,t} \frac{Q_{i',j,k,t}}{\sum_{i' \notin \mathbf{L}} Q_{i',j,k,t}} \tag{3}$$

, where $P_{i,j,k,t}$ denotes the price paid by firm i to seller j for product k in year t, $Q_{i,j,k,t}$ the corresponding quantity, and $\mathbf{L}$ the set of LGFVs. We then estimate the price impact of the judicial reform via the following DID model and take into account heterogeneous treatment effect following Sun and Abraham (2021):

$$rp_{i,j,k,t} = \beta \cdot Reform_{c(i),t} + \alpha_i + \theta_t + \varepsilon_{i,j,k,t} \tag{4}$$

The results are shown in Panel A of Table 3. Although the VAT invoices provide detailed identifying information about the product, there is still quite some variation in unit prices across invoices for the same product from the same seller in the same year, probably driven by differential quality and variety. To ensure uniformity in quality and variety, we calculate price variation as the standard deviation scaled by the average of prices for products with the same name and specification from the same seller in the same year, and restrict the sample to products with price variation smaller than the median, which is about 0.2. The average rp conditional on this sample is 1.029 as shown in Table 1. Column (1) shows that the prices paid by LGFVs decrease significantly by 0.09% after the reform. In Column (2)-(3) we check the robustness of the results with different threshold for the price variation. Overall, contractors and suppliers are willing to offer lower prices to LGFVs after the reform, despite small magnitude of the effect.

In addition to the price effect, the reform may also attract contractors and suppliers previously unwilling to do business with the LGFVs to do it now. As shown in Table 2, the reform benefits external counterparties more than local ones. Thereby, we expect external contractors and suppliers to increase their business with LGFVs more than local ones after the reform.

To test this prediction, we aggregate the VAT invoices to the LGFV-year level and calculate the share of invoices from external contractors and suppliers in terms of transaction value. We then run the following DID model:

$$y_{i,t} = \beta \cdot Reform_{c(i),t} + \alpha_i + \theta_t + \varepsilon_{i,t} \tag{5}$$

Table 3: Impact of Judicial Reform on Ex-ante Contracts

Panel A: Price Effect

Dep Var: relative price, %	(1)	(2)	(3)
Sample: price variation <	0.2	0.15	0.25
Reform	-0.0947***	-0.0862***	-0.109***
	(-5.991)	(-3.654)	(-2.712)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	2623	2330	2962
Contract Value	0.9108	0.9264	0.8911

Panel B: Change in Contractors and Suppliers

	(1)	(2)	(3)
Dep Var:	supexternal	log(supregistercap)	log(supage)
Reform	0.0366*	0.019	0.0131
	(1.671)	(0.184)	(0.513)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	4,493	4,477	4,492
R-squared	0.7726	0.4968	0.6441

Note: This table shows how a more favorable legal environment for LGFVs' counterparties after the reform affects their contracts ex ante. Panel A shows that LGFVs can purchase inputs at lower prices, and Panel B shows that they buy more from external suppliers and contractors. Standard errors are clustered by cities. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Panel B of Table 3 shows the results. As shown in Column (1), LGFVs significantly increase their purchases from external sellers after the reform by approximately 3.7%. We do not find statistically significant changes in the sellers' size and age after the reform as shown in Column (2)-(3).

The analysis on the ex-ante contracts serves two purposes. First, it lends further support to the hypothesis that the reform has benefited the counterparties of LGFVs. The evidence suggests that the counterparties have anticipated such benefit when initiating the contract. Second, and mostly importantly, the magnitude of the price impact is very small and unlikely to fully offset the increased lawsuit losses. We will compare the magnitude of the price advantages with the lawsuit losses in Section 5.4.

5 LGFVs' Debt Capacity and Spending

In this section, we study whether and to what extent alleviating local court capture affects the financing and operating activities of LGFVs. About 93.67% of the lawsuits involving LGFVs are against their business partners, with the rest against financing companies and few against banks. This is probably because unlike debt contracts that are simple and complete, procurement contracts are much more complex and incomplete, leaving much room for disputes. As a result, the creditors of LGFVs do not directly benefit from the reform but can be indirectly affected. We start by discussing the identification assumptions, and then show the adverse impact of the reform on LGFVs' debt capacity, borrowing costs, and credit default. We then investigate the channels through which the reform affects the financing of LGFVs. We provide evidence on the real effect in the end.

5.1 Identification Assumptions

There are two types of concerns when we interpret the effect of the judicial centralization reform on LGFVs' debt capacity as due to less court favoritism towards LGFVs. The first concern is reverse causality, i.e., city governments with tightened budget constraints may be more willing to implement the reform so as to save the money that would be allocated to the local courts and procuratorates without the reform. However, this argument is unlikely to be relevant because court and procuratorate expenditure constitutes a negligible fraction of city governments' budgetary expenditure. In 2014 (2021), total local court and procuratorate expenditure is 1.4% (1.4%) of total city government budgetary expenditure. The reform can barely directly improve city governments' financial conditions and cannot be driven by the city governments' financial conditions in the first place.

Relatedly, Yang and Li (2023) argue that provincial government financial capacity may affect their decision to implement the reform since the reform makes court and procuratorate expenditure their burden. In 2021, total court and procuratorate expenditure was 9.2% of total provincial government budgetary expenditure, with substantial variations across provinces. In particular, the average ratio between province-wide public security expenditure (of which court and procuratorate expenditure is a significant component) and provincial government expenditure is 47% for non-reformed provinces, much higher than 31% for reformed provinces.

Table 4: Judicial Reform and City/LGFV Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var	$\frac{\text{NetIssue}}{\text{FisRev}}$	MCB Yield	log(Asset)	borrowing rate	$\frac{\text{Bond}}{\text{Asset}}$	$\frac{\text{ResLand}}{\text{Pop}}$
Reform	-0.0403	-0.0478	0.122	0.153	0.00119	12.96
	(-1.057)	(-0.520)	(1.206)	(0.648)	(0.371)	(0.937)
Constant	0.199***	7.014***	4.405***	2.631***	0.0389***	189.5***
	(5.775)	(98.50)	(69.42)	(14.25)	(14.89)	(17.96)
Observations	266	264	1,760	1,042	1,740	312
R-squared	0.005	0.001	0.002	0.001	0.000	0.002

Note: This table shows the correlation between the reform status and the city's and LGFVs' characteristics in 2013. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The second concern involves omitted variables or confounding shocks that may be related to both the reform and the local fiscal conditions. In the following analysis, we conduct event study analysis and find that there is no significant difference in time trends between treated and control cities before the reform along all the outcome variables examined in this paper. This finding partially mitigates the concern that some persistent differences between treated and control cities are underlying the treatment effect that we find. Moreover, in Table 4, we regress all the important outcome variables measured in 2013 and to be examined in this section on cities' reform status until 2020. None of these variables are significantly correlated with the cities' reform status. This result ensures that the major driving forces for the reform do not stem from the local fiscal sector.

One potential concern of confounding shock is that the reform, as it weakens local government power, might be driven by or associated with a reduction of political support from upper-level governments, which could affect local government debt capacity. In appendix Section A.5 we examine three major forms of support from superior governments: fiscal transfer, allocation of quota of municipal bonds, and local official promotion. We do not find any evidence suggesting reduced superior support in reformed cities.

5.2 LGFVs' Debt Capacity

Issuance and Yield of MCBs. To study the effect on LGFVs' debt capacity, we start by analyzing the MCBs as there is more detailed information (e.g., yield)

of bonds. We first aggregate the individual MCBs to obtain the city-level MCB outstanding balance at the end of each year since 2010. We then calculate the net MCB issuance, $NetIssue_{ct}$, as the change in the total MCB balance from year $t-1$ to year t, scaled by city c's fiscal revenue in 2013, right before the earliest judicial reform year. We also calculate the city-year level average MCB yield, $AveYield_{c,t}$, using all MCBs issued by government units in city c and year t and weighted by the issuance amount of each bond.[17]

We then conduct the event study analysis with the following specification:

$$Y_{ct} = \sum_{\tau \neq -1} \beta_\tau \cdot \mathbf{1}_{t-T_c=\tau} \cdot Reform_{ct} + \alpha_c + \theta_t + \varepsilon_{c,t}, \tag{6}$$

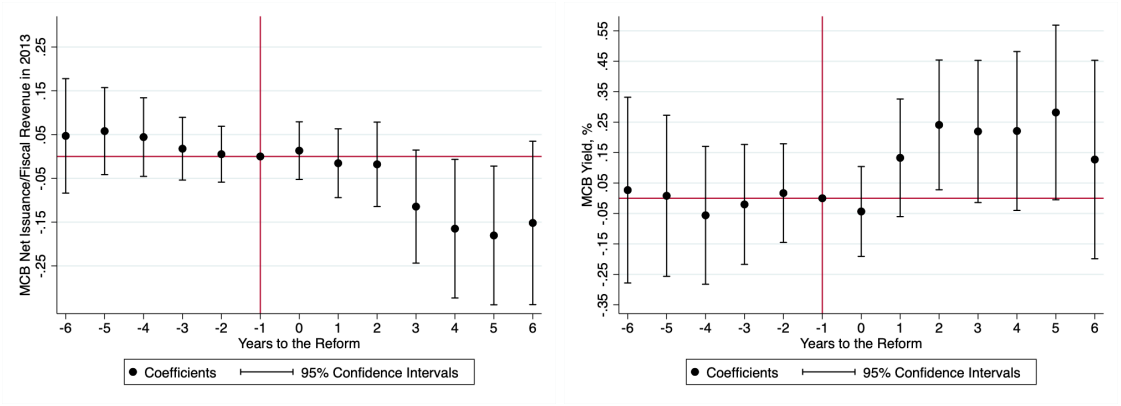
where T_c is the reform year of city c and $Reform_{ct}$ is a dummy that equals one if the city has experienced the judicial reform in year t and zero otherwise. The coefficient β_τ captures the effect of the judicial reform τ years away from T_c. Again, we account for heterogeneous treatment effects following Sun and Abraham (2021).

Figure 5 plots the β_τ estimates for $NetIssue_{ct}$ and $AveYield_{ct}$. For both $NetIssue_{ct}$ and $AveYield_{ct}$, the treated and control cities exhibit parallel trends before the reform. For $NetIssue_{ct}$ as shown in Panel (a), the treated and control cities begin to diverge three years after the reform. In the third year, net MCB issuance in the treated cities is significantly lower by more than 10%, and the magnitude of the effect increased to approximately 15% in the next three years. There is no evidence of a reversal at least six years after the reform. For $AveYield_{ct}$, one year after the reform, the average MCB yield for the treated cities is significantly higher by 0.15%. The magnitude of the effect further increases to 0.25% and remained stable over the next four years. The decline in net issuance and increase in yield suggest an adverse effect of the reform on investors' demand for MCBs.

LGFVs' Balance Sheets. We now investigate the overall effect of the reform on LGFVs at the firm-level.[18] As shown in Table 1, bond financing accounts for about 8.1% of LGFVs' assets on average. Other creditors may be affected in the same way

[17] In case of no new MCB issuance in year t, we fill $AveYield_{ct}$ first with previous non-missing and then with future non-missing values.

[18] We do not aggregate the firm-level balance sheets to the city level and conduct the analysis at the city level because we can only observe financial information for those bond-issuing LGFVs. The number of all LGFVs published by the CBRC is approximately four times the number of LGFVs in our sample, because a large fraction of LGFVs do not issue bonds.



(a) Net Issuance/Budgetary Revenue (b) Average Yield, %

Figure 5: MCB Issuance and Yield after the judicial reform

Note: This figure plots the 95% confidence interval of the effect of the judicial reform on the city-level MCB net issuance and yield over time. Standard errors are clustered by cities.

as bond creditors. Otherwise, LGFVs may substitute bond financing with other credit sources, leading to a smaller effect at the firm level.

We first examine LGFVs' asset size with the following event-study specification:

$$\log(\text{Asset}_{ict}) = \sum_{\tau \neq -1} \beta_\tau \cdot \mathbf{1}_{t-T_c=\tau} \cdot \text{Reform}_{ct} + \alpha_i + \theta_t + \varepsilon_{ict}, \tag{7}$$

where i denotes the LGFV, c denotes the city, and t denotes the year.

Figure 6 shows the 95% confidence intervals of the β_τ estimates. Treated and control LGFVs exhibit parallel trends in asset growth before the reform. After the judicial reform, the treated LGFVs start to experience significantly slower asset growth than those untreated LGFVs. The gap increases from approximately 5% in the first year to 15% in the sixth year after the reform.[19]

We now examine LGFVs' balance sheet by estimating the following DID specification with various firm-level variables as the outcome variables:

$$Y_{ict} = \beta \cdot \text{Reform}_{ct} + \alpha_i + \theta_t + \varepsilon_{ict} \tag{8}$$

Column (1) of Table 5 confirms that on average, the judicial reform reduced

[19]The effect on asset growth seems to occur earlier than the effect on MCB issuance. In Section 5.3 we examine the effect on LGFVs' default on non-bond debt obligations. Non-bond creditors seem more affected by the reform than bond creditors. Reduced financing from these non-bond credit sources may be underlying the effect on asset growth in the first few years.

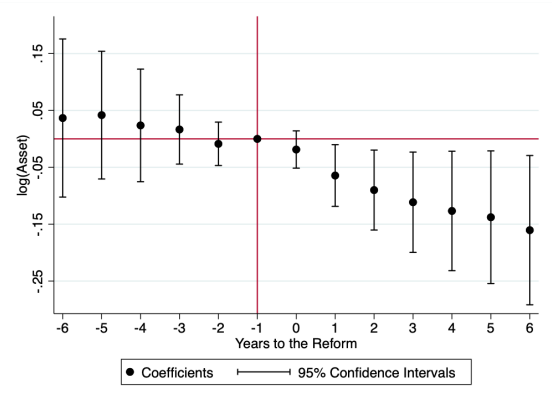


Figure 6: LGFV asset size after the judicial reform

Note: This figure plots the 95% confidence interval of the effect of the judicial reform on the LGFV's asset over time. Standard errors are clustered by cities.

LGFVs' asset size by about 9.3%. Column (2) shows that the average borrowing rates also increased by about 0.32% after the reform. The decrease in asset growth and increase in borrowing rates suggest that at the firm level, LGFVs have suffered from higher borrowing costs and slower growth.

Moreover, the magnitude of the impact on asset growth and borrowing rates suggests that other creditors are affected in the same direction as the bond investors. This is because bond financing accounts for only 8.1% of LGFVs' assets, and that the judicial reform reduces the net bond issuance (bond yields) by no more than 15% (0.25%) as shown in Figure 5. If other credit sources are not adversely affected, then we will not observe a decrease in asset growth by as large as 9.3% or an increase in borrowing rates by as large as 0.32%. Note that our sample is restricted to bond-issuing LGFVs. The estimates suggest that non-bond-issuing LGFVs must have also experienced similar negative impacts as our sample LGFVs.

In Table 5 we further examine different components of LGFVs' liabilities. In Column (3), we find that even when scaled by firms' asset, there is still a significant and negative effect on the use of bond financing. Column (4) shows there is no significant impact on firms' leverage, due to a shift towards more trade credit as shown in Column (5). The increased use of trade credit reflects not only the substitution of bond and loan financing for trade credit but also higher willingness of contractors and suppliers to extend trade credit, because if LGFVs attempt to default, they are more likely to recover the loss via lawsuits after the reform.

Table 5: Effect on LGFVs' Balance Sheet

	(1)	(2)	(3)	(4)	(5)
Dep Var:	log(Asset)	borrowing rate	Bond/Asset	leverage	AccPayable/COGS
Reform	-0.0934**	0.325**	-0.0116**	0.00981	0.0237**
	(-2.147)	(2.442)	(-2.376)	(1.029)	(2.319)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	27,967	21,692	27,884	27,884	27,627
R-squared	0.8664	0.6257	0.5833	0.7153	0.5193

Note: This table shows how the judicial reform has affected LGFVs' balance sheets, including total asset size, borrowing rate, bond financing, leverage, and trade credit. Standard errors are clustered by cities. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.3 LGFVs' Default on Debt

Although LGFVs have not defaulted on MCBs, they have defaulted on less standardized debt obligations such as trust products. Compared with MCBs that are issued in the interbank market or traded on exchanges, these non-standardized (*feibiao* in Chinese) debt securities are subject to less strict regulations and fewer information disclosure requirements. Creditors of these *feibiao* debt securities are typically smaller than bond investors and banks. Hence, defaulting on *feibiao* debt securities is less costly for LGFVs than defaulting on other types of securities. This selective default on *feibiao* debt securities by LGFVs has been widely reported in the news and indicated in LGFVs' announcements.[20]

By looking at the raw data, in 2023 there are 34 LGFV default events, among which 29 come from cities that have experienced the reform, suggesting significant correlation between the reform and LGFVs' default. Formally, we study the impact of the reform on LGFVs' default on these *feibiao* debt securities by estimating Equation (8). We use two variables to capture LGFVs' default. The first is a

[20]For instance, Zunyi Daoqiao, the largest LGFV in Zunyi city, Guizhou province, reorganized its bank loans by expanding its maturity to 20 years and reducing its loan interest to 3-4.5%. At the same time, Zunyi Daoqiao indicated that it would not default on its outstanding bonds in the open market. Zunyi Daoqiao's official announcement was made on ChinaBond on December 30, 2022. Another example is the bankruptcy case filed by a trust company against two LGFVs in Guizhou in August 2023, which mentioned that these LGFVs delivered principal and interest payments for bonds but not for trust products. See the report here.

Table 6: Effect on LGFVs' Defaults

	(1)	(2)
Dep Var:	default	defaultn
Reform	0.00511**	0.00918**
	(2.237)	(2.408)
Firm FE	Yes	Yes
Year FE	Yes	Yes
Observations	24,381	24,381
R-squared	0.2779	0.2175

Note: This table presents the city-level DID estimation results of the judicial reform's impact on LGFVs' default on *feibiao* debt obligations. The dependent variable $\mathtt{default}$ is a dummy indicating whether the LGFV has defaulted on *feibiao* debt in that year, and $\mathtt{defaultn}$ is the number of default events of the LGFV in that year. Standard errors are clustered by cities. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

dummy, $\mathtt{default}_{ict}$, which equals one if the LGFV i has at least one default event in year t and zero otherwise. The second is $\mathtt{defaultn}_{ict}$, the number of default events of the LGFV i in year t. As in the previous sections, we control for heterogeneous treatment effect following Sun and Abraham (2021).

Table 6 reports the results. In Column (1), the reform has significantly increased the LGFVs' default by 0.51%, which is economically important compared to the mean of 0.3% reported in Table 1. In Column (2) when we use the number of default events as the dependent variable, we find similar results. Overall, the significant and positive effect on LGFVs' default suggests an important adverse impact of the reform on LGFVs, which is consistent with our conjecture.

5.4 Mechanism

In this section, we discuss channels through which the judicial centralization reform that reduces local court favoritism towards LGFVs leads to an adverse impact on the debt capacity of LGFVs. Throughout the discussion, we highlight that it is the business partners that are directly affected by the reform, with creditors indirectly affected. In short, the reform discourages LGFVs from delaying payment to business partners, and conditional on entering the lawsuit with business partners,

they will incur higher losses, the court is more likely to refute their defense for payment delays and force them to pay immediately, and their credit risk will be signaled to outside investors in case of lawsuit default, all of which will lead to credit tightening by creditors.

5.4.1 Lawsuit Losses

Lawsuits typically involve some cost that the two parties disagree on how to split. By decreasing LGFVs' win rate, the reform increases the share borne by LGFVs, leaving them with less resource to meet their debt obligations. We conduct a back-of-the-envelope calculation to gauge the magnitude of the increased lawsuit losses. Specifically, the dollar value of the increased lawsuit losses per LGFV can be calculated using the following equation:

$$\begin{aligned}&\text{Dollar value of lawsuit losses per LGFV}\\ =&\Delta\text{ LGFVs' Win Rate} \times \text{No. of lawsuits per LGFV} \times \text{Average Disputed Amount}\end{aligned}$$

That is, the dollar value of the increased lawsuit losses should equal the effect of the reform on the LGFVs' win rate, which we estimate in Table 2, times the number of lawsuits and the average disputed amount.

As defendants, given that the effect on LGFVs' win rate is larger for high-stakes than low-stakes cases, we calculate the increased lawsuit losses per LGFV of high-stakes cases to be 7.73 million RMB ($= 0.268 \times 6.22 \times 4.64$ million RMB) and of low-stakes cases to be 2.70 million RMB ($= 0.084 \times 13.14 \times 2.45$ million RMB). As local plaintiffs, we calculate the dollar value per LGFV to be 3.94 million RMB ($= 0.078 \times 7.18 \times \$\ 7.03$ million RMB). Therefore, the total dollar value of the increased lawsuit losses per LGFV due to the reform is 14.37 million RMB, which is about 0.18% of the LGFV's accumulated cash paid for goods, services, fixed assets, intangible and other long-term investment during 2014-2021. Recall that in Section 4.3, we find that the prices paid by LGFVs for manufacturing products are slightly lower by about 0.09% after the reform, which is quantitatively smaller than the direct lawsuit losses.

At first glance, the increased lawsuit loss is very small, especially after adjusting for lower contracting prices ex ante. However, this may underestimate the total increased losses of LGFVs over all disputes because many disputes do not reach

the court and could be similarly affected by the reform. In the example we give to illustrate judges' discretion in Section 4.2, the subcontractor sues the contractor and the LGFV as the contractee, the disputed amount is only between the subcontractor and the contractor, which is a small part of the total disputed amount between the LGFV and others. In case they have the disputes settled outside to the court, the disputed amount is not recorded. The reform may also affect the negotiation over disputes outside the court by affecting the outside option of going to the court. Therefore, we expect the total dispute losses of LGFVs due to the reform to be larger than 0.18% of its accumulated cash paid, although we do not take a stance on how large or important this channel is.

5.4.2 De Facto Seniority of Creditors

Local governments deal with multiple creditors and have a certain degree of discretion regarding the order of payments, leading to *de facto* differences in debt seniority. For instance, Gao et al. (2021) find that local governments in China selectively default on banks with weaker political power. In our paper, because suppliers and contractors are much less powerful as compared to the financing companies, bond investors and banks (which are typically large financial institutions), local governments may opt to default on suppliers and contractors to pay other creditors in times of financial distress. This selective default hypothesis explains why almost all the lawsuits involving LGFV payment delays are against suppliers and contractors, only a few against small financing companies, and very few against banks. The judicial reform then changes the order of seniority by increasing the legal constraint of payment delays to suppliers and contractors, making claims of contractors and suppliers more valuable while creditors' claim less valuable. Creditors then respond by cutting lending to the LGFVs.

The effect on creditors' de facto seniority can occur on two margins. The effect on the first margin follows directly from the lawsuit results, i.e., conditional on entering the lawsuit, the court post-reform is more likely to refute the LGFVs' defense for payment delays and will make LGFVs to pay the business partners immediately rather than let them hold the money for future debt obligations. The second margin is regarding the LGFVs' tendency to delay payment to suppliers and contractors in the first place. Anticipating less court favoritism post-reform, the LGFVs may be less likely to delay payment.

An imperfect approach to examine effect on the second margin is to look at changes in the number of LGFVs' lawsuits featuring their payment delays. This is imperfect because of two reasons. First, the reduced debt capacity of LGFVs will increase their financial distress and worsen their payment delays to all existing counterparties, such as default on non-standardized debt obligations as shown in Section 5.3. Second, anticipating a fairer legal environment, the suppliers and contractors are more likely to initiate lawsuits against LGFVs, especially those smaller and less powerful plaintiffs who were previously deterred from suing LGFVs (See Section A in the Supplementary appendix). Both factors lead to an increase in the number of observed lawsuits featuring LGFVs' payment delays and counteract the LGFVs' decreasing tendency to delay payment.

Nonetheless, we conduct the city-level event study using Equation (6) with the logarithm of the number of LGFV's lawsuits initiated within each semi-year and featuring their payment delays as the outcome variable. Figure 7 shows the event study results. Despite the two counteracting factors as we discuss above, we still see a decreasing trend in the number of payment delay cases, and the effect becomes significantly negative two years following the reform. This effect within two years is unlikely to be driven by reduced initiation of new projects because payment is typically scheduled a few years after project initiation. This result, in contrast to the increased default on non-standardized debt obligations, provides direct evidence consistent with improved de facto seniority of suppliers and contractors' claims to the disadvantage of other creditors.

5.4.3 Information Production of Courts

Information disclosure by the LGFVs has been quite limited. The LGFVs' debts are typically secured by illiquid land collaterals. The repayment of LGFVs' debts heavily relies on the financial support of local governments, whose financial positions are even more opaque. Court verdicts offer valuable information about the LGFVs' operation and financial conditions. Alert to firms' commercial lawsuits is highlighted on widely-used websites such as TianYanCha.com that offers comprehensive firm information. Thereby, information in court verdicts can reach many investors and plays an important role in firms' credit rating reports.

In the data, a large fraction of the lawsuits with LGFVs as defendants revolve around payment delays by LGFVs. In general, payment delays can stem from two

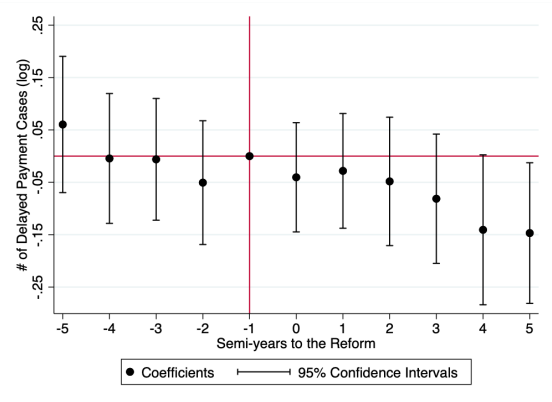


Figure 7: LGFVs' Payment Delay after the Judicial Reform

Note: This figure plots the 95% confidence interval of the effect of the judicial reform on the number of LGFV's payment delay cases. Standard errors are clustered by cities.

primary reasons: liquidity constraint of the LGFVs and misconduct of the contractors. When the LGFVs delay payment due to its liquidity constraint, they can defend themselves with various excuses. For example, the project quality does not meet the contractual standards, auditing of the project's actual cost and workload (which determines the payment amount) is not finished, the subcontracting by the contractor is illegal, and the contractor has not provided enough documents (e.g., legitimate proof of its cost) to process the payment. In case the defense is not supported by the court, outside creditors observe the court decision and will perceive payment delays to be more likely due to the LGFVs' liquidity constraint. The creditors will then respond by cutting lending, and difficulty in rolling over debt may in turn exacerbate the credit risk and default of LGFVs.

More rigorously, we can think of LGFVs as being of either good or bad type. The good type has enough endowment or financial support from the local governments to meet its obligations, while the bad type does not. The good type will only delay payment when they believe that the contractors misconduct, while the bad type always delays payment. Therefore, conditional on entering lawsuits over payment delays, the good type is more likely to win than the bad type. From the perspective of outside investors, those that win the lawsuits are more likely to be of good type and investors are willing to extend credit, and those that lose are more likely to be of bad type and investors will respond by tightening lending.

Empirical Evidence. To provide direct evidence on the information production role of the court, we examine whether, after losing a lawsuit around its payment delays, the LGFV is more likely to experience credit rating downgrading within the following one year.[21] Importantly, such patterns only exist for lawsuits involving LGFVs' payment delays, as rulings of other types of lawsuits do not inform investors about the financial conditions of LGFVs.

To this end, we first select LGFV-year pairs such that the LGFV has at least one lawsuit case in that year featuring its payment delays and calculate $winrate_{ict}$, the average win rate of the LGFV i for all such cases in year t. We then estimate the following equation:

$$Downgrading_{ic,t+1} = \beta \cdot winrate_{ict} + \alpha_i + \theta_t + \epsilon_{ict}, \tag{9}$$

where $Downgrading_{ic,t+1}$ is a dummy that equals one if the LGFV i experienced credit rating downgrading within the next one year and zero otherwise.

We first estimate Equation (9) using OLS estimation. As shown in Column (1) of Panel A in Table 7, losing lawsuits featuring LGFV payment delays is significantly correlated with the probability of credit rating downgrading within the next year.

The OLS estimation, however, does not necessarily suggest a causal effect through information production of the court because financial analysts could also obtain information about the LGFVs' credit risk from other sources, which may correlate with LGFVs' lawsuit win rate. To address this concern, we conduct the 2SLS IV estimation by instrumenting $winrate_{ict}$ with the city's reform status, $reform_{ct}$. As long as information collection from other sources is not affected by the judicial reform, the IV estimator captures whether changes in win rates induced by the reform, holding other information sources constant, can affect the credit rating downgrading. Column (2) shows a significant and negative effect of the win rate on the probability of rating downgrading within the next year, which provides strong support for the information production role of the court.

To interpret the magnitude of the effect, we multiply the IV estimate with the effect of the reform on LGFVs' win rate, as reported in Column (1) of Table 2. We find that the reform increases the probability of rating downgrading by

[21] We choose the time window to be one year because rating agencies typically track the firms on an annual basis. In the data, the median and mean time gap between two successive LGFV rating date is 12 months and 16 months, respectively.

2.38% ($= 0.171 \times 0.14$), which is economically important compared to the average downgrading probability of 2.1%.

The IV estimate is much larger than the OLS estimate. This is probably due to the spillover effect between LGFVs in the same city. As all LGFVs in the same city are ultimately supported by the same local government, liquidity constraint of one LGFV would imply similar conditions of other LGFVs in the same city. The judicial reform status would capture the effect of information production about all LGFVs in the same city. [22]

To check if the negative effect of losing lawsuits on credit rating only exists for cases informative of government financial conditions, we also construct LGFV-year pairs such that there are no lawsuit cases featuring the LGFV's payment delays in that year and calculate $winrate_{ict}$ as the average win rate for all cases for the LGFV i in year t. As reported in Columns (5)-(6), we do not find any significant effect on the LGFV's future credit rating after the LGFV loses in these non-informative cases. This result provides further support for the information production hypothesis of court rulings, rather than that the lawsuit losses per se lead to future downgrading.

Heterogeneity with Fiscal Transparency. Information production of the court should be more important in less transparent cities, i.e., cities with less self information disclosure. We first measure city-level information disclosure by LGFVs using the share of MCBs issued by all LGFVs in the city that have a bond rating at the time of issuance. The bond rating reports typically convey detailed information about the investment projects, such as construction cost, other financing sources, and government financial support. Therefore, more rating reports disclose more information to outside investors. During our sample period, about one quarter of the MCBs were issued without a bond rating.We divide the cities into two groups based on whether the share of MCBs with bond rating is greater than 50% and repeat the same 2SLS estimation using cases with LGFV

[22]Formally, suppose there are two LGFVs, $i \in \{1,2\}$, in the same city, and $downgrading_1 = \beta_1 winrate_1 + \beta_2 winrate_2 + \epsilon$, $winrate_i = \gamma_i reform + \varepsilon_i$ and $Cov(\epsilon, winrate_i) = Cov(\varepsilon_i, reform) = 0$ for any $i \in \{1,2\}$. Then:

$$\beta_1^{OLS} = \beta_1 + \beta_2 \frac{\gamma_2}{\gamma_1 + \sigma_1^2/(\gamma_1 \sigma_z^2)},\ \beta_1^{IV} = \beta_1 + \beta_2 \frac{\gamma_2}{\gamma_1}$$

When $\beta_1\beta_2 > 0$ and $\gamma_1\gamma_2 > 0$, we have $|\beta_1^{IV}| > |\beta_2^{OLS}|$.

Table 7: Response of Credit Rating to LGFV's Win Rate

Panel A: Judicial Reform and Information Production of Courts

Dep Var: Rating Downgrading	(1)	(2)	(3)	(4)
Spec.	OLS	IV	OLS	IV
Cases with LGFV Payment Delays:	Yes	Yes	No	No
Win Rate	-0.016***	-0.171***	-0.004	0.003
	(-3.04)	(-2.37)	(-0.42)	(0.03)
		First Stage		First Stage
Reform		-0.140***		-0.160***
		(-7.43)		(-2.54)
F-statistics		71.05		12.39
Observations	6,687	6,972	8,63	1,265
Mean of Outcome	0.021	0.021	0.009	0.009
Year FE	Yes	Yes	Yes	Yes
LGFV FE	Yes	Yes	Yes	Yes

Panel B: Heterogeneity by City Fiscal Transparency

Dep Var: Rating Downgrading	(1)	(2)	(3)	(4)
Cities with Higher Fiscal Transparency:	High	Low	High	Low
Win Rate	-0.092	-0.212**	-0.110	-0.183*
	(-3.04)	(-2.37)	(-1.29)	(-1.81)
		First Stage		
Reform	-0.160***	-0.125***	-0.145***	-0.134***
	(-5.90)	(-5.13)	(-5.09)	(-5.45)
F-statistics	41.88	30.35	28.78	42.27
Observations	3,068	3,619	2,766	4,355
Year FE	Yes	Yes	Yes	Yes
LGFV FE	Yes	Yes	Yes	Yes

Notes: This table examines whether an LGFV is more likely to experience credit rating downgrading in the next year after losing lawsuits around its payment delays. Panel A Columns (1)-(2) focus on LGFVs with payment delay cases, while Columns (3)-(4) focus on LGFVs without payment delay cases. In Panel B, we explore the heterogeneity of city-level measures of fiscal transparency. We report results of a two-way fixed effect model using both OLS and IV estimation with the judicial reform status as the instrumental variable. Standard errors are clustered at city level. Robust t-statistics in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

payment delays. We report the estimation results in Column (1)-(2) in Panel B. Consistent with our prediction, the effect is quantitatively larger and only significant in cities with less information disclosure by LGFVs.

To check robustness, we replicate this exercise using the City Government Fiscal Transparency Index, which is published by the Center for Public Economics, Finance, and Governance Research under Tsinghua University School of Public Policy and Management. The index is constructed based on whether the govern-

ment publishes information on its various sources of income, expenditures, debt balance and repayment, state-owned capital, organizational structure, etc.[23] We use the 2014 Index which covers information disclosure for 2012-13, right before the implementation of the judicial reform. We divide the cities into two groups based on the index and repeat the same 2SLS estimation using cases with LGFV payment delays. As shown in Columns (3) and (4) of Panel B. we continue to find stronger effect for less transparent cities, confirming that the court's information production is indeed more critical in less transparent cities.

Shut Down Information Production. The information production hypothesis works through lawsuits with LGFV payment delays. We find that when information production is shut down, the effect of the judicial reform on LGFVs' financing becomes much smaller and insignificant. This result lends further support to our claim that the effect of the reform must go through the lawsuits, in particular lawsuits with LGFVs' payment delays.

Specifically, we shut down the information production channel by selecting sub-city jurisdictions for which there are no affiliated LGFVs that ever experienced lawsuits due to their payment delays during our sample period.[24] Since there are no lawsuits with LGFV payment delays, there will be no information production. We select jurisdictions rather than LGFVs because information production of one LGFV can spill over to other LGFVs affiliated with the same jurisdiction, even if they do not have lawsuits by themselves. We then repeat the estimation in Table 5 using LGFVs affiliated with the selected sub-city jurisdictions. Table 8 shows that without information production through court rulings, the reform does not have any significant impacts on the balance sheet of LGFVs.

Note that we do not take Table 8 as evidence against the other two channels because the other two channels are also largely shut down on this selected sample. Therefore, more precisely, Table 8 provides evidence that the effect of the reform on LGFVs' financing must go through their lawsuits, rather than capturing the effect of some omitted reforms or economic shocks.

[23] See the website of the Center for Public Economics, Finance, and Governance Research.

[24] A sub-city is defined as a county, a county-level city, or a union of all districts within the same prefecture-level city. Each prefecture-level city consists of multiple sub-cities, who enjoy some extent of fiscal autonomy.

Table 8: Effect on LGFVs' Balance Sheet in Jurisdictions without LGFV Lawsuits

	(1)	(2)	(3)	(4)	(5)
Dep Var:	size	borrowing rate	Bond/Asset	leverage	AccPayable/COGS
Reform	0.0264	-0.339	0.00584	0.0124	0.00980
	(0.380)	(-0.924)	(0.751)	(1.004)	(0.564)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	2,692	2,216	2,689	2,689	2,674
R-squared	0.8786	0.6072	0.4991	0.7439	0.5458

Note: This table shows the effect of the judicial reform on LGFVs affiliated with jurisdictions where there are no lawsuits involving LGFVs' payment delays. Standard errors are clustered by cities. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.5 Further Discussion

Judicial Reform and Court Information Production Efficiency. We argue that as the reform decreases the LGFVs' win rate, it essentially allocates some LGFVs from the lawsuit-winning and relatively safe pool to the lawsuit-losing and relatively risky pool, resulting in tightening of credit supply to these marginally affected LGFVs. How about credit supply to LGFVs whose lawsuit outcomes are not marginally affected? Does the reform improve court information production?

We shall highlight that the reform does *not* necessarily improve the court information production because those marginally affected LGFVs include both good and bad types. The good type is also affected because they can delay payment for reasons that turn out not to be supported by the court. Therefore, the effect of the reform regarding information production is conceptually different from papers that study the effect of improved information disclosure. Depending on the share of good and bad types among the marginally affected LGFVs relative to the lawsuit-winning and lawsuit-losing LGFVs, the change in average credit risk of the lawsuit-winning and lawsuit-losing LGFVs is ambiguous, so is the credit supply to the non-marginally affected LGFVs. What is certain is reduced debt capacity and higher borrowing costs of marginally affected LGFVs. Our results suggest that the effect on these marginally affected LGFVs dominate the overall impact.

General Equilibrium Effect. In addition to affecting lawsuits involving LGFVs, one may be concerned that the judicial centralization reform has multiple effects on the local economy, some of which may be related to the financing of LGFVs or explain our findings. In particular, Liu et al. (2022) shows that alleviating local court capture can increase investment from external cities. First, more private investment should increase demand for local infrastructure and public facilities, thereby increasing demand for services provided by LGFVs. This should predict more issuance of MCBs and higher growth in LGFV asset size, which is opposite to our findings. Second, a larger number of firm entries may increase the demand for credit and crowd out the credit supply to LGFVs. This may adversely affect LGFVs' bank loans but not likely their bond issuance because the bond market is highly integrated as most bond investors are national financial institutions. In general, alleviating local court capture typically leads to economic advantages, which is unlikely to undermine LGFVs' borrowing capacity.

5.6 Real Effect

Finally, we provide some evidence on the real impact of the reform through the LGFVs. The reform has multiple effects on the real economy through various channels as we discuss above. In this section, we focus on the activities of LGFVs which are directly linked to their financing capacity and less affected by the general equilibrium effect through other channels.

We first investigate how the judicial reform affects LGFVs' spending. We measure their spending with cash paid for goods, services and investment. We then estimate Equation (8) using the LGFVs' spending as the outcome variable.

Table 9 shows that following the reform, LGFVs' annual spending decreases significantly by about 22.2%. This effect is more pronounced for spending on goods and services than for investment. Figure A.5 in the appendix presents results of the event study analysis, which shows parallel trends between treated and control LGFVs before the reform and a significantly negative and gradually increasing effect over time post-reform. This result implies that the judicial reform has either undermined local governments' capacity to offer public goods or curtained inefficient excessive government investment, depending on the productivity of the marginal government spending during our sample period.

Table 9: Effect on LGFVs' Spending

	All	Goods&Services	Investment
Dep Var: log(Cash Paid)	(1)	(2)	(3)
Reform	-0.222***	-0.229***	-0.122**
	(-3.574)	(-3.094)	(-2.051)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	27,884	27,365	27,865
R-squared	0.7065	0.6407	0.6743

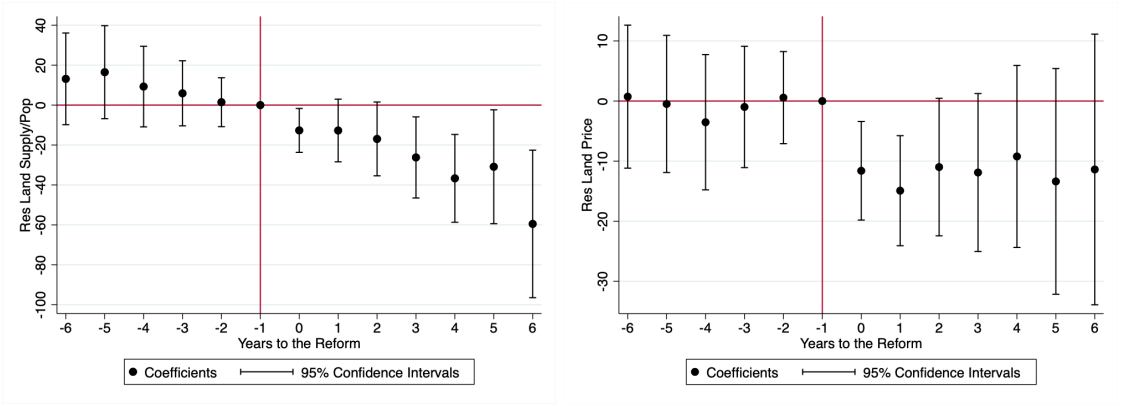
Note: This table shows how the judicial reform has affected LGFV-level spending measured with its annual cash paid for goods&services and investment. Standard errors are clustered by cities. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

One of the most important roles played by LGFVs is as "land banks." LGFVs typically invest in producing land inventories, especially residential land inventories, for local governments to sell. Specifically, they will take either unoccupied or occupied land parcels (after compensation to incumbents), level the ground, and connect the land to main roads and utility services (such as drainage, water supply pipelines, electricity, gas, telecommunications, and the Internet) to prepare it for sale on the market. The production of land inventories is costly and LGFVs play an important role in transforming urban landscapes.

To study whether decreased spending affects the role of LGFVs as land banks, we use land auction data from the Ministry of Natural Resources and aggregate land sale at the city level to calculate the annual land supply and average land price (weighted by the land parcel size) for each year. We then estimate Equation (6) using the annual land supply per capita and land price scaled by GDP per capita as the dependent variables.

Figure 8 present the event study results for residential land.[25] The judicial reform has gradually decreased the city's new residential land supply over time. Five years after the reform, the annual residential land supply decreased by 30 square meters per 100 urban residents, which is economically important compared to the mean of 147 square meters per 100 urban residents. Meanwhile, the average

[25]We present the results for commercial and industrial land in Section A of the Appendix. Consistent with the fact that the LGFVs' role is mainly in producing residential land inventories, we do not find significant impact on the supply of commercial and industrial land.



(a) Residential Quantity (b) Residential Price

Figure 8: LGFVs' Debt Capacity and Residential Land Supply

Note: This figure plots the 95% confidence interval of the effect of the judicial reform on the city-level residential land supply per capita (square meters per 100 urban residents) and land price scaled by GDP per capita. Standard errors are clustered by cities.

residential land prices also dropped significantly after the reform. Translating the coefficient estimates into dollar value, we find that the average residential land prices decrease by about 310 RMB per square meter after the reform, representing approximately a 10% decrease in the average prices.

The simultaneous reduction in quantity and price represents both a shift in the quantity of land supply and a shift in the quality of land parcels. That is, with reduced spending induced by the reform, LGFVs not only cut the production of land inventories but also reduce the amount of investment on facilities for each land parcel, which lowers of quality of land parcels. Accordingly, the local governments respond by both reducing land supply and accepting lower prices for transacted land parcels.

6 Conclusion

We provide novel evidence on how local court capture at the grassroots level affects municipal financing and spending. Courts captured by local governments may help conceal information regarding the local government liquidity constraint by supporting their defense for payment delays. We find that the judicial centralization reform in China has reduced the borrowing and spending of local governments. To

the extent that local governments have strong incentive to over-spend (Xiong, 2018), the reform is likely to have put a harder constraint on local government borrowing and improved efficiency of government spending. In this sense, the judicial reform has improved the role of market forces in disciplining local governments' borrowing behavior.

In some sense, judicial centralization enhances fiscal centralization by constraining local governments' ability to raise off-budget funding. Notably, Cantoni et al. (2024) show that after centralizing their fiscal administration in Chambers, territories became more likely to survive, increased in size, and achieved a more compact extent. Therefore, our findings indicate a fruitful future research direction to investigate the impact of judicial reforms on central-local fiscal relations and state capacity in the modern era.

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Supplemental Appendix

A Supplementary Empirical Results

A.1 LGFVs' Win Rates

Figure A.1 presents alternative event-study estimators for the effect of the reform on local LGFVs' win rates as defendants and local plaintiffs following the approach suggested by Borusyak et al. (2021).

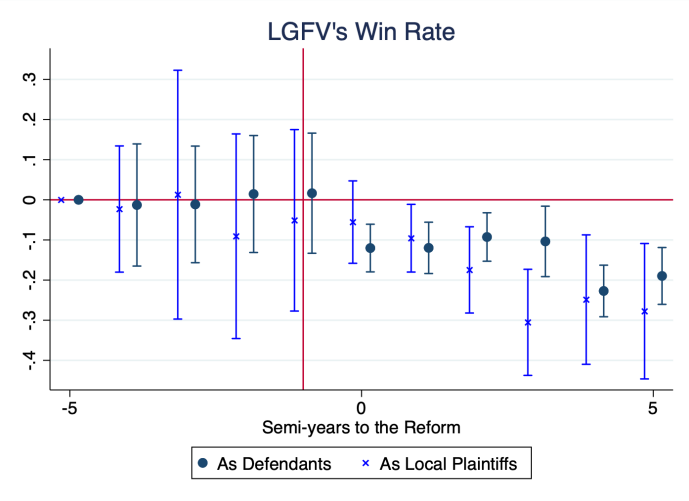


Figure A.1: Alternative Event Study Estimation

Note: In this figure, we plot the event study coefficients (and the corresponding 95% confidence intervals) for the baseline results in Columns (1) and (6) of Table 2 following the approach suggested by Borusyak et al. (2021).

A.2 Placebo Tests

We interpret the effect of the reform on LGFVs' win rates as causal. One concern is that there could exist some local economic shocks that are both correlated with the status of the reform and affect LGFVs' win rates. To provide sharper

evidence to address this concern, we delve into two placebo tests by focusing on two specific subsamples: a) cases involving financing vehicles controlled by a provincial or the central government and b) cases in which LGFVs are plaintiffs filing lawsuits against external defendants (and hence are handled by courts in their counterparties' jurisdictions).

For cases involving provincial or central-level financing vehicles, we anticipate two potential effects of the reform: on the one hand, these high-level financing vehicles are likely unaffected by the reform because, unlike locally-controlled LGFVs, they are not directly connected to local governments; on the other hand, the reform increases provincial governments' control over local courts, potentially pressuring them to provide biased rulings favoring these financing vehicles even more post-reform. For cases handled in external courts, the reform in the LGFVs' cities should not have any impact on the lawsuit outcomes. Note that the alternative explanation based on local economic shocks should predict similar effects for these two subsamples.

Figure A.2 shows the dynamics of the treatment effect for these two samples by plotting the event study coefficients obtained from estimating Equation (2). There are no changes in the win rates of provincial- or central-level financing vehicles after the reform, or in cases handled by external courts. This finding indicates two things. First, the baseline result is specific to the reduction in courts' protectionism, which favors local governments, rather than other factors affecting all plaintiffs and defendants. Second, despite granting more power to provincial-level governments, the reform does not seem to result in greater protectionism toward these provincial-level LGFVs.

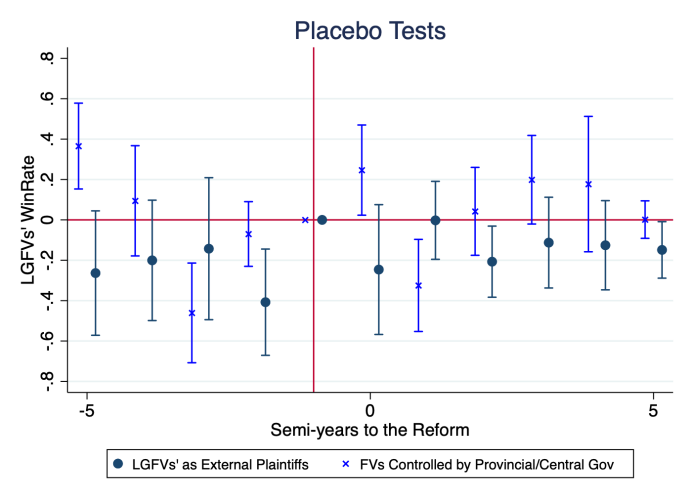


Figure A.2: Placebo Tests

Note: In this figure, we plot the event study coefficients and the corresponding 95% confidence intervals for: a) cases involving LGFVs controlled by provincial or central government; and b) cases where LGFVs are plaintiffs filing lawsuits against external defendants, which are handled by courts in the counterparts' jurisdiction. All event studies are estimated following the approach suggested by Sun and Abraham (2021).

A.3 Intensive and Extensive Margin

Our baseline findings regarding the win rates of LGFV defendants using case-level data represent a mixture of intensive and extensive margin adjustments. First, on the intensive margin, the judicial centralization reform may alter incentives of judges, leading to a scenario where identical cases are adjudicated differently pre- and post-reform. Second, on the extensive margin, after noting the effects on the intensive margin, plaintiffs may adjust their litigation strategies (e.g., firms may become more inclined to file lawsuits against LGFVs), thus shifting the composition of LGFV-related lawsuits. In this section, we delve into these two aspects separately and find that the intensive margin predominantly influences our baseline findings. Changes in case composition appear to counteract our baseline results, as the reform has emboldened plaintiffs who were previously less likely to win the case to initiate litigation.

First, to examine the effect on the intensive margin, we focus on a subset of

Table A.1: Composition of Cases Received Rulings Before and After Reform

	Regis. Capital (Million CNY)		# of Employees		Age	
	Plaintiff	Defendant	Plaintiff	Defendant	Plaintiff	Defendant
	(1)	(2)	(3)	(4)	(5)	(6)
Ruling After Reform	-18.923	18.385	91.895	327.533	-5.574	-0.697
	(-0.499)	(0.093)	(0.311)	(0.578)	(1.637)	(0.528)
Court FE	Y	Y	Y	Y	Y	Y
Semi-year FE	Y	Y	Y	Y	Y	Y
Observations	426	1,554	467	1,789	480	1,798
R-Squared	0.754	0.757	0.677	0.678	0.723	0.695

Notes: Standard errors are clustered by cities. Robust t-statistics in parenthesis. * significant at 10% ** significant at 5% *** significant at 1%.

lawsuits initiated within six months prior to the implementation of the reform. We then compare the outcomes of these cases based on whether the rulings were issued before or after the reform. As indicated in Table A.1, this approach ensures a consistent case composition: the characteristics of plaintiffs and defendants in cases adjudicated before and after the reform are well balanced. Thus, by comparing these two groups of cases, we can effectively eliminate the influence of the extensive margin. As presented in Table A.2, for the identical court and judge, decisions rendered immediately before the reform were significantly more favorable toward LGFV defendants than those made shortly after the reform. The magnitude of this effect is considerably greater than that observed in our baseline DID analysis, supporting the conclusion that the changes in judges' incentives are the primary driver of the observed baseline outcomes.[26]

Second, we delve into the extensive-margin impact of the reform on the case composition. Column (1) of Table A.3 shows that lawsuits involving LGFV defendants have seen a 4.2% increase post-reform.[27] This finding aligns with the notion

[26]This analysis of changes in judges' behavior aligns conceptually with Ash et al. (2022).

[27]On average, LGFVs are defendants in 76% of the cases involving either LGFVs as defendants

Table A.2: Intensive Margin - Conditional on Case Composition

Dep Var: win rate	(1)	(2)
Reform	-0.159**	-0.189**
	(-2.24)	(-2.33)
Mean of Outcome	0.55	0.55
Court FE	Y	N
Semi-Year FE	Y	Y
Judge FE	N	Y
Observations	1,588	1,554
R-Squared	0.579	0.615

Notes: This table focuses on the subset of cases that were filed within six months before the local court adopted the reform and compares the rulings made before and after the reform. Standard errors are clustered by cities. Robust t-statistics in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

that the reduced judicial protection of LGFVs has emboldened plaintiffs to pursue legal action against LGFVs. Beyond the sheer volume of cases, the characteristics of the plaintiffs involved in these lawsuits against LGFVs have also noticeably shifted. As detailed in Columns (2)-(4) of Table A.3, after the reform, the plaintiffs are characterized by 25.4% less registered capital, 55.4% fewer employees, and 7.5% younger in terms of firm age. These findings suggest that before the reform, many smaller plaintiff firms may have been deterred from suing LGFV firms due to the low perceived likelihood of success. The reform has encouraged such firms to initiate lawsuits, leading to a significant shift in the case composition.

The extensive margin effect results in more lawsuits featuring weaker plaintiffs against LGFV defendants. Given that these cases typically exhibit lower win rates for the plaintiffs—a factor contributing to their reluctance to initiate such lawsuits prior to the reform—the inclusion of these cases in the overall composition introduces a downward bias in our baseline estimates. This observation aligns with the finding that the intensive margin effect, which is not influenced by this

or plaintiffs.

Table A.3: Extensive Margin - Changes in Case Compositions After Reform

	LGFV's lawsuits	Plaintiff's Characteristics		
	percentage of being sued	Regis. Capital (M)	# of Employees	Firm Age
	(1)	(2)	(3)	(4)
Reform	0.042***	-18.196**	-87.370**	-0.983*
	(3.50)	(-1.99)	(-1.99)	(-1.91)
Mean of Outcome	0.76	71.29	157.74	12.18
Court FE	Y	Y	Y	Y
Seimi-year FE	Y	Y	Y	Y
Observations	16,773	16,133	17,602	17,857
R-Squared	0.608	0.260	0.272	0.264

Notes: This table reports the impacts of the judicial organizational reform on the composition of commercial lawsuits against LGFVs. Column (1) presents the DiD estimate for the share of LGFV defendant lawsuits over all cases involved LGFVs in each court for each semi-year period. Columns (2), (3), and (4) present the DiD estimates on plaintiffs' registered capital, number of employees, and firm age, respectively. Standard errors are clustered by cities. Robust t-statistics in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

shift in case composition, is significantly more pronounced than the baseline effect which represents both intensive and extensive margin effects.

A.4 Potential Side Effects

The effect of the judiciary not only depends on the court decisions per se, but also on the trial process and the enforcement of court decisions. In this section, we investigate whether the reform has (a) led to prolonged delays in issuing rulings (Section A.4.1); or (b) diminished the rate of compliance with court orders (Section A.4.2).

A.4.1 Delays in Court Decisions?

Despite a notable decrease in favoritism toward local governments, the reform may have led to slower judicial decisions if judges encountered a trade-off between the

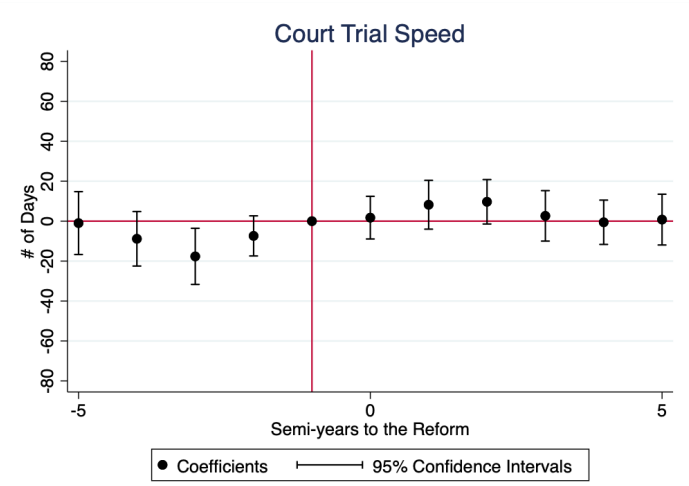


Figure A.3: Judicial Centralization Reform and Trial Speed

Note: In this figure, we estimate the event study specification (Eq. 2) using the number of days taken for each case from start to finish as the outcome variable. We plot the event study coefficients (and the corresponding 95% confidence intervals), respectively. All event studies are estimated following the approach suggested by Sun and Abraham (2021).

quantity and quality of case handling. To explore this possibility, we estimate the changes in the duration of a case (number of days from filing to verdict) in response to the reform. As illustrated in Figure A.3, the duration of the trials involving LGFVs has not changed significantly. This finding suggests that local judges have managed to enhance the quality of judicial decisions without compromising quantity.

A.4.2 Weakened Enforcement of Court Orders?

Given the greater political distance between provincial governments and local law enforcement agencies, one concern is that the degree of enforcement of rulings against LGFV defendants may diminish. Should this occur, the significant reduction in the judicial favoritism toward LGFVs may not translate into meaningful effect on LGFVs. However, our empirical analysis does not support this concern.

Table A.4: Judicial Reform and Ruling Enforcement

	Non-compliance Rate	Complete Non-compliance	Partial Non-compliance
	(1)	(2)	(3)
Reform	-0.005	-0.006	0.006
	(-0.833)	(1.000)	(1.000)
Court FE	Y	Y	Y
Seimi-year FE	Y	Y	Y
Observations	53,893	53,893	53,893
R-Squared	0.170	0.167	0.167

Notes: Standard errors are clustered by cities. Robust t-statistics in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

We employ a unique dataset from *Credit China*, which records every instance of non-compliance with court orders and labels the non-compliant party as a "dishonest debtor subject to enforcement." By integrating these non-compliance data with all commercial lawsuits involving LGFVs in our dataset, we assess the quality of judicial enforcement before and after the reform. As indicated in Table A.4, the rate of non-compliance has remained statistically unchanged following the reform. Moreover, when we further categorize the outcome variable into "partial non-compliance" and "complete non-compliance," as classified by *Credit China*, the results still show no significant difference. These findings do not support the hypothesis of weakened enforcement.

A.5 Superior Government Support

To examine whether the judicial reform is driven by or related to a reduction of political support from upper-level superior governments, we first look at two major forms of financial support: fiscal transfer and allocation of municipal debt quota. Figure A.4 shows the event study results about the effect of the reform on the size of fiscal transfer granted to local governments scaled by their fiscal

revenue in 2013. There is no significant difference in fiscal transfers between the reform and control cities either before or after the reform. Panel A of Table A.5 reports the effect on the size of local government debt quota and debt balance scaled by their fiscal revenue in 2013. With the introduction of municipal bonds in 2015, the central government imposes a quota management system on the debt balance outstanding. That is, the total quota is first determined at the national level, and then each administration can allocate its quota among all its subordinates. Therefore, local governments more favored by superiors may receive larger debt quota. The first two columns report the Difference-in-differences estimation results after controlling for heterogeneous treatment effect following Sun and Abraham (2021). We do not find any effect of the reform on the debt quota or debt balance. Since the formal municipal debt is introduced mostly after the reform, the DID specification is likely uninformative about the effect of the reform because it only considers the change in outcome variables. In Column (3)-(4), we look at the level of municipal debt in 2017 (when data is non-missing for most cities) by conducting a cross-sectional analysis. Again we fail to find any evidence of reduced financial support in reformed cities.

Next, we look at a major form of non-financial support from superior governments – the promotion of local officials. In particular, we use the officials dataset collected by Yao et al. (2020) and consider all city party secretaries and mayors in office in 2013, right before the beginning of the reform. We define a dummy variable, *promotion*, which equals one if the official has been promoted to a senior position by 2017 and zero otherwise. Column (1)-(2) in Panel B of Table A.5 shows that if anything, the reform is associated with higher probability of getting promoted for city mayors. In Column (3)-(4), we conduct placebo test by looking at officials in office five years before (the typical political cycle in five years) and

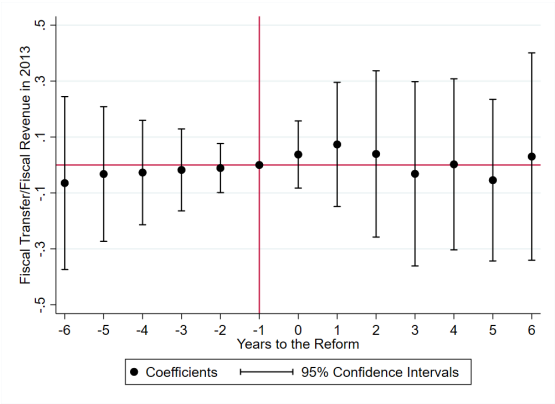


Figure A.4: Judicial Reform and Fiscal Transfer

Note: This figure plots the 95% confidence interval of the effect of the judicial reform on the local government fiscal transfer income from superior governments. Standard errors are clustered by cities.

considering their promotion status four years later. Before the reform, the future reform status is not correlated with the promotion prospect of local officials.

A.6 More Results on LGFVs' Default

Most LGFV default events happen in the recent years. As complementary evidence to the DID estimation results in Section 5.3, we conduct cross-sectional analysis in this section. We define the city-level default ratio of LGFVs as the share of our sample LGFVs that default on non-standardized debt obligations during 2016-2023. The average LGFV default rates is 2.7%, with a standard deviation of 8.8%. Conditional on the cities with at least one LGFV default event, the average default ratio is 20.5%, with a standard deviation of 15%.

We then conduct the following cross-sectional regression analysis:

$$\mathrm{Default}_c = \alpha + \beta \cdot \mathrm{Reform}_c + \gamma \cdot X_c + \epsilon_c, \tag{10}$$

Table A.5: Judicial Reform and Superior Government Support

Panel A: Debt Quota and Debt Balance

	(1)	(2)	(3)	(4)
Year	2015-23	2015-23	2017	2017
Dep Var	quota	debtbal	quota	debtbal
Reform	0.139	0.0498	-0.0284	0.0628
	(0.788)	(0.298)	(-0.117)	(0.274)
City FE	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	Yes
Observations	2527	2660	289	299
R-squared	0.859	0.859	0.000	0.000

Panel B: Promotion of Local Officials

	(1)	(2)	(3)	(4)
Year in office	2013	2013	2008	2008
Dep Var	promotion	promotion	promotion	promotion
Reform×				
Party Secretary	0.0112	-0.0469	0.0415	0.0711
	(0.177)	(-0.705)	(0.639)	(1.076)
Mayor	0.199***	0.180***	-0.0241	-0.0577
	(3.363)	(2.966)	(-0.481)	(-1.155)
Position FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Observations	616	567	613	570
R-squared	0.095	0.132	0.068	0.131

Note: This table shows whether the judicial centralization reform is associated with a reduction in superior government support for the local governments. Panel A shows that the reform is not correlated with either the change or the level of local governments' municipal debt quota and balance, and Panel B shows that the reform does not predict worse promotion prospect of local officials in office in 2013. The control variables in Panel B include official age, gender, education, and indicators for rural working and overseas experience and minority group. Standard errors are clustered by cities. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.6: Effect on LGFVs' Defaults

	LGFV Default Rate, %					
	All cities			Cities with defaults		
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	2.042**	1.961**	2.035**	9.000**	7.259**	7.242*
	(2.41)	(2.33)	(2.35)	(2.16)	(2.12)	(2.01)
GDP per capita, ln		-0.400	-0.352		-5.224**	-5.913**
		(-0.47)	(-0.32)		(-2.22)	(-2.10)
Population, ln		-0.198	-0.164		-11.163***	-11.863***
		(-0.23)	(-0.18)		(-8.84)	(-6.53)
Average LGFV leverage			-2.059			11.357
			(-0.37)			(0.52)
Constant	1.294**	3.062	3.715	13.519***	91.388***	91.698***
	(2.58)	(0.60)	(0.71)	(4.42)	(9.82)	(9.56)
Observations	310	302	295	41	39	39
R-squared	0.011	0.012	0.013	0.062	0.438	0.444

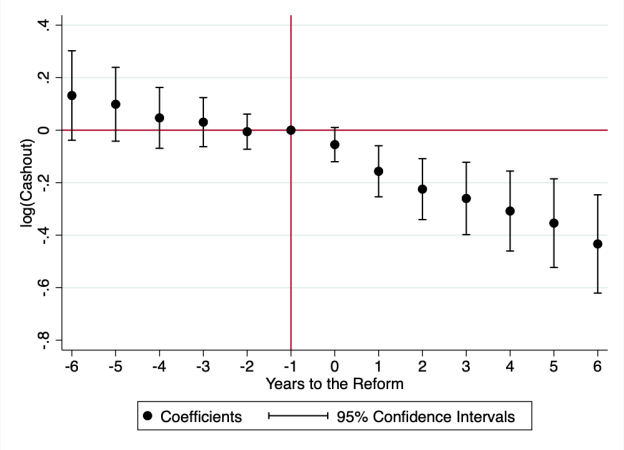
Note: This table presents the city-level cross-sectional regression results of the judicial reform's impact on LGFVs' default rate, i.e., the fraction of LGFVs with *feibiao* defaults between January 2017 and January 2024 in all LGFVs in a city. The explanatory variable, $\mathtt{Reform}$, is a dummy that equals one if a city has implemented the judicial reform by 2020 and zero otherwise. We use the full sample of 310 cities in Columns (1)-(3) and the subsample with at least one LGFV default in Columns (4)-(6). Average LGFV leverage is calculated as $leverage_c = \frac{\Sigma_i \mathrm{liability}_{ic}}{\Sigma_i \mathrm{asset}_{ic}}$. We calculate robust standard errors and present corresponding t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

where $\mathtt{Default}_c$ is the number of LGFVs with feibiao debt defaults in city c divided by the total number of LGFVs in the city, and $\mathtt{Reform}_c$ is a dummy that equals one if a city has implemented the judicial reform by the end of 2020 and zero otherwise. The coefficient of interest is β, which captures the differences in LGFVs' default rates between cities with and without the judicial reform. Our city-level control variables include GDP per capita, population, and average LGFV leverage ($leverage_c = \frac{\Sigma_i \mathrm{liability}_{ic}}{\Sigma_i \mathrm{asset}_{ic}}$), all of which take the value in 2013 which is right before the reform.

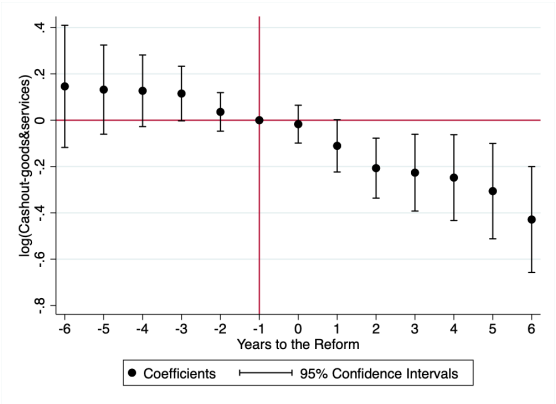
As shown in Column (1) of Table A.6, we find the LGFVs' default rate is significantly higher in cities with the judicial reforms as compared to those without the reform by approximately 2.0%. The coefficient is economically important compared to the mean of 2.7%. The coefficient estimate is robust to various controls, such as GDP per capita, city population size, and, most importantly, the average leverage of LGFVs. In Columns (4)-(6), we restrict to cities with at least one LGFV default. The coefficients increase to 9.0% without controls and 7.2% with all the city-level controls.

A.7 More Results on the Real Effect

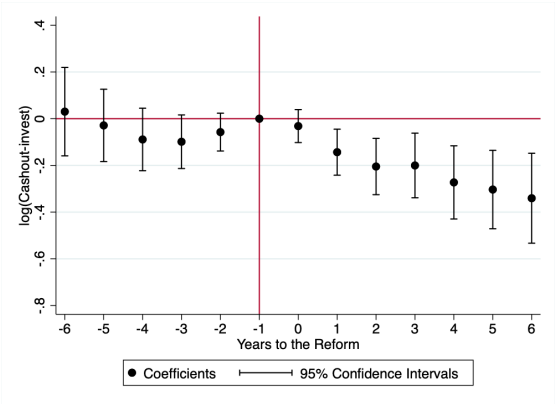
Figure A.5 presents the event study results for the effect of the reform on the LGFVs' spending. Figure A.6 presents the event study results for the effect of the reform on commercial and industrial land supply using Equation (6).



(a) All Cash Paid



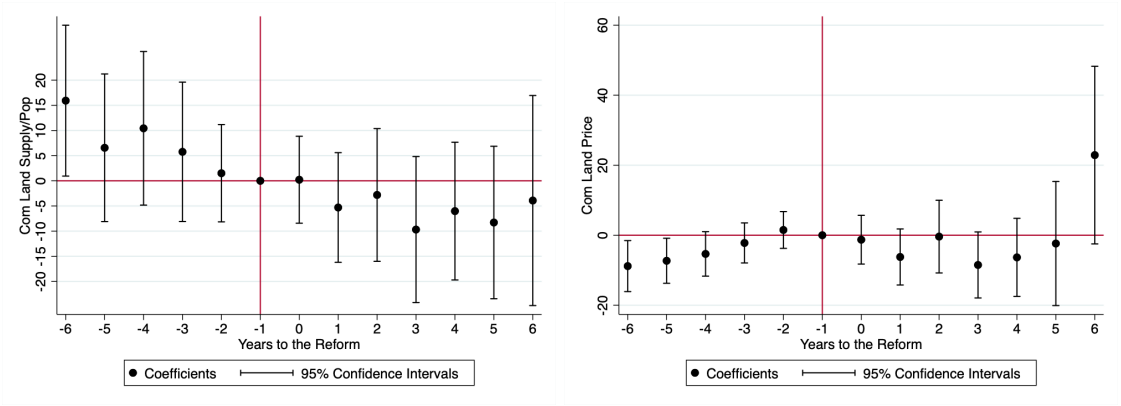
(b) Goods&Services



(c) Investment

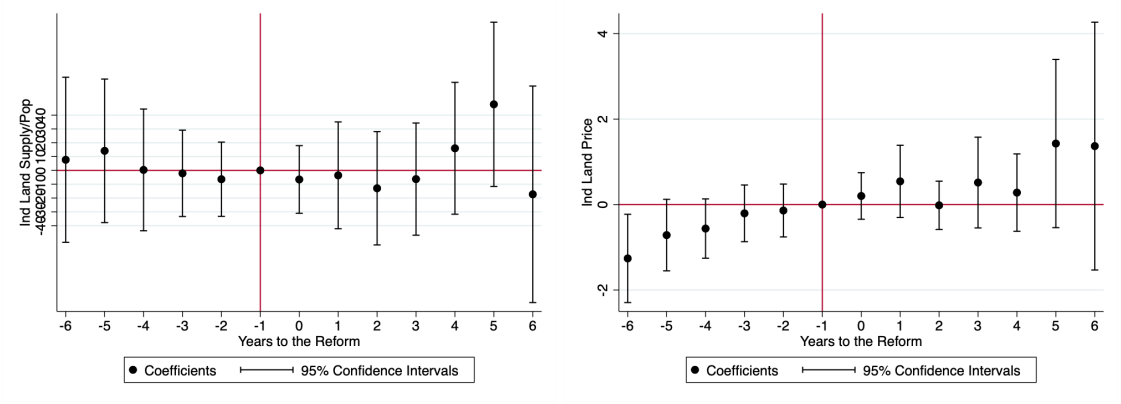
Figure A.5: The Responses of LGFVs' Spending

Note: This figure plots the 95% confidence interval of the effect of the judicial reform on the LGFVs' annual cash paid for goods and services, fixed asset, intangible asset and other long-term asset. Standard errors are clustered by cities.



(a) Commercial Quantity

(b) Commercial Price



(c) Industrial Quantity

(d) Industrial Price

Figure A.6: LGFVs' Debt Capacity and Land Supply

Note: This figure plots the 95% confidence interval of the effect of the judicial reform on the city-level commercial and industrial land supply per capita (square meters per 100 urban residents) and land price scaled by GDP per capita. Standard errors are clustered by cities.