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JEL classifications: G12, G30, H70, K40

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1 Introduction

There is a long literature that demonstrates the economic advantages of a judiciary independent of political influences (North and Weingast, 1989; Feld and Voigt, 2003; Voigt et al., 2015). Ultimately, an independent judiciary will adjudicate disputes involving governments in a more impartial manner, thereby providing a credible signal of government discipline and adherence to contractual obligations (Klerman and Mahoney, 2005; Liu et al., 2022). In reality, however, the degree of judicial independence varies substantially across the globe and political capture of judiciaries is substantially more common in developing economics and civil law countries (Glaeser and Shleifer, 2002; Porta et al., 2008). Judicial independence is particularly important when addressing matters of public indebtedness, as an independent judiciary makes the governments less likely to strategically default on debt made with creditors (Dove, 2018). However, there has been little rigorous research on how court capture affects the government ex-ante borrowing capacity and spending. This paper tries to fill this gap.

One view argues that judicial independence should enhance governments' borrowing capacity by deterring strategic default ex post. However, this view overlooks two aspects of government interactions with counterparties. Firstly, governments have multiple counterparties, such as bond creditors and contractors. The judiciary's discretion may vary with different contracts, resulting in more discrimination against one group of counterparties to the advantage of other less-discriminated counterparties. Secondly, in the spirit of financial contracting, the judiciary's role can be thought of as altering the state contingent payoff to different parties. Since the ex-ante contracts can be designed to fully counteract the judges' decisions in a risk-neutral world without frictions, it is unclear how and to what extent an independent judiciary matters for the government borrowing capacity.

In this paper, we exploit the staggered roll-out of China's high-stakes judicial organizational reform aimed to alleviate local court capture and analyze the impact on local governments' debt capacity and borrowing costs. We find causal evidence supporting that alleviating court capture mitigates court bias in favor of local governments. Given that disputes between governments and contractors are much more prevalent than with other creditors, the reform inadvertently *disadvantages* creditors especially bond creditors and leads to a material negative

effect on the governments' ex-ante borrowing capacity. This occurs despite the fact that contractors and suppliers offer slightly lower prices to governments in anticipation of a fairer legal environment. Therefore, alleviating court capture has unintentionally weakened the local governments' borrowing capacity in China.

China provides an ideal setting to study the effect of court capture on municipal debt capacity. First, with China's high level of economic decentralization (Xu, 2011), judicial local protectionism in commercial lawsuits used to be ubiquitous (Gong, 2004; Xu, 2011). Since 2014, the Supreme People's Court of China (SPC) has been gradually rolling out a high-stakes judicial reform that aims to alleviate court capture by taking away local governments' financial and personnel controls over local courts. This reform has been widely recognized by both judges, legal scholars and economists to have significantly transformed China's judicial system (Zhang and Ginsburg, 2019; Liu et al., 2022). Second, since the Great Financial Crisis of 2007-2008, local governments have increasingly relied on the issuance of Municipal Corporate Bonds (MCBs) through the Local Government Financing Vehicles (LGFVs) to finance their expenditure especially infrastructure investment (Chen et al., 2020). Although there has not been any default of MCBs, fear of MCB default always exists and there is substantial variation in yield across different MCBs. LGFVs have recently defaulted on debt with other creditors and fear of upcoming MCB default has intensified.¹

We combine four distinct datasets to obtain a comprehensive understanding of the effects of alleviating court capture on municipal debt capacity. We start from the universe of MCBs issued by local LGFVs by the end of 2023. With this list of local LGFVs that ever issued bonds, we download their financial statements, and all the court verdicts with these LGFVs as either plaintiffs or defendants. In total, 2144 out of the 3201 local LGFVs are matched to at least one court verdict and the average number of verdicts involving these LGFVs is 33, much higher than that of non-LGFV firms. Finally, we merge all these local LGFVs to a sample of value-added tax (VAT) invoices, which covers the universe of VAT invoices for about 849 contractors and suppliers (excluding grocery stores, hotels and restaurants) during 2016-2023. In total, 1475 out of the 3201 local LGFVs are matched with at least one VAT invoice with these contractors and suppliers.

¹See several reports from [Financial Times](#), [Wall Street Journal](#) and [Reuters](#).

We start by showing that the judicial organizational reform that alleviates court capture significantly mitigates court bias in favor of LGFVs. During our sample period, LGFVs' average pre-reform win rate is 0.59 of the lawsuits as plaintiffs and 0.55 as defendants. We find that following the reform, the win rate of local LGFVs dropped by 16.2 percentage points when against external plaintiffs, by 10.0 percentage points when against local plaintiffs. The magnitude of the effect is larger for external than local counterparties because external firms suffer from additional local protectionism bias before the reform (Liu et al., 2022). We also find that the decline in local LGFVs' win rate is quantitatively larger when against contractors with a larger contract value.

As placebo tests, we find no significant changes in the win rates of local LGFVs when facing external defendants. This is because such cases are typically handled by courts in the defendants' cities. Similarly, we find no significant changes in the win rates of national and provincial LGFVs. This aligns with the fact that the judicial organizational reform decouples local courts from local governments by increasing provincial governments' control over local courts.

We then break down the baseline effect of judicial reform on LGFVs' win rate into both extensive and intensive margins. On the one hand, we might expect that—this pertains to the intensive margin—given the same composition of cases (i.e., the same defendants and plaintiffs in similar cases), judges' incentive to favor LGFVs would decline after the reform, leading to a reduction in LGFVs' win rate. On the other hand, with reformed courts being fairer towards LGFVs' counterparts in lawsuits, the reform might encourage those who were marginally losing in lawsuits before the reform to bring lawsuits against LGFVs after the reform. We find empirical evidence that aligns exactly with these two hypotheses in our data. First, by focusing on a subset of cases that were filed six months before the reform and comparing LGFVs' win rate in cases that reached a verdict before and after the reform—thus holding the case composition constant—we observe a much greater effect. Second, we observe the reform encouraging smaller, less powerful counterparts to file lawsuits against bigger and more powerful LGFVs. This change on the extensive margin in case composition could potentially bias our baseline estimates downwards, which is consistent with the results showing that the estimates of the intensive margin are much larger in magnitude than our baseline estimates.

The composition of the LGFVs' lawsuit counterparties is crucial for understanding the ex-ante effect of court capture. The vast majority of these counterparties are business partners, including contractors and intermediate input suppliers, rather than creditors such as banks and bond investors. In our data, over 93.67% of the lawsuits involving LGFVs are filed by or against their business partners. As the reform benefits these contractors and suppliers, it unintentionally disadvantages the creditors in case of lawsuits. We will revisit this point later.

The contractors and suppliers are aware of the more favorable legal environment after the reform, and as a result, the LGFVs are able to source at lower prices and more from external contractors and suppliers. We draw this conclusion based on the VAT invoice data. First, we classify invoices as associated with the same product if they are created in the same year, carry the same product name and product specification, and are from the same seller. Then for the same product, we use the prices paid by non-LGFV buyers as the benchmark and calculate the LGFVs' relative price, i.e., the ratio between the price paid by the LGFV and the average price paid by non-LGFV buyers. We find that after the reform, the LGFVs' relative price dropped significantly by about 6.2%-9.4%, especially two years after the reform. Second, on the extensive margin we also find that after the reform, the LGFVs' purchase from external contractors and suppliers increases significantly by about 3.8%.

As the judicial reform favors the contractors and suppliers of LGFVs, it inadvertently disadvantages other creditors in case of lawsuits with the contractors and suppliers. Given the prevalence of the legal disputes of LGFVs, such disadvantage effect can be significant. We will first analyze the adverse effect on the bond issuance and yield at the city level, and then use the LGFVs' financial statements to study whether and to what extent the LGFVs' other credit sources are affected and how they adjust spending.

First, we aggregate all the MCBs issued by local LGFVs to construct the annual net issuance of MCBs for each city and scale it by the city government's fiscal revenue in 2014 (the year before all the reforms). We also calculate the average MCB yield weighted by the issuance amount for each city and issuance year. We find that there were no differential changes in the cities' net MCB issuance or average MCB yield before the reform. After the judicial reform, the MCB net

issuance decreased gradually, and the magnitude of the effect stabilized at around 15% since the third year after the reform. The average MCB yield also increased gradually and stabilized at around 0.25% since the second year of the reform. The fall in net issuance and rise in yield suggests a reduction in investors' demand for the MCBs. This is consistent with the hypothesis that more favorable judicial environment for the LGFVs' contractors and suppliers indirectly disadvantages the bond creditors, leading to decreased borrowing capacity ex ante.

Second, as MCB is only part of the LGFVs' credit source, we use the financial statements to study the firm-level aggregate effect. After the reform, the LGFVs' total asset decreases by about 9.3%, and there is no evidence of reversing back. Their average borrowing rate also increased by about 0.29%. As the bond financing only accounts for 8.0% of the LGFVs' asset on average, this suggests other credit sources, such as bank loans, are also adversely affected by the judicial reform. Unlike other credit sources, trade credit, measured by account payable over cost-of-goods-sold, increases by about 2.4%. The increase of trade credit is likely a joint outcome of LGFVs' tightened financial constraint and a fairer legal environment that makes business partners more willing to extend trade credit.

Next, we provide more evidence on the economic mechanism. We first examine whether, as we have conjectured, the judicial reform that favors LGFVs' contractors and suppliers inadvertently increased their debt default risk. Although there has not been any default on MCBs, LGFVs have defaulted on some non-standard debt obligations such as trust products in most recent years. By calculating the percentage of local LGFVs that have ever defaulted on these non-standard debt obligations, we find that the judicial reform has increased their average default rate by approximately 2.0% as compared to the mean of 2.7% and the standard deviation of 8.8%.

Second, why is the ex-ante decrease in prices offered by contractors and suppliers not enough to counteract the higher cost of lawsuits after the reform? We propose three non-exclusive explanations.

The first is the existence of contracts signed before the judicial reform yet not completed at the time of the reform. As the effect does not diminish six years after the reform, this is unlikely the only explanation.

Second, compared to bond investors, the entrepreneurs are likely to be more

risk-neutral and have higher discount rates. As a result, the contractors and suppliers will adjust the ex-ante prices by relative less, and bond investors will disvalue the increased ex-post uncertainty by relative more, leading to an overall increase in the LGFVs' cost.

Third, alleviating court capture may worsen the moral hazard problems of the contractors. In 40 percent of all the lawsuits against project contractors, the contractors have exhibited behaviors that represent contractor moral hazard problems, such as quality problems or project delays. Alleviating court capture may have led to more moral hazard problems, reducing the project return and thereby LGFVs' debt capacity.

Finally, the tightened debt capacity can lead to multiple effect on the local government policies. In particular, we examine how it affects the functioning of the LGFVs. First, we find a significant drop in the LGFVs' spending after the judicial reform. By aggregating all the purchase VAT invoices, we find that after the reform, the LGFVs' total spending dropped by 69%; regarding the nature of the expense, the consumption spending (e.g., groceries, hotel and restaurant) dropped by 50%, and the rest investment spending by 76%.

Second, the decreased borrowing capacity and spending has undermined their main role in producing land inventories for the local government to sell. We find that after the judicial reform, annual residential land supply at the city level will decrease by about 13.6 square meters per 100 people. The magnitude of the effect is economically important as the mean (standard deviation) of annual land supply is 59.5 (46.0) square meters per 100 people.

Literature Review. This paper is mostly related to three strands of literature. First, it adds to the long-standing literature on the economic consequences of the judicial system, especially the extent of judicial independence. While the majority of this literature has pointed out the effect, mostly advantages, of an independent judiciary free of political influences (Hayek, 2020; North and Weingast, 1989; Glaeser and Shleifer, 2002; Porta et al., 2008; Besley and Persson, 2011; Acemoglu and Robinson, 2013), there is little rigorous studies on judicial independence or court capture affects the judicial outcomes and how it shapes the behaviors of firms and governments. By exploiting the same judicial reform in China, Liu et al. (2022) finds that alleviating court capture can decrease the win rate of local

defendants against external plaintiffs and encourage more external investment. While their focus is on local versus external firms, our focus is on LGFVs versus all non-LGFVs, regardless of whether they are local or external. [Tabarrok and Helland \(1999\)](#) find that compared to appointed judges, elected state judges in the US will favor in-state plaintiffs more by making larger trial awards to redistribute wealth from out-of-state businesses to in-state plaintiffs. [Mehmood \(2022\)](#) show that in Pakistan, the change of selection procedure of judges from presidential to peer appointment reduced pro-governmental rulings.

Several papers have investigated the interplay between judicial independence and municipal finance. In the context of the US state courts, elected judges are typically believed to be more affected by local popular wills and hence less independent than appointed judges. Using the municipal default data between 1830 and 1910, [Dove \(2018\)](#) found that independent judiciary, such as those appointed rather than elected, is negatively correlated with the 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 raising more fine and fee revenues. This paper differs in two ways. First, we provide micro-level evidence on how court capture affects the judicial outcomes involving the government financing vehicles. Second, and most importantly, we exploit different groups of creditors and show how the judicial reform that benefits business partners can inadvertently hurt bond investors.

Lastly, the paper also adds to the literature on the local government financing in China. [Chen et al. \(2020\)](#) shows the rise of the municipal corporate bond market in China can be linked to the Global Financial Crisis and following four-trillion RMB fiscal stimulus ([Acharya et al., 2024](#)). As the local government debt soared since 2010, there has been increasing concern about the local government default risk ([Gao et al., 2021](#)) and potential knock-on effect on the credit access of the private sector ([Liang et al., 2017](#); [Huang et al., 2020](#); [Zhang et al., 2022](#)), on land zoning ([He et al., 2023](#)), and on factor allocation ([Wu et al., 2018](#)). We add to this literature by showing the extent of local court capture as another source of risk to local government debt and how the local government credit access affects their spending.

2 Institutional Background

2.1 The Judicial Organizational Reform

The judicial reform in China was initiated in 2013 to address concerns regarding local protectionism and judicial bias. Prior to the reform, China implemented a hierarchical judicial system where local courts and procuratorates were managed by governments within the same jurisdiction. This 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 made a comprehensive reform outline at the Third Plenary Session of the 18th Central Committee of the Communist Party of China (CPC). Regarding judicial reform, one objective is to ensure the independence and impartiality of the judicial system. Specifically, the CPC Central Committee calls for reforming the judicial management system by "promoting the unified management of personnel and budgets in local courts and procuratorates below the provincial level and exploring the establishment of a judicial jurisdiction system that is appropriately separated from administrative divisions."²

Pilot reforms have been rolled out since June 2014, when the Central Political and Legal Affairs Commission selected seven provinces and municipalities (including Shanghai, Hubei, and Qinghai, which represent eastern, central, and western China, respectively) to launch the first batch of pilot reforms. These reforms are centered on improving the judicial responsibility system, with judicial independence being one of the four pillars.

The judicial independence reform has two essential components: personnel and budgetary. The personnel reform requires that the appointment of judges in local courts be managed by the upper-level provincial governments, and the budgetary reform aims to secure funds for local courts from, again, the upper-level provincial governments.

The judicial independence reform has been steadily implemented in the past

²The 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 the 18th Central Committee of the Communist Party of China, November 12, 2013. https://www.gov.cn/zhengce/2013-11/15/content_5407874.htm.

decade. As of November 2017, 21 provincial-level governments (including provinces, autonomous regions, and municipalities) have completed the personnel reform of local courts below the provincial level, where the presidents of intermediate and primary courts have been managed by provincial-level party committees rather than lower-level governments. 13 provincial-level governments have completed the budgetary reform, where a significant fraction of funds for local courts are appropriated by upper-level governments.³

2.2 China's Local Government Debt System

The Budget Law of the People's Republic of China was promulgated in 1994 as the legal infrastructure of the seminal tax-sharing reform, a large-scale adjustment of the tax distribution system and tax structure between the central and local governments. Establishing a centralized fiscal system, the 1994 Budget Law prohibits local governments in China from financing fiscal deficits directly through banks or the financial market. Lacking the ability to issue municipal bonds, local governments with deficits had limited options to finance their expenditures and relied on transfers from the central government.

The tension between local government expenditures and financing restrictions heightened in 2008 when the Chinese government rolled out the 4 trillion fiscal stimulus plan to stabilize the economy amid the global financial crisis. A substantial funding gap emerged between local governments' fiscal revenue and stimulus expenditures. Opening alternative channels for local governments to raise funding for investment projects promoted by the stimulus package, the central government started to encourage local governments to establish LGFVs,⁴ which are essentially SOEs engaging in off-balance-sheet activities to raise funding for

³The Supreme People's Court's Report on the People's Courts Comprehensively Deepening Judicial Reform at the 30th Meeting of the Standing Committee of the 12th National People's Congress, November 1, 2017. http://www.npc.gov.cn/zgrdw/npc/xinwen/2017-11/01/content_2030821.htm.

⁴For example, in March 2009, the People's Bank of China issued guidance encouraging local governments to incentivize and motivate the banking industry to increase their credit support for the investment projects planned by the central government by establishing compliant LGFVs and supporting qualified local governments in establishing LGFVs to issue corporate bonds, medium-term notes, and other financing tools to broaden the financing channels for the investment projects planned by the central government. See http://www.gov.cn/gongbao/content/2009/content_1336375.htm.

local governments (Chen et al., 2020). Several papers (e.g., Bai et al., 2016; Chen et al., 2018; Ang et al., 2018; Chen et al., 2020) have documented the connection between the 4 trillion yuan stimulus package and the reliance of local governments on the shadow banking system to raise off-budget funding.

Different from typical municipal bonds in developed countries, bonds issued by LGFVs are corporate bonds in the legal sense but are usually backed by local governments, explicitly or implicitly. For instance, local governments would inject assets into or provide guarantees for LGFVs, hence MCBs are often backed by land and fiscal revenue. Additionally, many senior executives of LGFVs are also senior officials of the local government, which blurs the boundary between on-budget and off-budget revenues. The funds raised by LGFVs, through bond issuance or bank loans, are used to finance local governments' investment expenditures. Therefore, these corporate bonds issued by LGFVs are commonly referred to as municipal corporate bonds (MCBs) (e.g., Chen et al., 2020).

In 2015, the National People's Congress revised the budget law and authorized provincial governments to issue municipal bonds. However, MCBs issued by LGFVs are widely recognized as part of the "hidden debt" of local governments. Fitch, one of the three major international rating agencies, downgraded China's long-term local currency credit rating in 2013, partly because "the lending behavior between local governments and their platforms is not transparent."⁵ Moody's downgraded the credit rating outlook of 22 LGFVs from stable to negative in December 2023, reflecting growing concerns over "local governments' ability to deal with mounting hidden debt amid a protracted property slump."⁶

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

The extent of judicial independence is one of the central issues in the design of the judiciary system. The interaction between judicial independence and local government financing has drawn much attention in the history of the US state judiciary system.

Early state courts in the United States were typically selected through legislative

⁵See the Fitch Ratings [report](#).

⁶See the Wall Street Journal [report](#).

appointment for relatively lengthy terms. Due to concerns regarding undue influence from a state legislature and outright corruption within the judiciary, many states had begun to shift toward popular elections by the 1840s. New York set a precedent in 1846; by 2021, about three-fourths of the states select their judges through elections. Nonetheless, anecdotal evidence suggests that popularly elected state judiciaries were influenced by political realities. [Hillhouse \(1936\)](#) noted that many citizens believed any state court decision in favor of creditors was proof that the court had been “bought up”, especially in popularly elected state courts. These judicial results are generally ousted from the bench and rapidly overturn prior decisions.

One example to illustrate the relationship between state judiciary and municipal financing is from Iowa. In 1850, the Iowa state legislature passed legislation granting county indebtedness with the support of the state Supreme Court. While municipal indebtedness grew as a result, the panic of 1857 severely crippled the industry within Iowa. Simultaneously, the state Supreme Court was transformed into a popularly elected body in 1858. Faced with growing agitation against the 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 through the latter half of the nineteenth century, most of which came from states with elected state courts.⁷

Previous studies have documented the impact of judicial independence on municipal finance in the United States. [Dove \(2018\)](#) found that independent judiciaries, such as those appointed rather than elected, are negatively correlated with the municipal defaults between 1830 and 1910. There is, however, little rigorous research on the ex-ante effect on municipal debt capacity. One exception is [Dove \(2017\)](#) who finds that the extent of judiciary independence is positively correlated with bond ratings across different states.

⁷The US Supreme Court reprimanded some of these high courts in several instances. In referencing the Iowa decision, Justice Noah Swayne of the US Supreme Court suggested “We 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).

3 Data

3.1 LGFVs and Municipal Corporate Bonds

We start by downloading all the corporate bonds issued by the end of 2023 and identified by both WIND and Rating Dog as MCBs from WIND. For each MCB, WIND and Rating Dog classify the issuers based on the administrative level of the government units that control the issuers. We define the MCBs as local MCBs if the issuers are county-level or city-level LGFVs, and as non-local MCBs if the issuers are provincial or central government financing vehicles. We focus on the local MCBs in this paper. In Section 4, we use the non-local LGFVs for some placebo tests.

Panel A of Table 1 shows some summary statistics for the local MCBs. There are 3,223 unique LGFVs and approximately 37,000 MCBs. The average issuance amount is 0.735 billion RMB. The average coupon rate, which is also the bond yield as all MCBs are issued at par in practice, is about 5.11%. There is considerable variation in the bond yield as reflected by a standard deviation of 1.55%. This suggests that although there has not been any default of MCBs by 2023, investors do not think of all MCBs as safe and the extent of credit risks priced by investors varies across different bonds.

Panel B shows some key summary statistics for the LGFVs during 2011-2022. The average asset size is 24 billion RMB. On average, 17.9% of their asset is financed by loans, 8.0% by bond, and 49.8% by all kinds of liabilities. To calculate the trade credit that they receive, we divide their account payable by the sum of account payable and cash outflows from operating and investing activities. We include cash outflows from investing activities because the LGFVs typically invest in infrastructure projects which are considered as investing activities in their balance sheet. The measure of trade credit then reflects all credit the LGFVs receive from both input suppliers and project contractors. Finally, we also estimate the firm-level borrowing rate as the interest expense scaled by the sum of short-term and long-term loans and bonds. The average borrowing rate is about 2.3%, with a standard deviation of 2.6%.

Panel C shows the average default ratio of our sample LGFVs. While LGFVs have not yet defaulted on MCBs, there are defaults by LGFVs on non-standardized

debt obligations such as trust products.⁸ We obtain the full history of 5,522 default events involving non-standard debts and identify 2,676 unique borrowers.⁹ We then match these borrowers with bond-issuing LGFVs and find 73 LGFV borrowers that have defaulted or demonstrated material default risks, coming from 39 prefecture-level cities and 2 municipalities (Tianjin and Chongqing).

We define the default ratio of LGFVs as the number of bond-issuing LGFVs that have defaulted on non-standard debt obligations divided by the total number of bond-issuing LGFVs in a city. We then calculate the average default ratio as the national mean of 310 cities with at least one bond-issuing LGFV. The national average of LGFV default rates is 2.7% with a standard deviation of 8.8%, indicating the vast differences across cities. When restricting the sample to the 41 cities that have experienced LGFV defaults, we find a much larger average default ratio of 20.5%, with a standard deviation of 15%, meaning that one fifth of bond-issuing LGFVs have ever defaulted on non-standard debts in cities with at least one LGFV default event.

3.2 Universe of Court Verdicts

In 2013, the Supreme People's Court of China established the *China Judgment Online* (CJO) website to enhance judicial transparency and provide non-binding precedents for judges. This initiative mandated local courts at all levels to publish both contemporary and historical verdicts on the CJO website. While there is a backlog in digitizing and disclosing historical verdicts, local courts must 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.

We collected all court verdicts in China between 2014 and 2021 from CJO, totaling over 144 million judgment files. From this dataset, we identified 71,073

⁸In 2013, the China Banking Regulatory Committee (CBRC) issued regulations on wealth-management products issued by commercial banks and provided an official definition of non-standardized debts, or "feibiao" (Chinese for non-standardized) debts. These non-standardized debts refer to debt assets not traded on exchanges or in the interbank market, including but not restricted to bank loans, trust products, entrusted loans, bank notes, warrants, accounts receivable, and equity products with repurchase agreements.

⁹A non-standard debt product can involve multiple borrowers and a borrower can issue multiple debt products.

civil lawsuits involving LGFVs.¹⁰ In Appendix Figure A.1, we plot the time trend of the number of cases involving local LGFV defendants and local LGFV plaintiffs, respectively. For each judgment file, we extracted information such as the court in charge, trial and ruling dates, names of the judge and other court clerks, names of the plaintiff and defendant, basic facts about the case, summary of the trial process, claims made by the plaintiff, whether the plaintiff’s claims were supported by the court, and the judicial reasoning provided by the judge.

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, how court fees are shared between the plaintiff and the defendant indicates the extent to which each party wins or loses in a lawsuit, from the court’s perspective.¹¹ To measure the win rate of each side, we use the share of the opposing side’s obligation to pay court fees: $WinRate_j = \frac{CourtFee_i}{CourtFee_i + CourtFee_j}$. As shown in Panel D of Table 1, the average win rate for LGFVs as plaintiffs is 0.588, whereas it is 0.503 for cases where they are defendants.

3.3 Value-add Tax Invoices

All firms in China, including both intermediate goods producers, final goods sellers and service providers, pay value-added taxes (VAT) since 2016.¹² The VAT invoice records detailed information at the transaction level, including the name and tax ID of the seller and the buyer, the name, parameters and tax classification of the transacted goods and services, the total value, and the tax rates. For intermediate and final goods, it also records the quantity and price information.

We obtain a sample of the VAT invoices issued during 2016-2023 from a private data vendor, covering about 400 different conglomerates with about 27,000 different subsidiaries that use the platform to issue VAT invoices to about 4.7 million different customers. The total number of VAT invoices increases from 2.5 million

¹⁰We retained all court verdicts for cases tried between 2014 and 2021 and released by *China Judgments Online* before August 2022.

¹¹For example, a plaintiff who wins completely would be ordered to pay 0% of the court fees, while an even split of the fees implies that each side won 50%.

¹²Before 2016, firms in certain industries paid the “business tax” and switched to “value-added tax” after the so-called “business-to-value added” reform conducted in 2016. See [He et al. \(2023\)](#) for more details.

in 2016 to about 80.0 million in 2022.

These conglomerates will be the contractors and suppliers for the LGFVs. As the sample selection is at the sellers' side, we believe our analysis at the LGFV level is not biased due to sample selection issues. Because the prices from grocery stores, restaurants and hotels are unlikely to be negotiable and vary with the customers, we first drop all sellers whose average selling VAT invoice value is below 50,000 RMB. We then find all VAT invoices issued by the remaining sellers for which the LGFVs are the buyers. In total, 1496 LGFVs are matched with at least one VAT invoice with these selected contractors and suppliers.

Panel E of Table 1 provides some summary statistics for these matched VAT invoices. The average price of these VAT invoices is about 67,207 RMB and the average value is about 57,193 RMB. We then aggregate the VAT invoices to the LGFV-year level. On average, the LGFVs purchase 61.2% from sellers in different cities and 90.7% from sellers in different counties. The average size of their sellers, measured with contributed capital, is about 41,999 RMB and the average age is 23 years. To measure the LGFVs' spending, we also match LGFVs with VAT invoices from all sellers (including grocery stores, hotels and restaurants) and aggregate them to the LGFV-year level. The total annual spending by these LGFVs from these sellers is about 7.76 million RMB. Conditional on the invoices with product and service information, roughly 0.26 million RMB is spent for consumption (e.g., groceries and hotels) and 2.09 million RMB is spent for investment purposes.

Table 1: Data Summary

	Mean	Median	St. Dev.	Obs
A: MCB				
MCBamount, RMB Bil	0.735	0.600	0.447	37086
MCBrate, %	5.110	4.960	1.553	36781
B: LGFV				
Asset, RMB Bil	24.672	15.240	30.992	26581
Loan/Asset	0.179	0.166	0.113	26497
Bond/Asset	0.080	0.068	0.081	26497
leverage	0.498	0.518	0.162	26497
Accpayable/COGS	0.171	0.111	0.182	26242
borrowrate, %	2.340	1.328	2.644	20404
C: Non-Standard Debt Defaults				
LGFV default ratio, %, all cities	2.717	0	8.839	310
LGFV default ratio, %, cities with defaults	20.543	15.384	15.105	41
D: Court Judgement Files				
LGFV's Win Rate as PLTF	0.588	0.964	0.468	22,509
LGFV's Win Rate as DFDT	0.503	0.482	0.468	48,564
E: VAT Invoices				
Price, RMB	67207.263	59.575	2.01E+06	2.77E+07
Contract Value, RMB	57193.024	1384.8	6.02E+05	2.77E+07
difcity	0.612	1.000	0.473	4474
difcounty	0.907	1.000	0.281	4474
suppliersize, RMB	41991.221	14789.211	63189.18	4460
supplierage	23.222	22.010	9.602	4474
lgfvspend, RMB	7.76E+06	32756.08	4.48E+07	9228
lgfvspend_cons, RMB	2.63E+05	67.10	1.33E+07	9228
lgfvspend_invt, RMB	2.09E+06	0.00	2.49E+07	9228
F: Land Supply				
LandSupply/Pop, Res	59.475	45.937	52.852	5104
LandSupply/Pop, Ind	103.824	68.042	126.901	5104
LandSupply/Pop, Com	26.196	16.097	32.333	5104

Note: This table reports summary statistics. Panel A is for MCBs (as classified by both WIND and Ratingdog) issued by 2023. Panel B is based on the financial statements of LGFVs during 2010-2022. Panel C presents the ratios of LGFVs who defaulted on non-standard debt obligations in all bond-issuing LGFVs in a city. Panel D presents the win rate of LGFVs as plaintiffs and defendants, respectively. Panel E is based on the value-added tax invoices issued during 2016-2023, where the first two report the purchase invoices from the contractors and suppliers and the remaining are aggregated at the LGFV level. Panel F reports the annual land supply per capita (square meter per 100 people) at the city level.

4 Judicial Reform and LGFVs' Win Rate

We first lay out our empirical strategy in Section 4.1. In Section 4.2, we examine the impacts of the reform on the judicial outcome for LGFVs in commercial lawsuits; in Section 4.3, we present several placebo tests to show that the effects of the reform are indeed specific to court's favoring towards LGFVs; in Section 4.4, we decompose the baseline effects into "intensive margin change in judges' incentives" and "extensive margin change in case compositions;" in Section 4.5, we investigate several potential side effects of the judicial reform by examining: a) whether the judicial efficiency has changed as a result of the increased caseload of external plaintiffs against local defendants induced by the reform; b) whether the reform has weakened enforcement of court orders against local firms.

4.1 Empirical Strategy

In this section, we start our empirical analysis by exploring the impact of judicial organizational reform on judicial outcomes for LGFVs in commercial disputes. We specifically investigate the reform's influence on the extent of judicial protectionism towards LGFVs, the underlying mechanisms, and its potential effects on court congestion.

For the baseline analysis, we employ case-level data, and exploit the staggered roll-out of the reform between 2014 and 2021 to estimate the following Difference-in-Differences (DiD) model:

$$Y_{ict} = \alpha \cdot \text{Reform}_{ct} + \phi_c + \lambda_t + \epsilon_{ict}, \quad (1)$$

where Y_{it} is the win rate of LGFVs in case i at time t , where each time period is six months; Reform_{ct} is a dummy variable that equals one if court c had already gone through the reform at time t , and zero otherwise. ϕ_c and λ_t stand for court fixed effects and semi-year fixed effects, respectively. The standard errors are clustered at the prefecture level, since the reform rolled out at this level.

To understand the dynamics of the reform and gauge the validity of our DiD

design, we also estimate an event study model:

$$Y_{ict} = \sum_T \beta_{1T} T_{ct} + \phi_c + \lambda_t + \epsilon_{ict} \quad (2)$$

where T_{ct} represents the event study dummy variables: T_{ct} equals one if, at time t , T periods (6 T months) have passed since court c experienced the reform, and zero otherwise. For all the baseline event studies, 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 (i.e., [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 Baseline Impacts of the Reform

Table 2 presents the baseline DiD results, obtained from estimating Equation 1 using case-level data. As the cases are handled by courts in the defendants’ city, in Column (1)-(5) we focus on LGFVs as the defendants so that that the cases will be handled by local courts. Column (1) reveals a notable shift post-reform: local courts demonstrate a significant decrease in favorable rulings towards LGFV defendants, with their average win rate against plaintiffs dropping by 12.6 percentage points—a 22.9% decline from the pre-reform baseline win rate. We then split the sample into cases involving external plaintiffs (Column 2) and local plaintiffs (Column 3), respectively. Given the pre-reform local protectionism bias disadvantaging external plaintiffs, it’s anticipated that the reduction in LGFVs’ win rates against them would be more substantial than in cases against local plaintiffs and the results confirm this hypothesis. Furthermore, as shown in Columns (4) and (5), the reform’s effects are more pronounced in cases involving higher economic stakes and considerably smaller in cases of lower economic stakes.¹³

Columns (6) investigates the changes in LGFVs’ win rate in cases where they file lawsuits against local defendants. These cases will still be handled by local courts. Here, a negative but statistically insignificant impact on LGFVs’ win rates is

¹³For instance, disputes over construction contracts represent the most frequently observed cases with higher economic stakes, featuring an average monetary damage of 5 million CNY.

observed, with the effect size being considerably smaller compared to when LGFVs act as defendants. This suggests a strategic adjustment by LGFVs in response to the reform’s reduction of local court favoritism; they might now be more selective in pursuing litigation, favoring cases with stronger legal grounds and potentially opting for alternative dispute resolution methods, such as settlements, for weaker cases.

Table 2: Judicial Organizational Reform and LGFVs’ Win Rate

	(1)	(2)	(3)	(4)	(5)	(6)
	LGFVs as Local Defendants					Local PLTF
	All	External Plaintiffs	Local Plaintiffs	High-stakes	Low-stakes	All Cases
Post Reform	-0.126*** (0.030)	-0.162*** (0.046)	-0.100*** (0.032)	-0.261*** (0.030)	-0.068* (0.037)	-0.040 (0.047)
Mean of Outcome	0.55	0.55	0.55	0.55	0.55	0.59
Court FE	Y	Y	Y	Y	Y	Y
Seimi-year FE	Y	Y	Y	Y	Y	Y
Observations	45,681	14,389	31,292	14,355	31,326	21,099
R-Squared	0.233	0.322	0.257	0.264	0.304	0.320

Notes: This table reports the baseline DiD estimates on judicial outcomes in commercial lawsuits involved LGFV. Columns (1) to (5) focus on the average win rates of LGFVs as local defendants in cases against both local and external plaintiffs tried in local courts. Column (6) investigates the LGFV’s average win rates as local plaintiffs in cases against local defendants tried in local courts. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Figure 1 shows the dynamics of the treatment effect, by plotting the event study coefficients obtained from estimating Equation (2). For the baseline specification, we observe a flat pre-trend before the reform, and a salient reduction in local defendants’ win rate immediately after the reform. The reform’s impact is persistent over more than three years after the reform. Similarly, the event study estimates for cases where LGFVs 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 LGFVs’ win rate after the reform. For robustness, Appendix Figure A.2 presents alternative event study specifications following [Borusyak et al. \(2021\)](#), our results remain.

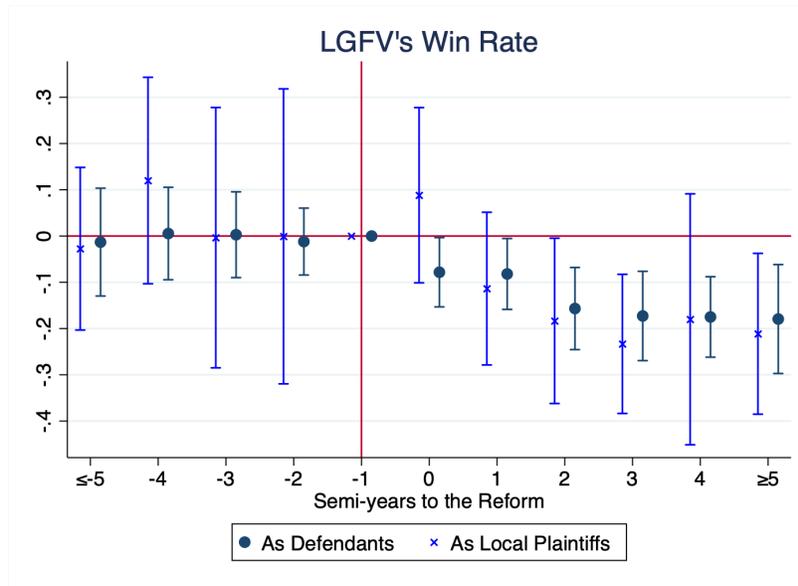


Figure 1: In this figure, we plot the event study coefficients (and the corresponding 95% confidence intervals) for the baseline results in Columns (1) and (6) of Table 2, respectively. All event studies are estimated following the approach suggested by Sun and Abraham (2021).

4.3 Placebo Tests

We then delve into placebo tests by focusing on two specific subsamples: a) cases involving LGFVs controlled by provincial or central government; and b) cases where LGFVs are plaintiffs filing lawsuits against their counterparts in courts located in the counterparts' jurisdiction. Figure 2 shows the dynamics of the treatment effect for these two samples, by plotting the event study coefficients obtained from estimating Equation 2, respectively.

For cases involving provincial or central-level LGFVs, we anticipate two potential effects of the reform: on one hand, these high-level LGFVs are likely unaffected by the reform because unlike local LGFVs, the high-level LGFVs are not directly connected to local governments; on the other hand, the reform increase provincial governments' control over local courts, potentially pressuring them to give biased rulings favoring LGFVs even more post-reform. The results show no baseline pattern for this placebo sample and there is no change in provincial or central-level LGFVs' win rate after the reform. This indicates two things: first, the baseline result is specific to the reduction of courts' protectionism favoring local governments, rather than other factors affecting all plaintiffs/defendants; second, despite granting more power to provincial-level governments, the reform did not result in greater protectionism towards these provincial-level LGFVs.

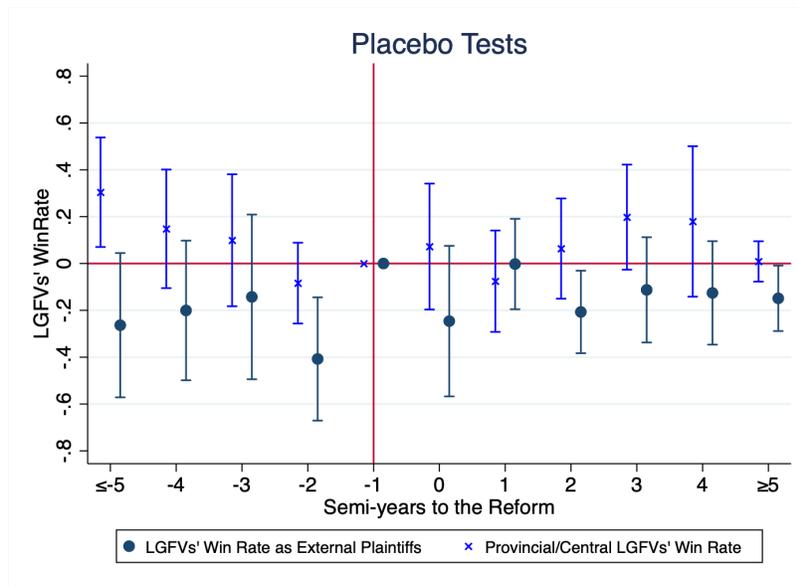


Figure 2: In this figure, we plot the event study coefficients (and the corresponding 95% confidence intervals) for: a) cases involving LGFVs controlled by provincial or central government; and b) cases where LGFVs are plaintiffs filing lawsuits against their counterparts in courts located in the counterparts' jurisdiction. All event studies are estimated following the approach suggested by [Sun and Abraham \(2021\)](#).

In cases where LGFVs are plaintiffs filing lawsuits against their counterparts in courts located in the counterparts' jurisdiction, we expect that the reform at the LGFV's location should not affect LGFVs' judicial outcomes in other courts, as long as the reforms timing in other courts is not closely correlated with that the LGFV's home court. The results confirm this hypothesis, showing no significant changes in LGFVs' win rates before and after the reform.

4.4 Intensive Margin vs. Extensive Margin

Our baseline findings regarding the win rates of local LGFV defendants could stem from two distinct sources. First, on the intensive margin, the judicial organizational reform might alter the incentives for judges, leading to a scenario where identical cases are adjudicated differently pre- and post-reform. For instance, prior to the reform, judges might have been under pressure from local governments to issue biased rulings in favor of local LGFVs. However, post-reform, this dynamic is likely to change, with the reform reducing the local government's leverage over local courts. Secondly, on the extensive margin, after noting the effects on the intensive margin, plaintiffs may recalibrate their litigation strategies (e.g., firms might become more inclined to file lawsuits against local LGFVs), thus shifting

the composition of LGFV-related lawsuits. In this section, we delve into these two aspects separately and find that our baseline findings are predominantly influenced by the intensive margin. Contrary to our initial observations, changes in case composition appear to counteract our baseline results, as the reform seems to have emboldened plaintiffs, who were previously less likely to win, to initiate litigation.

Firstly, to specifically examine the effect on the intensive margin, we concentrate on a subset of lawsuits initiated within six months prior to the implementation of the judicial organizational reform by local courts. We then compare the outcomes of these cases based on whether the rulings were issued before or after the reform. As indicated in Appendix Table A.1, this approach ensures a consistent composition of cases: 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 and focus exclusively on the intensive margin effect. As presented in Table 3, for the identical court and judge, decisions rendered immediately before the reform were significantly more favorable towards local LGFV defendants than those made shortly thereafter. The magnitude of this effect is considerably larger than that observed in our baseline Difference-in-Differences (DiD) analysis, reinforcing the conclusion that the changes in judge's incentives is the primary driver of the observed baseline outcomes.¹⁴

Secondly, we delve into the extensive-margin impact of the reform on the case composition. In Column (1) of Table 4, it's shown that lawsuits involving local LGFV defendants saw a 4.2% increase post-reform.¹⁵ This finding aligns with the notion that diminished judicial protection towards LGFVs has emboldened plaintiffs to pursue legal action against local LGFVs. Beyond the sheer volume of cases, there was also a notable shift in the characteristics of the plaintiffs involved in these lawsuits against LGFVs. As detailed in columns (2) to (7) of Table 4, post-reform, the plaintiffs were characterized by 6.5% less registered capital and 16.1% fewer employees, and were 6.9% younger in terms of firm age. These findings suggest that before the reform, many smaller plaintiff firms may have

¹⁴This analysis of changes in judge behavior aligns conceptually with [Ash et al. \(2022\)](#).

¹⁵On average, LGFVs are defendants in 76% of cases involving either LGFVs as defendants or plaintiffs.

Table 3: Intensive Margin - Conditional on Case Composition

	Local Defendant's Win Rate	
	(1)	(2)
Post Reform	-0.149*** (0.028)	-0.156*** (0.030)
Mean of Outcome	0.55	0.55
Court FE	Y	N
Semi-Year FE	Y	Y
Judge FE	N	Y
Provincial Time Trend	Y	Y
Observations	5,263	4,531
R-Squared	0.241	0.551

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 clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

been deterred from suing local LGFV firms due to the low perceived likelihood of success. However, the reform appears to have encouraged such firms to initiate lawsuits, indicating a significant shift in the litigation landscape.

The extensive margin effect results in an increased number of lawsuits featuring weaker plaintiffs against local 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 could potentially introduce a downward bias in our baseline estimates. This observation aligns with the finding that the intensive margin effect, which is not influenced by this shift in case composition, is significantly more pronounced than the baseline effect. The latter encompasses both intensive and extensive margin effects, highlighting how the change in case dynamics post-reform impacts our analysis.

4.5 Potential Side Effects

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. In this section, we delve into the limitations of the reform. In particular, we assess whether the reform (a) led to prolonged delays in issuing

Table 4: Extensive Margin - Changes in Case Compositions After Reform

	LGFV Defendants/ All LGFVs Cases	Plaintiff's Characteristics		
		Regis. Capital (M)	# of Employees	Firm Age
	(1)	(2)	(3)	(4)
Post Reform	0.042*** (0.012)	-5.614*** (1.873)	-92.025*** (23.146)	-0.649*** (0.141)
Mean of Outcome	0.76	85.92	570.94	9.25
Court FE	Y	Y	Y	Y
Seimi-year FE	Y	Y	Y	Y
Observations	16,773	45,141	38,215	45,034
R-Squared	0.608	0.049	0.067	0.066

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 LGFVs 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. Clustered standard errors at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

rulings (Section 4.5.1); or (b) diminished the rate of compliance with court orders (Section 4.5.2).

4.5.1 Delays in Court Decisions?

There is also the possibility that, despite observing a notable decrease in favoritism towards local governments, the reform may have led to slower judicial decisions if judges encountered a trade-off between the 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 3, there is no significant change in the duration of trials. This suggests that local judges managed to enhance the quality of judicial decisions without compromising on the quantity.

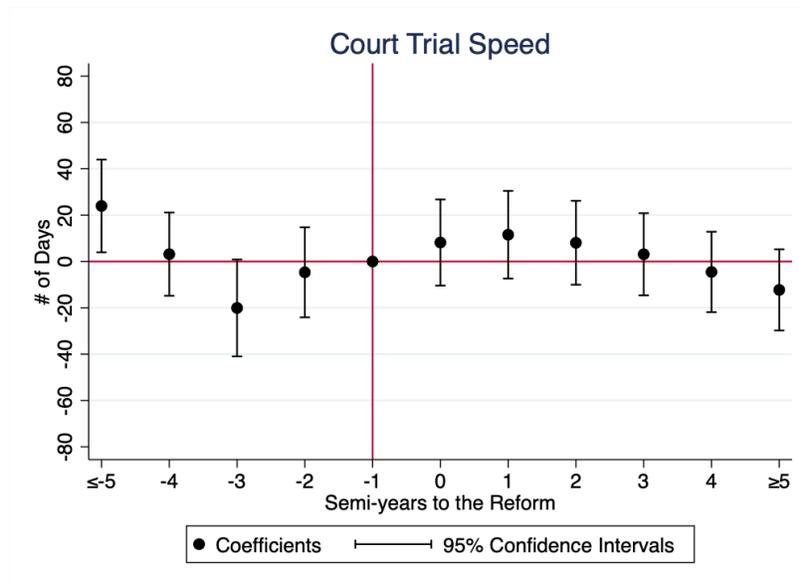


Figure 3: In this figure, we estimate the event study specification (Eq. 2) using the number of days taken for each case from start to finish as the outcome variable. We plot the event study coefficients (and the corresponding 95% confidence intervals), respectively. All event studies are estimated following the approach suggested by Sun and Abraham (2021).

4.5.2 Has the Reform Weakened Enforcement of Court Orders?

Given the greater political distance between provincial governments and local law enforcement agencies, there exists a concern that the enforcement of rulings against local LGFV defendants might diminish. Should this occur, the significant reduction in judicial decisions favoring local LGFVs might not translate into meaningful change. However, our empirical analysis does not support this concern. 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 this 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 Appendix Table A.2, the rate of non-compliance 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 effectively counter the hypothesis of “weakened enforcement.”

5 Impact on LGFVs' Purchase Contracts

In this section, we investigate whether the judicial reform that affects the dispute outcomes ex post would also affect the contracts ex ante. From the firms' perspective, the extent of judicial independence or court capture can be thought of as affecting the ex-post contingent payoff, with more judicial independence shifts the payoff more in favor of the LGFVs' counterparties. With higher expected payoff for the contractors and suppliers ex post, the LGFVs may ask for lower prices ex ante.

To isolate from any factors on the sellers' side that would affect the contracting prices, such as input prices and their financing costs, the ideal experiment to show the impact on prices is to conduct within-product analysis. That is, for the same product from the same seller, we can check whether LGFVs receive lower prices after the judicial reform as compared to LGFVs located in cities that did not experience the judicial reform. This experiment requires us to observe prices of transactions from the same seller to both the treated and control LGFVs in both the pre- and post-treatment periods. In the data, there are no such observations.

We address this challenge by making use of prices paid by the non-LGFVs to the same sellers. That is, we can use prices paid by the non-LGFVs as the benchmark and calculate the relative price paid by the LGFVs, which is the price paid by the LGFVs scaled by the average prices paid by all non-LGFVs to the sellers for the same product in the same year, weighted by the transaction value. We can then investigate whether the relative price decreases by more for LGFVs in cities with the judicial reform. This experiment only requires us to observe transaction prices paid by non-LGFVs to the same sellers for the same product in the same year. To the extent that these non-LGFVs are likely to be in the same city as the LGFVs and the judicial reform may also affect the prices they pay in the same way as for LGFVs, the relative price will be underestimate the price impact of the judicial reform.

Formally, denote $P_{i,j,k,t}$ as the price paid by firm i to seller j for product k in year t , and $Q_{i,j,k,t}$ as the corresponding quantity. Denote L as the set of LGFVs.

We then define the relative price as follows:

$$rp_{i,j,k,t}^1 = P_{i,j,k,t} / \sum_{i' \notin L, j'} P_{i',j',k,t} \frac{Q_{i',j',k,t}}{\sum_{i' \notin L, j'} Q_{i',j',k,t}} \quad (3)$$

$$rp_{i,j,k,t}^2 = P_{i,j,k,t} / \sum_{i' \notin L} P_{i',j,k,t} \frac{Q_{i',j,k,t}}{\sum_{i' \notin L} Q_{i',j,k,t}} \quad (4)$$

The first measure, $rp_{i,j,k,t}^1 = P_{i,j,k,t}$, does not impose the restriction that the product is purchased from the same seller, while the second measure, $rp_{i,j,k,t}^1 = P_{i,j,k,t}$, does impose such a restriction.

With the two measures, we then estimate the price impact of the judicial reform using the following DID model:¹⁶

$$rp_{i,j,k,t} = \alpha \cdot \text{Reform}_{c(i),t} + \theta_i + \lambda_t + \varepsilon_{i,j,k,t} \quad (5)$$

The results are shown in Panel A of Table 5. In Column (1), we use rp^1 as the dependent variable. After the judicial reform, the prices paid by the LGFVs' dropped significantly by about 11.6%, which is consistent with our hypothesis. To exploit heterogeneity with respect to the contract value, in Column (2) we focus on VAT invoices whose value is above 0.1m RMB. The judicial reform imposes similar negative effect on the prices of these large contracts, and the magnitude of the effect is marginally smaller. In Column (3) and (4), we repeat the estimation using rp^2 and find similar results. Overall, after the judicial reform, the LGFVs are able to get a discount of approximately 9.4%-11.6%.

The price effect represents adjustment on the intensive margin. On the extensive margin, the LGFVs may be able to source from different contractors and suppliers after the reform. As shown in Section 4, the judicial reform benefits external counterparties more than local ones. Therefore, we may expect that the external contractors and suppliers will increase their business with the LGFVs more than the local ones, although both of them shall be more willing to do business with the LGFVs after the reform.

To test this prediction, we aggregate the VAT invoices to the LGFV-year level to calculate the value share from external contractors and suppliers. We then run the

¹⁶The event study results are shown in the Appendix B.

following DID model:

$$y_{i,t} = \alpha \cdot \text{Reform}_{c(i),t} + \theta_i + \lambda_t + \varepsilon_{i,t} \quad (6)$$

Panel B of Table 5 shows the results. In Column (1), we look at the value share from sellers located in different cities. After the reform, LGFVs increase purchase from these sellers significantly by about 3.7%. In Column (2), we look at the value share from sellers located in different counties and find similar effect. We also consider some other seller characteristics by calculating the average seller characteristics at the LGFV-year level weighted by the VAT value. We do not find significant difference in the suppliers' size or age.

Before leaving this section, we shall mention that besides affecting the ex-ante contracting prices through the ex-post lawsuit outcomes, the judicial reform might also affect the prices indirectly through the LGFVs' borrowing capacity. As we shall show in the next section, alleviating court capture has inadvertently undermined local governments' borrowing capacity. The tightened financial constraint of the LGFVs may make them to ask for lower prices from their contractors and suppliers. But the financial constraint channel can not explain why the LGFVs buy more from external contractors and suppliers after the reform.

Table 5: Impact of Judicial Reform on Ex-ante Contracts

Panel A: Price Effect

Price Group	All Sellers		Same Sellers	
	All	>0.1m	All	>0.1m
Dep Var: rp (relative price)	(1)	(2)	(3)	(4)
Reform	-0.116*** (-2.612)	-0.0797** (-2.536)	-0.0943*** (-2.655)	-0.0618*** (-2.723)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5787	1607	5660	1584
R-squared	0.7763	0.8093	0.7936	0.8206

Panel B: Change of Contractors and Suppliers

Dep Var:	difcity	difcounty	suppliersize	supplierage
	(1)	(2)	(3)	(4)
Reform	0.0377* (1.828)	0.0260* (1.877)	0.0549 (0.547)	0.0106 (0.425)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	4,017	4,017	4,008	4,016
R-squared	0.7882	0.7152	0.7205	0.6462

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

6 LGFVs' Debt Capacity and Spending

In this section, we study whether and to what extent alleviating court capture affects the financing and operating activities of the LGFVs. Since over 93.67% of the lawsuits involving LGFVs are with their business partners (the rest with banks and financing companies), the creditors of LGFVs can be negatively affected by the judicial reform. This is because as alleviating court capture increases the LGFVs' lawsuit costs, they will have less resources available to pay other creditors. This is particularly relevant for MCB creditors as the average maturity of MCB is about four years. However, alleviating court capture has also lowered LGFVs' contracting prices, which should decrease their credit demand ex ante. Therefore, it is unclear whether and how the LGFVs are affected. We will first study the impact on their issuance of MCBs, then use firm-level evidence to evaluate the overall impact, and finally discuss potential mechanisms.

6.1 Issuance and Yield of MCBs

We first aggregate the individual local MCBs to get the city-level MCB outstanding balance at the end of each year since 2010. We then calculate the net MCB issuance, $\text{NetIssue}_{c,t}$, as the change of MCB balance from year $t - 1$ to year t scaled by the city's fiscal revenue in 2014. We choose the year of 2014 as it is right before the earliest judicial reform year. We also calculate the city-year level average MCB yield, $\text{AveYield}_{c,t}$, using all MCBs issued by all government units in city c and year t . If the city governments did not issue any new MCBs in year t , we will first fill $\text{AveYield}_{c,t}$ with previous non-missing values and then future non-missing values. We end up with a balanced city panel for 2010-2023.

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

$$Y_{c,t} = \sum_{\tau \neq 0} \beta_{\tau} \cdot \mathbf{1}_{t=\tau+t_c} \text{Reform}_{c,t} + \theta_c + \lambda_t + \varepsilon_{c,t} \quad (7)$$

where t_c is the reform year in city c , $\text{Reform}_{c,t}$ 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 after the reform has occurred. We follow [Sun and Abraham \(2021\)](#) to account for heterogeneous

treatment effects.

Figure 4 plots the 95% confidence interval of the β_τ estimates for $\text{NetIssue}_{c,t}$ and $\text{AveYield}_{c,t}$, respectively. In Panel A, there was no significant difference in the change of net MCB issuance between the treated and control cities before the judicial reform. The treated and control cities began to diverge three years after the reform. In the third year, net MCB issuance by the treated cities was significantly lower by more than 10%, and the magnitude of the effect increased to about 15% in the next three years. There is no evidence of reversing back at least six years after the reform.

Panel B shows the results for the average MCB yield. Similarly, the treated and control cities were on a parallel trend before the reform. One year after the reform, the average MCB yield for the treated cities was significantly higher by about 0.15%. The magnitude of the effect further increased to about 0.25% and remained stable in the next four years.

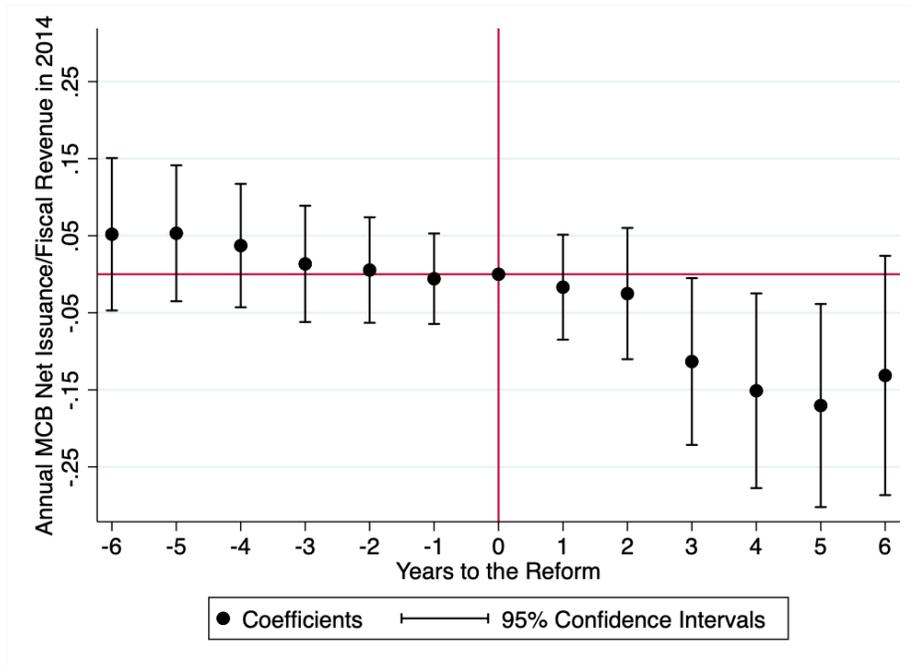
The decline of net issuance and increase of yield suggests that the investors' demand for the MCBs has decreased after the reform. Concerned about the less supportive judicial environment for the LGFVs, investors have decreased lending to these LGFVs and increased required yield for their bonds.

6.2 LGFVs' Balance Sheet and Spending

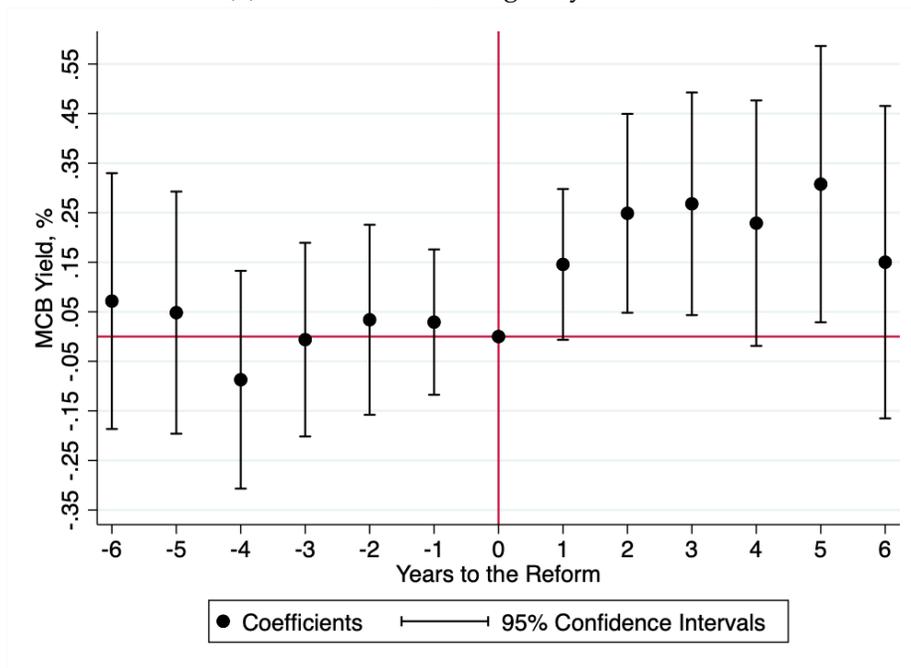
As shown in Table 1, bond financing only accounts for about 8.0% of the LGFVs' asset on average. We do not have detailed data on the LGFVs' loans and other sources of credit. The other creditors may be affected in the same way as the bond creditors. If not, the LGFVs may substitute bond financing with other credit sources, which will lead to smaller effect on the firm level. In this section, we investigate the overall effect on the LGFV firm level.¹⁷

We start by looking at the firms' asset size using the following event-study

¹⁷We do not aggregate the firm-level information 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 total number of LGFVs as published by the China Banking Regulatory Committee is about four times the number of our sample LGFVs.



(a) Net Issuance/Budgetary Revenue



(b) Average Yield, %

Figure 4: 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.

specification:

$$\log(\text{Asset}_{i,c,t}) = \sum_{\tau \neq 0} \beta_{\tau} \cdot \mathbf{1}_{t=\tau+t_c} \text{Reform}_{c,t} + \theta_i + \lambda_t + \varepsilon_{i,t} \quad (8)$$

Figure 5 plots the 95% confidence interval of the β_{τ} estimates. There is no significant difference in asset growth between treated and control LGFVs before the judicial reform. Right after the judicial reform, the treated LGFVs started to experience significantly slower asset growth compared to those untreated LGFVs. The gap increased from about 5% in the first year to about 10% since the third year after the reform.

In Table 6 Column (1), we confirm that on average, the judicial reform reduced the LGFVs' asset size by about 9.3%. Column (2) shows that the average borrowing rates also increased by about 0.29% after the judicial reform. The decrease of asset growth and the increase of borrowing rates suggest that on the firm level, LGFVs have suffered from higher borrowing costs and slowed down their growth.

Moreover, the magnitude of the impact on asset growth and borrowing rates suggests that other creditors are affected in the same way as the bond investors. This is because bond financing only accounts for about 8.0% of the LGFVs' asset and the judicial reform only reduces the net bond issuance by no more than 15% and bond yield by no more than 0.25% as shown in Figure 4. If other credit sources are not adversely affected, we will not observe a drop in asset growth as large as 9.3% and an increase in borrowing rates by 0.29%. Note that our sample is restricted to the bond-issuing LGFVs. We believe that the financing of LGFVs that never issued any bonds, such as through bank loans, must also be negatively impacted as our sample LGFVs.

In Table 6 we further look at different components of the LGFVs' liabilities using Equation (6). In Column (3), we find that even scaled by the firms' asset size, there is still a significant and negative impact on the use of bond financing. This suggests that bond financing is affected more than other credit sources as a whole, although the magnitude is only about 1.2%.

In Column (5) of Table 6, we find that LGFVs use more trade credit after the reform. There are two potential explanations. First, unlike other creditors, contractors and suppliers may be more willing to offer trade credit because if

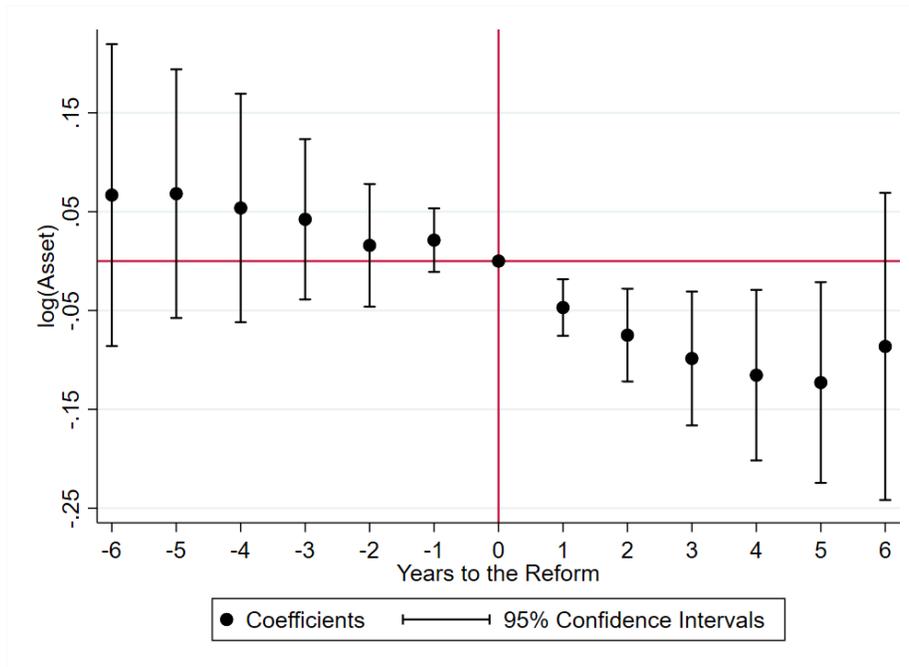


Figure 5: LGFV asset size after the judicial reform

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.

the LGFVs attempt to default, the contractors and suppliers are more likely to recover the loss through lawsuits after the reform. This is consistent with our key hypothesis that the judicial reform benefits the contractors and suppliers at the cost other creditors. Second, more use of trade credit may also reflect the substitution from bond and loan financing to trade credit.

6.3 Mechanisms

In this last section, we provide more evidence on the mechanism, discuss the generosity of our findings, and refute some alternative explanations.

6.3.1 LGFVs' default on debt obligations

We conjecture that the judicial organizational reform undermined LGFVs' borrowing capacity by increasing their default on debt obligations. While LGFVs have not

Table 6: Effect on LGFVs' Alternative Funding Sources

Dep Var:	size	borrowrate	Bond/Asset	leverage	AccPayable/COGS
	(1)	(2)	(3)	(4)	(5)
Reform	-0.0941** (-2.284)	0.279** (2.130)	-0.0116*** (-2.580)	0.0105 (1.237)	0.0241** (2.413)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	25,848	19,677	25,765	25,765	25,514
R-squared	0.8659	0.6294	0.5924	0.7198	0.5324

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

defaulted on MCBs, potentially for reputation concerns, they have done so on less standard, more opaque debt obligations such as trust products.¹⁸ Compared to municipal corporate bonds issued in the interbank market or traded on exchanges, these non-standardized debts or *feibiao* (Chinese for “non-standardized”) debts are subject to more lax regulation and less information disclosure requirements. Hence, defaulting on *feibiao* debts is less costly to LGFVs. The selective defaults on non-standardized *feibiao* debts by LGFVs have been widely reported in the news and indicated in public announcements.¹⁹

To confirm that LGFVs default more after the judicial reform, in this section we study the impact on LGFVs' default on these non-standardized debts, referred

¹⁸A strand of banking and corporate finance literature has investigated firms' choice between privately placed and publicly offered debts and firms' reluctance in open market defaults. For instance, Diamond (1991) wrote that “reputation effects eliminate the need for monitoring when the value of future profits lost because of the information revealed by defaulting on debt is large. Borrowers with higher credit ratings have a lower cost of capital, and such a rating needs to be maintained to retain this source of higher present value of future profits.”

¹⁹For instance, Zunyi Daoqiao, the largest LGFV in Zunyi City in the southwestern province of Guizhou, reorganized its bank loans by expanding the maturity to 20 years and reducing the 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. See Zunyi Daoqiao's official announcement on ChinaBond, December 30, 2022, https://www.chinabond.com.cn/xxpl/ywzc_fxyfxdh/fxyfxdh_zqzl/zqzl_zyz/qyz_qtgggtz/202307/t20230716_853444315.html. Another example is the bankruptcy cases filed by a trust company against two LGFVs in Guizhou in August 2023, which mentioned that these LGFVs delivered principal and interest payments to bond holders but not to trust products. See, for instance, <https://finance.sina.com.cn/jjxw/2023-08-03/doc-imzexiiv9831839.shtml>.

to as *feibiao* defaults. As there are only a few default events of LGFVs in the most recent years, the DID strategy that we have used above does not apply here. Instead, we conduct the following cross-sectional regression:

$$\text{Default}_c = \alpha + \beta \cdot \text{Has_Reform}_c + \gamma \cdot X_c + \epsilon_c, \quad (9)$$

where Has_Reform equals one if a city has implemented the judicial reform by the end of our sample period and zero otherwise. Default_c is the cumulative number of LGFVs with *feibiao* defaults in city c divided by the total number of LGFVs in the city. Our coefficient of interest is β , which captures the differences in LGFV default rates between cities with judicial reforms and those without. Our city-level control variables include GDP per capita, population, and average LGFV leverage ($\text{leverage}_c = \frac{\sum_i \text{liability}_{ic}}{\sum_i \text{asset}_{ic}}$), all taking the value in 2014.

As shown in Table 7, we find higher *feibiao* defaults among LGFVs in cities with judicial reforms ($\text{Has_Reform} = 1$). The coefficients of Has_Reform are approximately 2% in Columns (1)-(3), meaning that on average, the *feibiao* default rates of LGFVs in cities with judicial reforms is 2 percentage points higher than those in cities without such reforms. The coefficient is economically important as compared to the standard deviation of 8.8% reported in Table 1. 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 our sample cities to the 41 cities with at least one LGFV default. The coefficients increase to 9% without controls and 7.25% with city-level controls.

Overall, our results are statistically significant and economically important, showing that the judicial reform has affected the LGFVs' default rates on non-standardized debt obligations, which imply greater risks of loans and bonds that may material soon.

6.3.2 Contracting prices versus debt default

As alleviating court capture not only increases the LGFVs' lawsuit costs but also decreases their contracting prices (as shown in Section 7, why is the decrease in contracting prices not enough to counteract the ex-post increase in lawsuit costs? We propose three non-exclusive explanations below. We formalize the last two

Table 7: Effect on LGFVs' Defaults

	LGFV Default Rate, %					
	All cities			Cities with defaults		
	(1)	(2)	(3)	(4)	(5)	(6)
Has_Reform	2.042** (0.848)	1.961** (0.843)	2.035** (0.866)	9.000** (4.174)	7.259** (3.421)	7.242* (3.605)
GDP per capita, ln		-0.400 (0.856)	-0.352 (1.088)		-5.224** (2.357)	-5.913** (2.820)
Population, ln		-0.198 (0.871)	-0.164 (0.887)		-11.163*** (1.263)	-11.863*** (1.816)
Average LGFV leverage			-2.059 (5.610)			11.357 (21.843)
Observations	310	302	295	41	39	39
R-squared	0.011	0.012	0.013	0.062	0.438	0.444

Note: This table shows how the judicial reform has affected the LGFVs' default rates on non-standard debts, or *feibiao* default rates, defined as the number of defaulted LGFVs divided by all LGFVs in a city. Has_Reform equals one if a city has implemented the judicial reform by the end of our sample period and equals zero for cities that never implement the reform. We use the full sample of 310 cities for our cross-sectional analysis in Columns (1)-(3) and the subsample of 41 cities with at least one LGFV default in Columns (4)-(6). We include city-level GDP per capita and population (both in natural logarithm) as controls in Columns (2) and (4) and add city-level average LGFV leverage ($leverage_c = \frac{\sum_i liability_{ic}}{\sum_i asset_{ic}}$) in Columns (3) and (6). Standard errors are clustered by cities. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

arguments in the appendix.

The first is the existence of contracts signed before the judicial reform and yet not completed at the time of the reform. As the contracting prices did not adjust, at least not fully, in favor of LGFVs, the LGFVs only suffer from the ex post lawsuits but not benefit from the lower contracting prices. If this is the only explanation, we shall expect the adverse effect on LGFVs to diminish and be transitory because the previous-signed contracts shall be complete soon. However, as we show in Figure 4 and 5, the adverse effect actually increases over time and did not diminish at least six years after the reform. Therefore, the overhang of previously signed contracts is unlikely to fully explain our results. The adverse impact on LGFV

financing is more likely to be persistent.

Second, the difference in discount rates and risk aversion between entrepreneurs and bond investors may explain the results. On one hand, the entrepreneurs have a higher discount rate than the bond investors because the marginal borrowing rates of entrepreneurs must be much higher than the bond rates. As a result, the contractors and suppliers will discount the future lawsuit gains by more, reduce the ex-ante contracting prices and hence the LGFVs' lending by less, and the small decrease in ex-ante borrowing is not enough to compensate for the higher risks ex post from the perspective of bond investors. On the other hand, bond investors select to invest in bonds because they are more risk averse. Therefore, they may disvalue a lot the high risks of LGFVs after the judicial reform, despite the decreased borrowing of LGFVs. Both of the two factors can explain why the shift of payoff towards the contractors and suppliers in case of lawsuits induced by alleviating court capture can undermine the LGFVs' borrowing capacity.

Last but not least, alleviating court capture may worsen the moral hazard problems on the contractors. Project contracting involves a lot of details on the contractors' side, such as the construction structure, the materials to be used, the timeline of construction, and whether certain parts can be outsourced; while on the contractee's side, it only needs to specify the reschedule of payment, etc. The greater action space on the contractors' side leads to more room for potential moral hazard problems. It is probably because of this reason that dispute over project contracts is one of the most common civil lawsuits.

To identify dispute as involving contractor moral hazard problems, we conduct searching based on key words such as quality, illegal outsourcing, delay, etc. During our sample period, for approximately 40 percent of all the lawsuits against project contractors, the contractors have exhibited behaviors that represent contractor moral hazard problems. The court does not always attribute these cases to the contractors' fault as the contractors can often find some excuses or the contracts fail to specify responsibilities for some scenarios. Alleviating court capture in favor of these contractors may then encourage more moral hazard problems, lower the net return of the LGFVs' project and thereby reduce their borrowing capacity.

6.3.3 General equilibrium effect

Besides affecting the investors' demand through lawsuits involving LGFVs, one may be concerned that the judicial organization reform shall lead to multiple effects on the local economy, some of which may also explain the effect on the financing of LGFVs. In particular, [Liu et al. \(2022\)](#) shows that alleviating court capture can increase external investment to the cities. First, more private investment should increase demand for local infrastructure and public facilities, and thereby increase demand for the services provided by the LGFVs. This should predict more issuance of MCBs, larger growth in asset size, and lower financing costs (if any effect), which is opposite to what we find. Second, more firm entry may increase demand for credit and crowding out credit supply to LGFVs. This may adversely affect the LGFVs' bank loan, but not the LGFVs' bond issuance because the bond market is highly integrated. Most of the bond investors are national financial institutions. In general, alleviating court capture typically leads to economic advantages, which is unlikely to undermine the LGFVs' borrowing capacity.

6.4 Real Effect

Finally, we provide some analysis on the real impact of the decrease of local government borrowing capacity induced by the judicial reform. The tightening of municipal financing capacity can have multiple effects on the government spending and local policies. In particular, it would adversely affect the functioning of the LGFVs.

We first investigate how the judicial reform affects the LGFVs' spending. We aggregate all the VAT invoices to the LGFV-year level to compute their annual total spending. We then classify the spending based on whether it is related to employee consumption or asset investment and compute their annual consumption and investment spending. Table 8 shows that following the reform, the LGFVs' annual spending dropped dramatically by about 68.9%. The effect is more pronounced for investment-related spending than the consumption-related spending. This result implies that by reducing the LGFVs' borrowing capacity, the judicial reform have either undermined the local governments' capacity to offer public goods or curtailed inefficient excessive government investment, depending on the efficiency

of the government marginal investment during our sample period.

Table 8: Effect on LGFVs' Spending

	All	Invest.	Consum.
Dep Var: log(Spend)	(1)	(2)	(3)
Reform	-0.689**	-0.757*	-0.495*
	(-2.251)	(-1.884)	(-1.833)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	8686	8686	8686
R-squared	0.6262	0.5967	0.3998

Note: This table shows how the judicial reform has affected the LGFVs' firm-level spending. In Column (2) and (3), we divide spending into investment and consumption spending based on the nature of the expense. Standard errors are clustered by cities. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

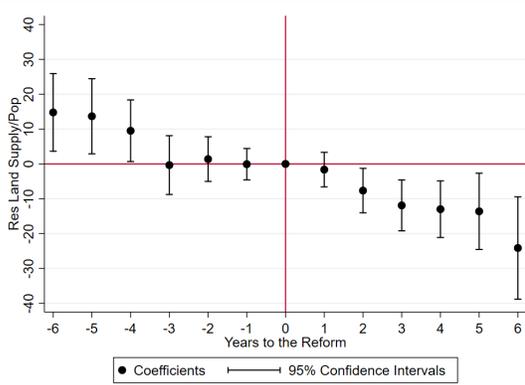
One of the most important roles played by the LGFVs is the "land bank." The LGFVs typically invest to produce land inventories for the local governments to sell. That is, they will take either unoccupied or occupied land parcels (after compensation to incumbents), level the ground, and connect to the land to main roads and services (such as drainage, water supply pipelines, electricity, gas, internet, etc) to make it ready to sell to the market. The production of land inventories is costly and LGFVs have played an important role in the transformation of urban landscape via new residential land supply.

To study whether decreased borrowing capacity has affected the LGFVs' role in land supply, we use the land auction data from Ministry of Natural Resources and aggregate to the city level to calculate the annual land supply and average land price (weighted by the size of land parcels) for each year. We then estimate Equation (7) using the annual land supply per capita and land price scaled by GDP per capita as the dependent variable.

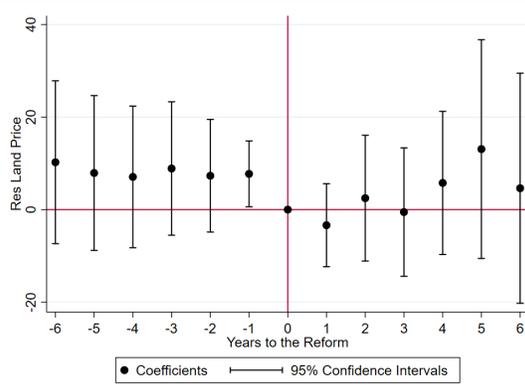
Figure 6 shows the event study results. The tightened borrowing capacity induced by the judicial reform has gradually decreased the city's new residential land supply over time. Five years after the reform, annual residential land supply will decrease by about 13.6 square meters per 100 people. The magnitude of the effect is economically important as the mean (standard deviation) of annual land supply is 59.5 (46.0) square meters per 100 people. In contrast, there does not

seem to be much impact on the commercial land supply. For industrial land, the supply seems to have increased if any effect. LGFVs typically do not engage in industrial land supply. The decrease of residential land supply may have led to more industrial land supply.

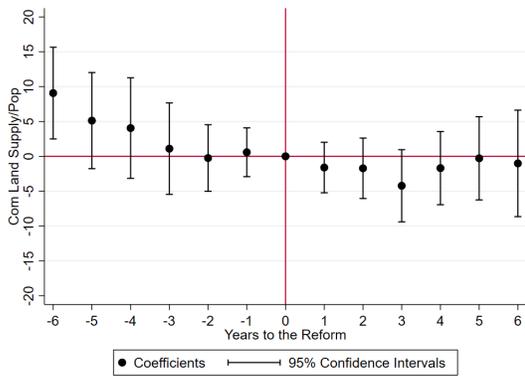
We shall emphasize that the reduction in residential land supply represents not only a shift of the quantity of land supply, but also a shift of the quality of land parcels as proxied by the supporting facilities. That is, with reduced borrowing capacity induced by the judicial reform, LGFVs will not be able to upgrade the facilities for the same amount of land, and by cutting the amount of land supply, the local governments will be able to maintain the same level of land prices. This is supported by Panel (b) of Figure 6, which shows no significant price impacts on the residential land. As supporting evidence for the identification strategy, there are also no significant impacts on other types of land either.



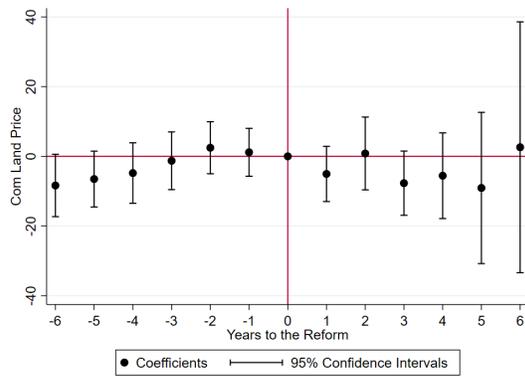
(a) Residential Quantity



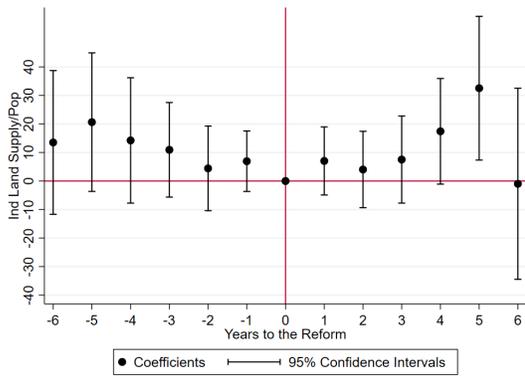
(b) Residential Price



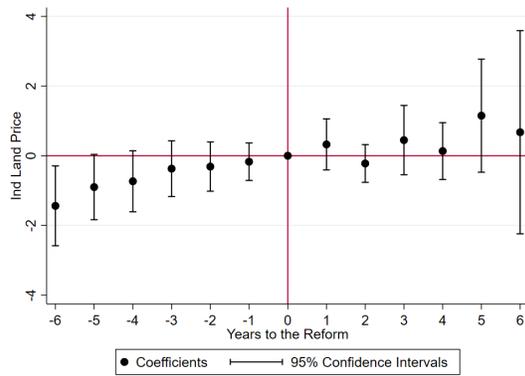
(c) Commercial Quantity



(d) Commercial Price



(e) Industrial Quantity



(f) Industrial Price

Figure 6: Land Supply after the judicial reform

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

7 Conclusion

There is a long literature that has documented the economic significance of the legal system. Among various issues, an independent judiciary that is free from political or popular influences is typically thought to bring economic advantages. Even for the governments per se, losing influences over the judicial system is thought to be good for the financing of governments as it represents a credible signal that the governments are less likely to strategically default on their debt. This paper refutes this view by introducing heterogeneity of the governments' counterparties. In particular, we show that the governments' contractors and suppliers benefit more from an independent judiciary than disputes involving contractors and suppliers are much more frequent in practice. Alleviating court capture then inadvertently disadvantages other creditors such as the bond investors, leading to decreased government borrowing capacity.

The context that we analyze in this paper involves not default of municipal government bond, or legal disputes between governments and bond investors. One may thus hold the view that the direct benefits of an independent judiciary for the bond investors is shut down and the adverse effect that we document in this paper dominates. However, this view is not necessarily correct. It is an endogenous decision for the governments and bond creditors not to go to the court. The extent of judicial independence or court capture can affect their payoff in the off-equilibrium path where they do go to the courts, and thereby affect their negotiation outcomes in the equilibrium path where they decide not to go to courts. Thus, alleviating court capture can lower the government bargaining power and lead to higher borrowing capacity ex ante, even if we do not observe any lawsuits between them in practice.

Moreover, the extent of discretion that the courts, especially lower-level courts, possess over debt disputes is very limited. Unlike the project contracts with contractors, the debt contracts are very standard, leaving less room to the courts to favor one versus the other. In the US where researchers have documented how the courts helped the municipalities to waive debt, only the state Supreme Court can change of interpretation of legislation in a significant way to help local governments nullify outstanding debt. Therefore, we believe our results should also hold in other context, especially when the court capture is about the lower-level courts.

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Online Appendix

A Data

Figure A.1 presents the time trend for the number of court cases involving LGFVs as local plaintiffs and local defendants respectively.

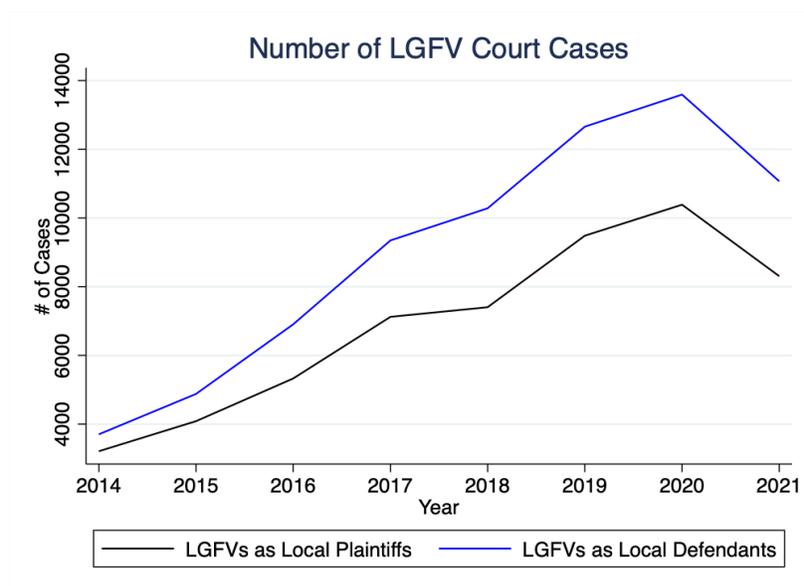


Figure A.1: In this figure, we plot the time trend of the number of court cases involving local LGFV plaintiffs and local LGFV defendants, respectively.

B Supplementary empirical results

Figure A.2 presents alternative event-study estimators for our baseline impacts on local LGFVs' win rate. We plots the baseline event study coefficients (as well as 95% confidence intervals), following the approach suggested by [Borusyak et al. \(2021\)](#).

Table A.1 ensure the consistent composition of cases: the characteristics of plaintiffs and defendants in cases adjudicated before and after the reform are well-balanced.

Table A.2 shows the effect of judicial reform on LGFVs' compliance rate on court orders.

Table A.3 shows the correlation between the city's reform status by 2021 and the city's/LGFVs' characteristics in 2013, one year before the earliest reform year.

Table A.4 shows the event study results of how the judicial reform affects the LGFVs' contracting prices and contractors' location, size and age.

Table A.5 shows the impact of the judicial reform on provincial and national financing vehicles.

Table A.6 shows the event study results of how the judicial reform affects the LGFVs' spending.

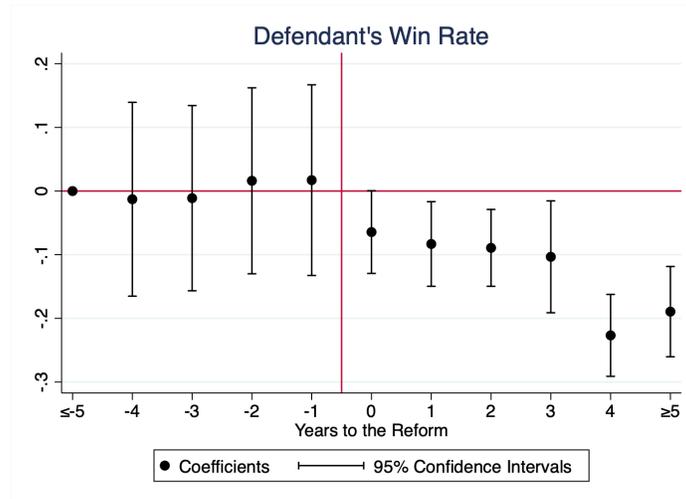


Figure A.2: In this figure, we plot the event study coefficients (and the corresponding 95% confidence intervals) for the baseline results in Columns (1). All event studies are estimated following the approach suggested by Borusyak et al. (2021).

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	10.251 (13.136)	9.136 (5.041)	8.210 (67.126)	-106.213 (112.455)	0.101 (0.612)	7.617 (8.216)
Mean of Outcome	84.58	84.93	495.45	648.41	10.70	10.55
Court FE	Y	Y	Y	Y	Y	Y
Seimi-year FE	Y	Y	Y	Y	Y	Y
Observations	4,125	3,146	3,420	2,751	3,937	3,091
R-Squared	0.158	0.136	0.346	0.181	0.156	0.145

Table A.2: Judicial Reform and Ruling Enforcement

	Non-compliance Rate	Complete Non-compliance	Partial Non-compliance
	(1)	(2)	(3)
Post Reform	0.003 (0.002)	0.002 (0.002)	0.001 (0.001)
Mean of Outcome	0.013	0.015	0.005
Court FE	Y	Y	Y
Seimi-year FE	Y	Y	Y
Observations	16,836	16,831	16,241
R-Squared	0.191	0.161	0.105

Notes: Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.3: Judicial Reform and City/LGFV Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var	NetIssue/FisRev	MCB Yield	size	Bond/Asset	leverage	AccPayable/COGS
Reform	-0.0348 (-1.030)	-0.0833 (-0.891)	0.0307 (0.353)	0.000945 (0.289)	-0.0584*** (-3.234)	-0.0358** (-2.330)
Constant	0.177*** (5.806)	7.059*** (97.01)	4.379*** (67.72)	0.0385*** (14.26)	0.446*** (31.55)	0.179*** (12.77)
Observations	266	264	1,660	1,640	1,640	1,583
R-squared	0.005	0.003	0.000	0.000	0.026	0.010

	(7)	(8)	(9)
Dep Var	ResLand/Pop	ComLand/Pop	IndLand/Pop
Reform	1.417 (0.198)	-6.419 (-1.130)	-28.30 (-1.486)
Constant	77.73*** (13.37)	57.90*** (12.20)	185.3*** (11.26)
Observations	318	318	318
R-squared	0.000	0.004	0.008

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

Table A.4: Impact of Judicial Reform on Ex-ante Contracts

Panel A: Price Effect

Price Group Contract Value Dep Var: rp (relative price)	All Sellers		Same Sellers	
	All (1)	>0.1m (2)	All (3)	>0.1m (4)
g1	-0.760** (-2.350)	-1.212** (-2.361)	-0.719** (-2.423)	-1.211** (-2.461)
g2	0.567 (1.613)	0.558 (1.616)	0.519 (1.327)	0.492 (1.249)
g3	-0.338*** (-6.151)	-0.213*** (-4.898)	-0.175*** (-3.111)	-0.0352 (-0.497)
g4	-1.532*** (-5.432)	-1.664*** (-4.275)	-1.441*** (-4.146)	-1.893*** (-4.997)
g5	-0.315*** (-4.886)		-0.270*** (-4.297)	
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5787	1607	5660	1584
R-squared	0.7893	0.8224	0.8045	0.8326

Panel B: Change of Contractors and Suppliers

Dep Var:	difcity (1)	difcounty (2)	Suppliersize (3)	supplierage (4)
g_1	-0.0366 (-0.843)	-0.0245 (-0.826)	0.151 (0.613)	-0.0281 (-0.534)
g1	0.0297 (0.964)	0.00204 (0.113)	0.116 (0.845)	-0.0261 (-0.761)
g2	0.0626* (1.931)	0.0405* (1.842)	0.241 (1.631)	0.0192 (0.460)
g3	0.0457 (1.455)	0.0277 (1.393)	0.134 (0.903)	0.0318 (0.944)
g4	0.0865*** (3.099)	0.0602*** (3.260)	0.175 (1.127)	0.0409 (1.082)
g5	0.0392 (1.453)	0.0217 (1.255)	-0.105 (-0.646)	0.0372 (0.841)
g6	0.00333 (0.203)	0.00576 (0.386)	-0.0744 (-0.593)	0.00915 (0.315)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	4,017	4,017	4,008	4,016
R-squared	0.7907	0.7191	0.7243	0.6491

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

Table A.5: Effect on the Balance Sheet of Non-local Financing Vehicles

Dep Var:	size	borrowrate	Bond/Asset	leverage	AccPayable/COGS
	(1)	(2)	(3)	(4)	(5)
Reform	-0.218*	0.521**	-0.0334**	-0.0411**	0.0230*
	(-1.815)	(2.019)	(-2.471)	(-2.110)	(1.876)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	1,597	1,271	1,595	1,595	1,584
R-squared	0.904	0.6556	0.5928	0.7051	0.5924

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

Table A.6: Effect on LGFVs' Spending

	All	Invest.	Consum.
Dep Var: log(Spend)	(1)	(2)	(3)
g ₋₁	0.425	0.298	0.604
	-1.001	-0.552	-1.215
g ₁	-0.468*	-0.285	-0.395
	(-1.889)	(-0.879)	(-1.474)
g ₂	-0.677**	-0.872**	-0.365
	(-2.156)	(-2.214)	(-1.151)
g ₃	-0.648*	-0.669	-0.211
	(-1.906)	(-1.417)	(-0.628)
g ₄	-0.898**	-1.134**	-0.710*
	(-2.312)	(-2.168)	(-1.822)
g ₅	-0.874**	-0.584	-0.820*
	(-2.109)	(-0.872)	(-1.757)
g ₆	-0.838**	-1.000**	-0.299
	(-2.432)	(-2.097)	(-0.831)
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	8686	8686	8686
R-squared	0.6285	0.5983	0.5504

Note: This table shows the event study analysis on how the judicial reform has affected the LGFVs' firm-level spending. In Column (2) and (3), we divide spending into investment and consumption spending based on the nature of the expense. Standard errors are clustered by cities. Robust t-statistics in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.