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Keywords: China, recentralization, judicial independence, rule of law

Making Local Courts Work: The Judicial Recentralization Reform and Local Protectionism in China*

Zhenhuan Lei[†] Yishuang Li[‡]

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[†]Assistant Professor, Department of Political Science and La Follette School of Public Affairs, University of Wisconsin-Madison, and Visiting Scholar, China Center for Economic Research, National School of Development, Peking University. Email: zhenhuan.lei@wisc.edu. Personal website: <http://www.reedlei.com>.

[‡]Ph.D. Candidate, Department of Politics, New York University. Email: yishuang.li@nyu.edu. Personal website: <https://www.yishuang-li.com>. Corresponding author.

Abstract

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1 Introduction

The rule of law plays a key role in effective governance by laying the institutional foundation for property rights protection, investment attraction, and economic development (La Porta et al., 2004; Jensen, 2008; Staats and Biglaiser, 2012; Haggard et al., 2008). A key to the rule of law, among other factors, is the independence of the judicial branch from external interference. On this point, earlier research reports compelling evidence that judicial independence is more likely if different parties control the government and the legislature (Chávez, 2007; Franck, 2009; Leiras et al., 2015). This is even true in authoritarian settings where divisions exist in the ruling coalition (Barros, 2002). In contrast, unified governments (Ramseyer and Rasmusen, 2001; Larkins, 1998; Iaryczower et al., 2002) or authoritarian regimes (Domingo, 2000; Moustafa, 2014; Solomon, 2007; Shen-Bayh, 2018) that lack political divisions are more likely to undermine the autonomy of courts.

However, this line of research is unlikely to explain the improvement of the rule of law under a unified government. Take China as an example. Figure 1 plots the percentile ranking of the rule of law index from the Worldwide Governance Indicators (WGI) for China (grey squares) and the average for autocracies (blue triangles) and democracies (red dots).¹ We define autocracies as those countries that receive a negative POLITY2 score and democracies as those that receive a non-negative POLITY2 score from the Polity Project.²

An interesting pattern arises from this figure. Although autocracies' WGI Rule of Law Index is consistently ranked much lower than democracies, China's ranking has improved since 2007 and most impressively under the Xi Jinping administration (2013–2020) from slightly below the 40th percentile to above the median where the average of democracies stands. This pattern is consistent with recent research that argues that the Chinese government has tried to make law more accessible and useful through various reforms (Peerenboom, 2002; Whiting, 2017; Zhang and Ginsburg, 2019; Ip and Kwok, 2017).

¹Appendix Figure B1 shows that the WGI rule of law *scores* also present a similar pattern as Figure 1.

²We follow Acemoglu et al. (2019) and use zero as the cutoff for regime type.

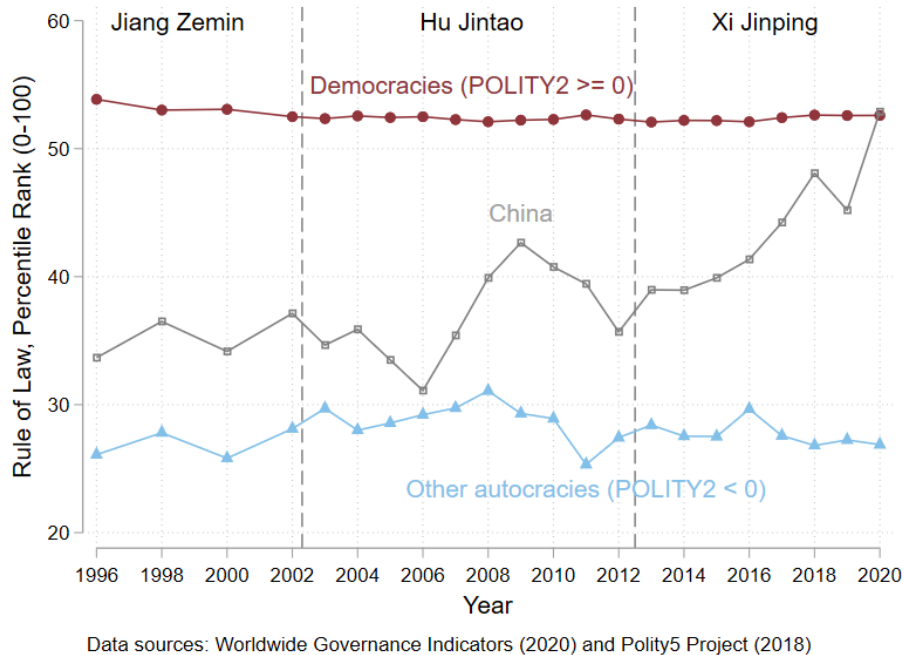


Figure 1: The WGI Rule of Law Ranking of China and Other Countries (1996–2020)

However, China’s investment and improvement in legality run against the prediction that unified governments often weaken the independence of judiciaries and harm the rule of law. Indeed, the Chinese government often instructs courts to try politically sensitive cases according to its preferences rather than law (Li, 2016; Hou and Truex, 2020; Ding and Javed, 2021). Furthermore, Xi Jinping is widely viewed as a more powerful leader who has stronger control over other branches of the Chinese government than his two predecessors (Choi et al., 2021). Then how did the Chinese government promote legality under a more unified single-party leadership?

We argue that this is at least partly due to China’s efforts to strengthen the independence of local judiciaries from local elites’ interference. Although the literature predominantly focuses on the political interference of national government, *local* economic and political elites also exert influence in judicial decisions (Hendley, 1996; Glaeser and Shleifer, 2002; Franco-Vivanco, 2021) and create judicial biases (Ang and Jia, 2014; Lu et al., 2015; Xu, 2020; Chen and Xu, 2021). This local influence is possible because local judiciaries are often

under the control of local governments. In the setting of China, Basic and Intermediate People’s Courts – the first-instance courts for most cases – were under the control of county and prefectural governments. As a result, local politicians in counties and prefectures often interfered with case decisions in favor of local litigants (Peerenboom, 2002; Li, 2012). Even the President of the Supreme People’s Court recognized that “law was taken by some local officials as a tool to protect parochial interests” (Zheng, 1994, p.472).

To contain rampant judicial local favoritism, Xi Jinping announced a reform that would gradually recentralize the control of personnel management (including the appointment, promotion, and removal of all judges), fiscal budget, and property management of Basic and Intermediate People’s Courts from counties and prefectures into the hands of provincial party-state leadership. The goal of this reform, according to the Chinese Communist Party (CCP), was to “ensure that courts can deliver trials independently, impartially, and according to law” by detaching local judiciaries from local governments.³

Our analysis examines the effectiveness of this judicial recentralization reform. To this end, we collect an original dataset on listed firms’ lawsuits between 2012 and 2018 and code their outcomes and other variables for the litigant and the lawsuit. With this dataset, even descriptive statistics confirm the salient judicial local favoritism: Courts fully supported a local litigant’s claims in 46.8% of the cases, while in only 28% of the cases the court fully supported an external litigant’s requests. Judicial recentralization quickly reduced the salient advantage of local litigants. Employing a differences-in-differences (DID) design, we find that judicial recentralization reduces the winning rate of local litigants by 33.3 percentage points, or roughly 3/4 decrease from the mean, based on our most preferred specification that controls for the full battery of case, litigant, and prefecture covariates.

These empirical findings demonstrate that local judiciaries become more independent from the influence of local political and economic elites as courts depend more on higher-

³The CCP Central Committee’s Decision on Several Major Problems regarding Comprehensively Deepening the Reform (中共中央关于全面深化改革若干重大问题的决定)

level governments after recentralization. To the extent that the protection of local litigants is a ubiquitous and entrenched bias in the Chinese judicial system, the recentralization reform and the reduced advantage of local litigants could, at least partly, account for the expeditious rise of China’s Rule of Law ranking under the Xi Jinping administration in Figure 1. In other words, recentralized judiciaries seem more likely to uphold a level playing field for external litigants, even though local judiciaries are always under the CCP’s firm control.

Before presenting these results, we first review the existing studies that investigate the relationship between decentralization and the rule of law in Section 2. Although some existing theories posit that a decentralized court system can uphold the rule of law, these theories often entail demanding assumptions that are unlikely to hold in reality. In contrast, we advance an alternative view that a recentralized court is more likely to resist the influence of local governments and special interests under certain conditions.

2 Decentralization and the Rule of Law

Is a decentralized or recentralized court system more likely to uphold the rule of law? One prominent view is that decentralized courts controlled by local governments have incentives to respect the rule of law because inter-jurisdictional competition for investment compels them to hold impartial trials (Wang, 2015). This conclusion builds on prior literature that shows that decentralization improves public goods provision (Grossman et al., 2017; Faguet, 2004), enhances regulatory standards (Shi and Xi, 2018), and promotes economic growth (Hatfield and Kosec, 2013; Montinola et al., 1995) because decentralization induces inter-jurisdictional competition and “market-preserving institutions” (Weingast, 1995).

However, several challenges limit the applicability of this theoretical argument. To begin, it is unclear why investors would prefer fair trials over favorable trials. For instance, Beazer and Blake (2018) show that foreign investors do not necessarily prefer to invest in countries that have better judicial environments. Instead, they find that foreign investors are more

likely to invest in places whose institutions mimic their home institutions, suggesting that investors may prefer a familiar, not necessarily fair, court system.

Furthermore, it is unclear why competition is the only possible result of decentralization. Those local governments that are unlikely to win in the inter-jurisdictional competition may give up competing at all (Cai and Treisman, 2005). As a result, decentralization may breed corruption and rent-seeking in settings that lack inter-jurisdictional competition (Fan et al., 2009; Albornoz and Cabrales, 2013; Lessmann and Markwardt, 2010; Mattingly, 2016).

Moreover, local governments do not have to uphold a level playing field to attract investment. For instance, regional protectionist measures that harm fair competition can also help local governments attract local investment and obtain rent-seeking opportunities (Sonin, 2010). Creating judicial biases in favor of local firms is an example for such protectionist practices. Tapping into their influence on local judiciaries, local politicians can instruct the court to favor local litigants. For instance, scholars of Russian judicial politics refer to such practices as “telephone law” because judges must follow politicians’ instructions usually conveyed through phone calls (Hendley, 2009).

Turning to our empirical setting, China’s decentralized court system also makes telephone law possible. The left panel of Figure 2 depicts China’s decentralized civil-law system before the 2014 judicial recentralization reform, which consists of a Supreme People’s Court in Beijing, 31 Higher People’s Courts scattered across 31 provinces, more than 400 Intermediate People’s Courts in prefectures, and more than 3,000 county-level Basic People’s Courts. Although there is not a division between trial courts and appellate courts, most cases receive their first trial in Basic and Intermediate People’s Courts which are the focus of our analysis.⁴ If the litigant is unsatisfied with the decision, s/he can appeal to the next higher-tier court.

Although higher-level courts stand above lower-level courts in this system and can overturn the latter’s rulings, a higher-level court does not directly control lower-level courts. By contrast, local politicians control the personnel and budget of the local judiciary. For

⁴Since second-trial cases might be different in nature, our analysis focuses on first trials.

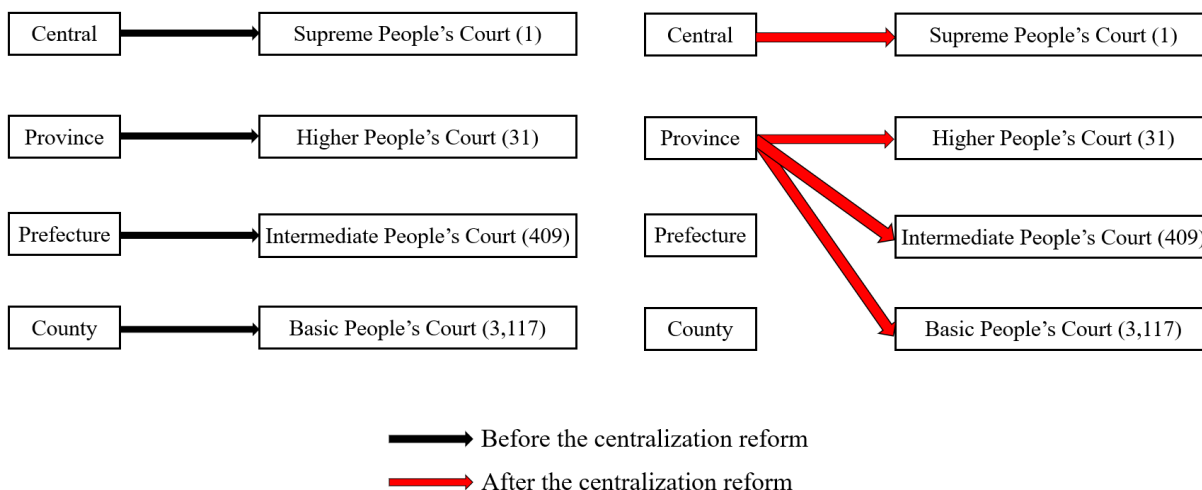


Figure 2: **China's Judicial System**

Notes: Arrows indicate the government (starting point) that manages the personnel and budget of different tiers of courts (ending point). The numbers in parentheses represent the number of courts at each administrative level, based on data in 2012.

instance, the Prefectural party-state apparatus appoints, removes, and promotes judges and (vice) presidents and manages the fiscal budget of the Intermediate People's Court in that prefecture. As a result, court judges are often compelled to follow politicians' instructions that favor local firms and elites (Gong, 2004; Peerenboom, 2009; Ng and He, 2017, Chapter 7), though China's Constitution and central leadership stipulate repeatedly that local judiciaries should adjudicate cases independently.

Examples of local judicial protectionism are not uncommon. For instance, Tencent, a major tech firm in Nanshan District of Shenzhen, won 94.3% of the cases in the Basic People's Court of Nanshan District and 98.15% of the cases in the Intermediate People's Court of Shenzhen between 2018 and 2020. By contrast, Tencent's winning rate in Beijing was only 53.57% during the same period.⁵ Similarly, another major tech company, Huawei, located in Longgang District of Shenzhen, is widely believed as "unbeatable" in the Basic People's Court of Longgang District. Furthermore, Alibaba of Yuhang District in Hangzhou

⁵The Chinese article can be found at <https://baijiahao.baidu.com/s?id=1692466384041368633> (accessed on March 1, 2022).

won nearly all the lawsuits in the Basic People’s Court of Yuhang District when it is the plaintiff.⁶

However, moving beyond the media coverage of salient cases, only a few studies have tried to measure local judicial favoritism in China. To the best of our knowledge, [Long and Wang \(2015\)](#) is the only study that utilizes the nation-wide data (rather than case studies of lawsuits from a region) to directly measure China’s judicial local protectionism. These authors report a strong advantage of local litigants using the data on intellectual property lawsuits from 1985 to 2011. Consistent with their findings, our data on lawsuits involving listed firms also reveal substantial advantage of local litigants (see [Section 5](#)).

In short, despite earlier research’s favorable expectations for decentralized courts, local governments often use their influence on courts to favor local litigants. Furthermore, although local judicial favoritism usually enriches local governments, such local protectionist policies undermine the unity of China’s domestic market and, as a result, undercut the national economic potential ([Young, 2000](#)). Having identified judicial local protectionism as a primary challenge for its court system, governance, and economic development, China launched a recentralization reform in 2014 to reduce local litigants’ advantages. Before we evaluate the effectiveness of this reform, we must first answer why the Chinese leadership believes judicial recentralization is the most reasonable solution. We take on this question in the next section, to which we now turn.

3 Overcoming Local Favoritism by Recentralization

Although previous administrations also experimented with some reforms to overcome local protectionism, Xi Jinping launched the most recent attempt in 2014 that intended to recentralize both personnel and fiscal control of Basic and Intermediate People’s Courts from

⁶The Chinese news report can be found at https://www.sohu.com/a/452555438_120463073 (accessed on June 30, 2022).

counties and prefectures into the hands of provinces. Figure 2 highlights this change in power relations after recentralization with red arrows.

More specifically, the Provincial Party Committee (with the necessary help of the Government, the People’s Congress, and the Higher People’s Court in this province) controls the selection, promotion, and removal of all rank-and-file judges and court leaders, finances the court budget, and manages properties of all Basic and Intermediate People’s Courts in this province after the reform. Put differently, the recentralization reform switches local judiciaries’ political and fiscal principal from counties and prefectures to provinces, even though the same judges continue to work in the recentralized courthouse after reform.

Interestingly, the effectiveness of judicial recentralization seems to rest on a contradictory logic. The Chinese leadership seems to believe that the most viable path to judicial independence (from local governments’ interference) is to make courts more *dependent* on a higher-tier government. In other words, the Chinese government never intends to give full autonomy to courts because they must still follow the CCP’s leadership after judicial recentralization. The reform only changes the tier of Party leadership to which local judiciaries must answer. Hence, China’s judicial recentralization is different from other autocracies’ efforts to hold local officials accountable and attract foreign investment by, at least temporarily, establishing a genuinely autonomous judicial system not controlled by the ruling party (Moustafa, 2014, 2007; Hendley, 1996).

Therefore, it is important to understand why the Chinese leadership pursues judicial recentralization rather than other reform alternatives. We believe that judicial recentralization is perhaps the most reasonable choice in the eyes of Chinese central leaders. Although judicial local protectionism is a deep-seated concern that requires swift actions from the central leadership, forgoing the Party’s control over courts is perhaps equally unacceptable. For instance, when Zhao Ziyang, the then General Secretary of the CCP, prepared a proposal for the Party’s 13th National Congress in 1987 that would eliminate the Central Political-Legal Committee, the Party organ that oversaw courts and other law enforcement departments

(e.g., police and prosecutors), it was vetoed by other senior Party leaders shortly before the Congress meeting, fearing that the reform would undermine the Party's control over courts (Tanner, 1999, p.64). These Party leaders' concern is not groundless. Other countries' experience shows that oppositions may use independent courts to challenge the one-party regime once the ruling party gives up its control over courts (Moustafa, 2007). Empowering opposition forces is perhaps the last thing that any sitting government would like to see.

Moving beyond political calculus, judicial recentralization is also a more practical plan than offering full independence to local judiciaries in China. Given the Communist Party's extensive influence, a court cannot avoid dealing with the Party and its local branches if the court wishes to turn its rulings into concrete results. In fact, obtaining external support from the people and the executive branch are critical consideration for courts in democratic settings too. Without such external support, court decisions will just be a piece of paper even in settings that value democracy and the rule of law. Similarly, Chinese courts must have the Party's blessing if they hope that the party-state will enforce their rulings (Li, 2016).

Hence, an advantage of recentralization is that local judiciaries continue to receive the Party's guidance and support through the Party's provincial officials. Having provincial leadership in their corner also helps local judiciaries resist the pressure from lower-tier governments when the court rules unfavorably to local litigants. Furthermore, provincial officials should also be less likely (albeit not impossible) than lower-tier Party officials to intervene in court trials in favor of local litigants due to following three reasons.

First, a primary reason for local judicial favoritism is to protect local tax revenue because local firms, rather than firms in other regions, contribute to the tax base. To retain these tax payments, local governments must provide favoritism to local firms whose tax payment constitutes a significant portion in the government's fiscal revenue. Otherwise, firms may move to other places that offer favoritism to them (Jia and Mayer, 2017). However, this incentive to protect tax revenue should become weaker after recentralization. Although a

firm might be a major taxpayer in a county or prefecture, its tax payment will look much smaller in the eyes of provincial officials since there are a greater number of firms that pay taxes to the province. In other words, the size of the firm and its tax contribution must be much larger to catch provincial officials' attention and win over their favoritism.

Furthermore, local judicial protectionism also requires a credible threat that a firm will move to another region if the government chooses not to offer favors. We expect that a firm's threat to move also involves a higher cost after recentralization because this firm must relocate to another province rather than another county or prefecture. This may be too costly for many firms that have focused on the market and business connections in a specific province. Since the threat to move is less credible, the province is less likely to give judicial favoritism to local firms than lower-tier governments.

Finally, local firms are more likely to receive favorable judicial decisions because they are better at bribing local politicians or judges than external firms that lack local information and personal connections. We also expect that recentralization should reduce local judicial favoritism due to this rent-seeking logic. This is because higher-level governments (Malesky et al., 2014; Bardhan and Mookherjee, 2000; Bordignon et al., 2008) and courts (Wang, 2018) are, on average, less likely to be captured by special interests. Moreover, the recent anti-corruption campaign has also put provinces under the direct supervision of Beijing's anti-corruption agency and raises the cautiousness of these officials (Wang, 2021). As a result, the price of provincial intervention is much higher than that at lower governmental tiers since provincial officials have stronger political power (i.e., higher value) and are faced with direct central control (i.e., higher risk). Hence, although provincial politicians are not necessarily uninterested in rent-seeking, we expect that they either are more prudent or demand a higher price. By putting the control of courts into the hands of provincial politicians, judicial local favoritism induced by rent-seeking should also decrease.

Hence, the discussion above forms the basis for our primary theoretical argument: We should expect to see that judicial recentralization reduces the advantage of local litigants. To

test this prediction empirically, we have to overcome still another methodological challenge that local judiciaries do not disclose when they implement the recentralization reform. We discuss our solution to this problem in the next section.

4 Data

After the Xi Jinping administration announced the judicial centralization reform in 2014, the reform was first experimented in a few provinces and then expanded to all other provinces in 2015 and 2016.⁷ However, this does not mean that all Basic and Intermediate People’s Courts have started the reform by 2016. In fact, these are the dates when the central government instructed provinces to *start* the planning of the recentralization reform, not necessarily the date that a province implements the reform. It will usually take additional time for the province to prepare for and arrange the reform after receiving Beijing’s instruction. Hence, earlier research (e.g., [Zhou et al. \(2021\)](#)) that uses the date when a province started to plan for the reform, not the date when the province implemented recentralization, will naturally obtain a downward biased estimation.

By contrast, we aim at pinpointing the year when courts *implement* the reform. To this end, we collect original data on the prefectural and provincial expenditure on courts from governments’ annual budget reports. Because a major component of recentralization is to switch the responsibility of financing the court budget from prefectures to provinces, we should find a substantial decrease in prefectures’ expenditure on courts and a significant increase in provinces’ spending on courts after recentralization.⁸ More specifically, we define that a Basic or Intermediate Court has started the recentralization reform if (a) this court’s fiscal expenditures are listed under the budget of the provincial government rather than the

⁷Appendix Section A reports the process of this reform.

⁸We do not study counties because many counties did not disclose their fiscal data on courts when we started the research in 2018.

prefecture or county, or (b) this court’s prefecture government spends 50% less on courts than the baseline year when the central government asked the province to consider the reform. Applying these coding schemes, we can identify the year when Basic and Intermediate Courts started recentralization in 218 prefectures and four province-level municipalities.⁹ We display the distribution of reform years in Appendix Figure B3.

Some discussion on these coding schemes is in order. Criterion (a) is intuitive since including a court’s spending into the provincial fiscal budget is a clear signal that the province, rather than a prefecture or a county, finances the court expenditures. Yet, sometimes we cannot determine criterion (a) because many provinces do not disclose which units are included in their budget reports. In these cases, we examine criterion (b). Although the threshold 50% is arbitrarily selected, cutting the spending on courts by half signals that the prefecture substantially reduces its fiscal responsibilities for local courts.¹⁰ Moreover, additional analysis that employs more stringent thresholds yields similar or more salient results.

Figure 3 gives an example for the coding scheme of criterion (b). Panel A of this figure plots the court expenditures earmarked by the Governments of **Tonghua** Prefecture in **Jilin** Province in **red diamonds** and **Heze** Prefecture in **Shandong** Province in **blue triangles** from 2014 to 2019.¹¹ Although the Prefectural Government of **Heze** maintained a rather stable spending over courts at around 60 million Yuan annually between 2015 and 2019, we see a significant decrease of this figure in **Tonghua** from 88.85 million Yuan in 2015 to 16.22 million

⁹We cannot determine when recentralization starts for some prefectures because these prefectures do not disclose fiscal reports in earlier years. These missing data may create a biased sample. Although we cannot perfectly address this missing data problem, Appendix Table B1 shows that the missingness of reform years is not correlated with a prefecture’s primary socioeconomic conditions.

¹⁰We do not use the complete elimination of the prefectural spending on the court as the criterion (b) because reducing the spending to zero is the *final* step of the reform. We focus on the start of the reform here. Additional analysis in Appendix Table C4 confirms that the complete elimination of the prefectural expenditures on courts yields even stronger results.

¹¹**Heze** does not publish its fiscal spending data before 2015.

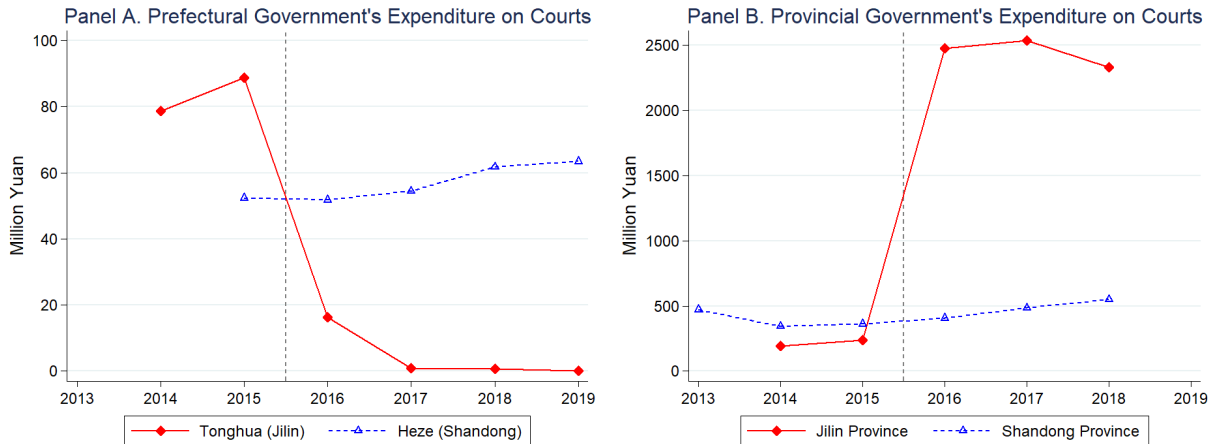


Figure 3: Fiscal Expenditures on Courts

Yuan in 2016. Hence, as an example of our coding scheme, we code 2016 as the start of the judicial reform for local courts in **Tonghua** since the court spending decreased by more than 50% this year. By contrast, the data do not support that local courts in **Heze** Prefecture have started the reform by 2019.

Panel B reports the court expenditure provided by provincial governments and suggests an explanation for the reduced court spending in **Tonghua** after 2015. It shows that the Provincial Government of **Jilin** substantially increased its spending on courts in 2016, the same year as **Tonghua** Prefecture suddenly reduced its fiscal expenditure on courts. These two panels together are consistent with the requirement of the recentralization reform that the responsibilities of financing local courts were transferred from **Tonghua** Prefecture to **Jilin** Province in 2016.

We also consider whether all governmental departments in **Tonghua** reduced their fiscal expenditure due to, for instance, fiscal austerity or a local economic crisis. To exclude this possibility, Appendix Figure B2 reports that prefectural and provincial spending on other law-related or law-enforcement agencies that should not be affected by judicial recentralization (e.g., the People's Congress, police, and civil judicial affairs) remain quite stable between 2014 and 2019. Hence, the plunge of **Tonghua's** expenditure on courts in 2015 was

very likely due to the start of judicial recentralization rather than other reasons that caused fiscal difficulties.

Turning to the lawsuits data, we compile the data on listed firms’ lawsuits from January 1, 2012, to December 31, 2018 primarily from the Wind Database, a financial information platform widely used in the financial sector that updates detailed information on Chinese public firms.¹² We focus on listed firms because, as primary contributors to local tax revenue, listed firms are likely beneficiaries of judicial local protectionism. Moreover, public firms have legal responsibilities to disclose their lawsuits to the public regularly. However, the short span of our data (2012–2018) only allows us to analyze the *short-term* impact of recentralization, focusing predominantly on the lawsuits that received the decision shortly before and after judicial recentralization.

Due to our focus on local protectionism, we further limit the sample to the first-trial cases between a local firm and an external firm. Here, a “local firm” refers to a firm registered in the court’s prefecture or county, while an “external firm” is registered in another prefecture. We also remove the cases tried by the Intermediate People’s Courts in four municipalities, including Beijing, Tianjin, Shanghai, and Chongqing (but keep the cases tried by the Basic People’s Courts in these municipalities), since these Intermediate People’s Courts were already controlled by the Municipal Party Committee before judicial recentralization. These data cleaning procedures leave us with a dataset of 1,184 cases. In the interest of space, Appendix Table B2 reports the summary statistics and data sources.

5 Research Design

We use the following DID design for our empirical analysis.

¹²We also cross-check and supplement our data with China Judgements Online (中国裁判文书网), the official website of Supreme People’s Court that publishes court rulings, and Qichacha (企查查), a third-party platform that publishes basic information of Chinese firms.

$$Y_{i,c,t} = \beta_0 + \beta_1 \text{Recentralization}_{c,t} + \alpha X_{i,t} + \gamma Z_{c,t-1} + \theta_c + \pi_{p,t} + \epsilon_{i,c,t} \quad (1)$$

$Y_{i,c,t}$ is the decision of case i issued by a Basic or Intermediate People’s Court in prefecture c , province p , and year t . To measure the trial result, we use *the local firm wins*, a dichotomous variable coded as one if the court *fully* supports a local firm’s claim and as zero if otherwise. In other words, the court supports *all* requests of the local firm if it is the plaintiff or rejects *all* the claims of the plaintiff if the local firm is the defendant.

Although this is a very demanding coding standard, we still find that courts fully supported local litigants’ position in 46.8% of the cases between 2012 and 2018. Following earlier research (e.g., Wang (2018)), we also use *local firm partially wins* for robustness checks, coded as one if the court supported at least part of the local firm’s request and as zero if otherwise. Using this alternative measure, we find that local firms at least partially won 72% of the cases. In other words, courts fully supported an external firm in only 28% of the lawsuits. These data confirm the salient advantage of local litigants.

Our key explanatory variable, $\text{Recentralization}_{c,t}$, is a dichotomous variable that is coded as one if prefecture c has implemented judicial recentralization in year t , using the coding method introduced in Section 4, and as zero if otherwise. An important question here is why some prefectures implemented the reform earlier than others. For instance, case studies suggest that provinces delayed the reform in wealthier prefectures whose courts usually required higher operational costs (e.g., higher salaries for judges) (Wang, 2020). However, analyzing the data on the 192 prefectures for which we can identify the reform year and have the data on major prefectural variables, we do not find that such variables as GDP per capita, economic growth, or fiscal revenue associate with a later (or earlier) reform date after controlling for province fixed effects (Appendix Table B3). Furthermore, some readers may wonder whether prefectures with more powerful leaders implement the judicial recentralization later (or earlier). To alleviate this concern, we analyze a set of personal characteristics for the mayor (Appendix Table A1) and party secretary (Appendix Table A2)

of the prefecture, and do not find any variable to be correlated with the timing of reform. This means that the timing of reform within a province is orthogonal to local socioeconomic indicators, fiscal resources, and plausibly other local conditions. Nevertheless, we still control for these socioeconomic and fiscal variables, all lagged by one year to avoid the post-treatment bias.¹³

More importantly, we highlight that one benefit of using the DID design is that this research design does not assume a perfect random assignment into treatment. In contrast, the design assumes the parallel trends between the recentralized and unreformed courts before the reform. We present evidence to support this parallel trends assumption in Section 6. Hence, we believe that the control variables we introduced ($X_{i,t}$) and the DID design should substantially alleviate the concern for the selection bias that certain types of prefectures started the judicial reform earlier affects our analysis.

Still, cautious readers may wonder whether recentralized courts receive similar types of lawsuits as other courts. For instance, one may expect that external firms would become more active in pursuing lawsuits against local firms after recentralization.¹⁴ To answer this question, Table 1 compares the cases tried by recentralized courts and unreformed courts. Indeed, this table shows that the proportion of cases submitted by local firms dropped from 60.3% to 50.7% after the recentralization reform, though this difference is not significant at the conventional levels (p-value = 0.136). Hence, we must control for the party that submitted the lawsuit in our analysis since local firms became less likely to be the plaintiff after recentralization.

Furthermore, variables that measure other characteristics of the lawsuit and listed firms' performance present more imbalances in Table 1. For instance, firms that accumulated fewer

¹³Table 2's notes report these control variables.

¹⁴Relatedly, earlier research finds that Chinese judges wish to avoid making decisions on complex cases by encouraging reconciliation (Liebman, 2020). Hence, we test whether judicial recentralization is correlated with a higher or lower chance of reconciliation in Appendix Table B4. Our analysis does not find meaningful correlation between judicial recentralization and the propensity of reconciliation.

Table 1: Balance Table

	Recentralized court	Unreformed court	Difference	<i>P-value</i>
<i>Litigation</i>				
The local firm is the plaintiff	0.507	0.603	-0.096	0.136
The listed firm is the plaintiff	0.557	0.612	-0.055	0.459
The listed firm is the local firm	0.615	0.685	-0.070	0.307
Stake (billion Yuan)	0.017	0.019	-0.003	0.251
Intermediate Court	0.281	0.403	-0.123	0.042
Trial year	2017.4	2015.4	2.003	0.000
The external firm is in different province	0.842	0.847	-0.005	0.894
The plaintiff wins	0.528	0.579	-0.051	0.299
Dispute type: contract	0.538	0.699	-0.161	0.014
Dispute type: loans	0.113	0.058	0.055	0.275
Dispute type: accident liability	0.005	0.001	0.004	0.460
Dispute type: infringement	0.054	0.025	0.029	0.130
Dispute type: others	0.290	0.217	0.073	0.183
<i>Variables for the listed firm</i>				
Return on Assets (ROA) (%)	3.381	1.608	1.773	0.320
Net profit (billion)	-0.129	-0.127	-0.002	0.989
Asset-liability ratio	0.446	0.572	-0.127	0.002
Net cash flow (billion)	-0.076	0.305	-0.381	0.032
Registered capital (billion)	1.123	1.178	-0.055	0.692
Firm age	17.73	16.90	0.829	0.187
Number of employees	3059.0	3858.7	-799.7	0.045
Political connection	0.045	0.316	-0.271	0.000
Government subsidy (%)	1.555	1.536	0.019	0.959
Business tax (%)	1.409	1.286	0.123	0.621

Notes: Government subsidy and business tax are measured as the share of revenue.

debts and lacked political connections filed lawsuits more actively.¹⁵ Moreover, recentralized courts were less likely to receive lawsuits for contract disputes. As well, cases involving the conflict between a local and external litigant were also more likely to be assigned to Basic Courts rather than Intermediate Courts after recentralization.

The presence of these imbalances justifies our analysis of the cross-sectional time-series data on lawsuits rather than a prefecture panel since we must control for these firm- and lawsuit-level imbalances.¹⁶ Furthermore, we also interact these variables with dummy vari-

¹⁵Political connection is a dichotomous variable that equals one if the firm's CEO or any member of the board of directors is member of the People's Congress or Chinese People's Political Consultative Conference or used to work in government at or above the county level.

¹⁶The main analysis does not control for government subsidy and business tax because they contain too

ables indicating (a) whether the listed firm is a local firm and (b) whether the listed firm is the plaintiff to allow the confounding influence to vary across different scenarios.¹⁷

Finally, we control for prefecture fixed effects (θ_c) to capture the influence of invariant prefecture characteristics. Furthermore, we also control for province-year fixed effects ($\pi_{p,t}$) to absorb the influence of province-specific shocks of different years. For instance, the Xi Jinping administration also implemented several other reforms.¹⁸ Most of these other reforms were implemented across the province at the same time. The inclusion of province-year fixed effects can reduce, albeit imperfectly, the influence of these other reform programs.

6 Judicial Recentralization and Court Rulings

We present our findings for the effect of judicial recentralization on the court ruling in this section. Applying the DID design to 1,184 cases, Subsection 6.1 shows that recentralization substantially reduces the advantage of local litigants. The dynamic-effect analysis further shows that our findings are not picking up the pre-recentralization characteristics of lawsuits or litigants. Additional results also demonstrate that our findings are robust to alternative modeling strategies and measurement forms of key variables. Finally, Subsection 6.2 rules out several alternative explanations.

6.1 Main Results

Table 2 contains our main results. Column (1) presents the most parsimonious model that only controls for the prefecture and province-year fixed effects. Although the coefficient in column (1) does not reach the conventional significance level, it shows that local firms are

many missing values. Appendix Table C1 presents the analysis where we sacrifice the sample size and control for these two variables. The results remain robust.

¹⁷Unfortunately, we cannot control for the characteristics of the firm not listed on the stock market, because only public firms are required to disclose their financial data to the public.

¹⁸Appendix Section A contains a brief discussion on other concurrent judicial reforms.

14.8 percentage points less likely to win in a recentralized court. However, column (1) does not control for the characteristics of lawsuits, firms, and prefectures. We gradually add these control variables in columns (2) to (4). Across these more stringent specifications, the coefficient of recentralization remains negative and turns statistically significant.

To interpret our findings, our most preferred specification in column (4) shows that a local firm is 33.3 percentage points less likely to win the case in a recentralized court, a 73% decrease from the average winning rate of local firms. However, this does not necessarily mean that the trials become “fairer” after recentralization since we do not have a baseline for what constitutes “fair” trials. Instead, our analysis demonstrates that court rulings become less favorable to local firms after judicial recentralization, suggesting that the reform has substantially reduced the local bias in trials.

Moreover, we also investigate the dynamic effect of judicial recentralization with the following specification.

$$Y_{i,c,t} = \beta_0 + \sum_{k \geq -2, k \neq +1}^{\leq +4} \delta_k \text{Recentralization}_{c,t+k} + \alpha X_{i,t} + \gamma Z_{c,t-1} + \theta_c + \pi_{p,t} + \epsilon_{i,c,t} \quad (2)$$

The key explanatory variables, $\text{Recentralization}_{c,t+k}$, are a set of dummy variables that indicate the control of a court in prefecture c will be recentralized in k years from year t . Hence, coefficients of $\text{Recentralization}_{c,t+k}$ measure the effect of recentralization before (i.e., $k > 0$) and after its implementation (i.e., $k \leq 0$). To avoid multicollinearity, we omit the year just before recentralization (i.e., $k = 1$). Therefore, all coefficients should be interpreted in comparison with this reference group. Moreover, we also include the same set of control variables as column (4) of Table 2.

Figure 4 contains the results of this exercise. This figure shows that coefficients of $\text{Recentralization}_{c,t+k}$ are not significantly different from zero before recentralization. Hence, our analysis in Table 2 is unlikely to merely pick up the pre-recentralization differences in

Table 2: Judicial Recentralization and the Advantage of Local Litigants

	The local firm wins			
	(1)	(2)	(3)	(4)
Recentralization	-0.148 (0.130)	-0.247** (0.107)	-0.272** (0.114)	-0.333** (0.142)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	0.456	0.456	0.456	0.456
Observations	1184	1184	1184	1184

Notes: Control variables: (1) Litigation controls include the type of dispute (i.e., contract, loans, accident liability, copyright infringement, and others), court level (i.e., Basic or Intermediate Court), the value of the case, and dummy variables indicating whether (a) the plaintiff is a local firm, (b) the plaintiff is a listed firm, and (c) whether the two litigants are from different provinces. (2) Firm controls include the ROA, net profit, asset-liability ratio, net cash flow, registered capital, employment size, firm age, and political connection of the listed firm. All firm controls are interacted with (a) whether the listed firm is the plaintiff and (b) whether the listed firm is local; (3) Prefecture controls include the population size, GDP per capita, GDP growth rates, fiscal revenue per capita, fiscal expenditure per capita, number of listed firms, the size of value-added tax revenue, fixed asset investment, unemployment rate, and the share of secondary and tertiary sector in the GDP. Standard errors clustered at the prefecture level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

the winning rates of local firms. Moreover, local firms are significantly less likely to win after recentralization starts, compared to the year just before recentralization. Hence, the analysis contained in Table 2 reflects the change *after*, not before, recentralization.

We also perform a series of robustness checks for our findings. First, recall that criterion (b) of the coding scheme identifies the start of recentralization by pinpointing the year when the prefecture reduces the expenditure on courts by 50%. A natural question is whether our results are sensitive to this specific cutoff, 50%. We conduct two additional tests to alleviate this concern. First, we replace the dichotomous measure for the reform with a continuous variable that indicates the percentage change of the prefecture’s expenditure on courts. With this continuous measure, we no longer rely on any specific cutoff. We find that

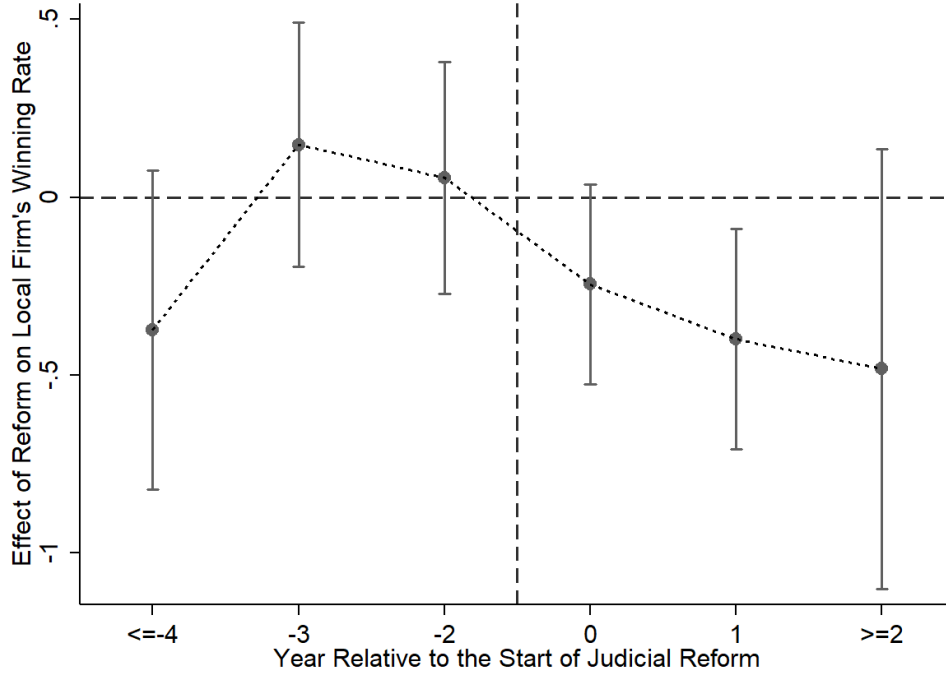


Figure 4: **Dynamic Effects of Judicial Recentralization**

Notes: Each dot displays a point estimate for the effect of the judicial centralization reform on the winning rate of the local firm. The vertical bars are the 95% confidence intervals. Negative numbers on the horizontal axis refer to the number of years it will take before a court starts the reform. Numbers without signs on the horizontal axis indicate the number of years since the court starts the reform. We omit the year before recentralization as the baseline. All coefficients should be interpreted in comparison with this baseline year. Appendix Table C2 reports the regression results.

the decrease of a prefectural government’s expenditure on courts significantly reduces a local firm’s winning rates (see Appendix Table C3).

Furthermore, we also repeat our analysis by replacing the explanatory variable with other cutoffs in Appendix Table C4. This additional test shows that the winning rate of local firms does not decrease if the prefecture’s expenditures on courts do not decrease by more than 50%. The advantage of local litigants only begins to diminish in those courts whose prefecture cuts the court spending by more than 50%. Furthermore, the results are most salient when the province takes over the full responsibility to finance the court budget and the prefecture eliminates expenditure on courts entirely.

Moreover, we also test if our results are robust to alternative functional forms and out-

come measures. First, we repeat the analysis with a Probit regression in Appendix Table C5 to examine whether our results rely on the linear functional form and find similar results. Furthermore, we replace the outcome variable with *the local firm partially wins* as defined in Section 5 and repeat the analysis in Appendix Table C6. Although the statistical significance drops, we still find that a local firm is less likely to partially win the case in a recentralized court. As well, we further construct a three-level categorical variable that indicates the local firm wins completely, wins partially, or loses the case. We analyze multinomial regressions using this categorical variable and report the results in Appendix Table C7. Again, we find that a recentralized court is less likely to fully support the position of a local firm with this alternative modeling strategy. In addition, we examine whether our results are robust to the inclusion of firm fixed effects in Appendix Table C8. Controlling for these fixed effects helps reduce the influence of other unobserved features of listed firms. Our results are generally robust when we implement this test.

Finally, we also explore the effect heterogeneity in Appendix Tables C9 to C12. Although the effect of recentralization does not vary with most features of lawsuits or litigants, the effects are stronger if the plaintiff is a local or/and listed firm. We also find suggestive evidence that recentralization’s effect is stronger if the listed firms are politically connected to the government. These findings are consistent with our argument that local judiciaries must pander to these powerful firms (i.e., local, publicly traded, and/or politically connected firms) before recentralization as they have a stronger influence on local governments.

6.2 Alternative Explanations

This subsection considers two rivalry accounts for our findings. First, one may be concerned that the “types” of lawsuits tried in recentralized courts are different from those in unreformed courts. Recall that our Table 1 confirms that the lawsuits are indeed quite different between recentralized and unreformed courts. Hence, it is possible that these differences in lawsuits, not the effect of recentralization, explain the lower winning rates of local litigants in

recentralized courts, notwithstanding our efforts to control for lawsuit covariates and various fixed effects to reduce the self-selection bias.

To further reduce the concern for selection biases, we conduct an more demanding analysis by restricting the sample to those cases admitted *before* recentralization (but could be tried after recentralization). These cases admintted before recentralization are less likely to fall prey to strategic selection since plaintiffs must decide whether they wish to launch the lawsuit when recentralization has not been announced yet. Although the central government is willing to be more transparent about its plans for judicial reforms, provincial governments do not bother publicly announcing the road map for the recentralization of local courts (which is why we must rely on fiscal data, published in the following year, rather than any policy documents to identify the reform year). Excepting some politically connected firms that could find out the date of reform through their connections (which we analyze separately in Appendix Table C12 and find that recentralization has a stronger effect on these politically connected firms), it seems reasonable to assume that firms submit their lawsuits without considering the impact of a potential judicial recentralization reform since they do not know exactly when (or even whether) that reform would take place.¹⁹

Hence, we repeat our analysis with this more restricted sample to reduce (albeit imperfectly) the concern for firms' strategic adjustment and report the results in Appendix Table D1. Although the statistical significance of our results drops slightly perhaps due to a smaller sample size, this analysis still shows that recentralization reduces the winning rates of local firms substantially.

Moreover, another related concern is that local governments may also act strategically.

¹⁹Appendix Section A shows that local courts in some provinces (e.g., Hubei) were recentralized the year after the central government announced the reform, while some other provinces could delay the reform for at least four years (e.g., The central government authorized Zhejiang to start the reform in 2015, but the fiscal data showed that Zhejiang did not yet start the reform until 2019, the most recent year of data that we have). These data indicate that guessing the reform date would be a risky gamble if the firm does not have insider information from the provincial government.

A prefectural government may expect that its local courts will also be recentralized soon if courts in neighboring prefectures are recentralized. Such prefectures may seize the last chance to interfere with trials. As a result, the winning rate of local firms become much higher shortly before recentralization. This form of strategic adjustment on the side of local governments may still produce a spurious negative correlation between recentralization and winning rates of local firms.

We conduct two additional tests to show that those recentralized courts, not the strategic adjustment from other unreformed courts, explain our findings. The first is a placebo test where we use the reform status of the external firm's court as the explanatory variable. Since the Intermediate People's Court in the external firm's prefecture does not handle the case under study, the recentralization of this court in the external firm's prefecture should not affect the winning rate of the local firm. This is exactly what we find in Appendix Table D2.

We also investigate which courts drive our findings after a province has started to implement the recentralization reform. Appendix Table D3 shows that the winning rates of local firms only drop slightly when a province starts to implement the recentralization reform. In Appendix Table D4, we then further interact the start of the recentralization in a province with whether a prefecture has implemented recentralization. This interaction term helps clarify two things. First, we find that the effect is driven by those prefectures that have implemented the recentralization reform. This shows that our different measures of the reform status are consistent. Second, unreformed courts are not more (or less) likely to favor local firms even if some other prefectures in the same province have started the recentralization reform. Hence, the data do not support the conjecture that local protectionism becomes more rampant when courts in neighboring prefectures are recentralized.

7 Potential Problems of Recentralization

Prior case studies of the recentralization reform have pointed out some potential negative influence of judicial recentralization (Wang, 2019, 2020; Zhou et al., 2021). We examine three potential concerns of recentralization raised by earlier research in this section: namely, the recentralization reform may (a) worsen the case enforcement, (b) reduce the court budget and judges' salaries, and (c) crowd out the provincial budget on other public services. With a nationally representative dataset and a more accurate measure for the implementation date of recentralization, our analysis could give a more unbiased assessment of these concerns for judicial recentralization. Our analysis generally does not find that recentralization exhibits these negative effects.

7.1 Case Enforcement

Case enforcement has long been a challenge for Chinese courts. It often requires local governments' cooperation since courts lack the necessary local knowledge and bureaucratic capacity to enforce some decisions (Zhu, 2016). For instance, local governments control the police and the banking system which are often critical to enforcement. Furthermore, courts may even need local governments' approval before taking any action. For instance, Wang (2015, p.30) presents a case where county government granted a "protective pad" to a firm that says "without the permission of the county party committee and government, no organization or individual is allowed to enter this factory to investigate or fine."

Since local governments continue to hold the bureaucratic resources and local information that are critical to case enforcement after judicial recentralization, one potential strategic adjustment on the side of local governments is that they may protect local firms by obstructing case enforcement, though recentralization reduces the local protectionism through rulings. As a result, the enforcement of court rulings may become more difficult after recentralization.

We examine the impact of judicial recentralization on case enforcement in Appendix

Tables E1 and E2. Since listed firms are required by law to update the status of their lawsuits, including the enforcement status of judicial decision, to the public regularly, we rely on these data to investigate whether the decisions of recentralized courts are less likely to be enforced within one year or are executed with a lower quality. Our analysis does not find that recentralization, on average, makes enforcement more difficult (or easier), rejecting the argument that recentralization worsens case enforcement.

Interestingly, additional analysis shows that recentralization improves the enforcement of the lawsuits that involve firms from the same province (but two different prefectures). The enforcement of cases that involve litigants from different places requires the collaboration of judicial officials in different courts. However, such inter-jurisdictional collaboration may be difficult since local governments can ask the court in their jurisdiction not to cooperate before the recentralization reform. As the province manages courts directly after recentralization, the province could improve the coordination of local judiciaries. Hence, judicial recentralization should improve the execution of the cases that involve litigants from the same province (but from different prefectures/counties). Those cases in which litigants are from different provinces, however, do not benefit from the improved *intra*-province coordination after the reform. This is exactly what we have found in Appendix Tables E1 and E2.

7.2 Court Budget

Does recentralization affect the *size* of the local judicial budget? If the provincial government provides more funding to local courts after recentralization, courts may hire more capable judges who are more likely to produce high-quality and unbiased decisions (Wang, 2015). Hence, it is important to analyze whether recentralization increases or reduces the size of the court budget.

To this end, we analyze the fiscal data on Intermediate People’s Courts from 2014 to 2019 with the DID design and report the results in Appendix Table E3. We do not find that the

recentralization reform increases or decreases the court budget size.²⁰ Further analysis also rejects the idea that recentralization undermines the funding allocated to judges' salaries and benefits or the expenditure on such daily operations as trial and case enforcement. Neither do we find that courts in less (more) wealthy regions receive more (less) funding (see Appendix Table E4). By contrast, we find evidence for our proposed mechanism that recentralization substantially reduces the *fiscal dependence* of local courts on prefectures without changing the size of the court budget (see Appendix Table E5).

7.3 Other Public Services

Finally, we consider whether provincial governments reduce the fiscal expenditure on other public goods provisions after they take up the responsibility of directly financing the budgets of local judiciaries. In Appendix Table E6, we show that provinces continue to provide a similar amount of expenditure on such primary public services as education, research and development, culture, environmental protection, and public safety after the judicial recentralization reform. Hence, we fail to find evidence that judicial recentralization suppresses the public spending on other governmental duties.

8 Conclusion

It is widely believed that unified governments led by powerful politicians or single-party regimes are a threat to judicial independence. For instance, Franklin Roosevelt in the United States (Ramseyer, 1994) and Carlos Menem in Argentina (Larkins, 1998) brought judges to their subordination by packing (or threatening to pack) the Supreme Court with allies. Judges in Japan are also constantly punished by the governing Liberal Democratic Party if they issue decisions against the interests of the government (Ramseyer and Rasmusen,

²⁰Since we are unable to analyze the informal expenditure not listed in the court's fiscal report, we cannot exclude the possibility that the court could obtain more (or less) informal revenue through other sources.

2001). As well, authoritarian governments often suppress the independence of courts and use them to serve other political purposes (Domingo, 2000; Moustafa, 2014; Solomon, 2007; Shen-Bayh, 2018).

While we agree with these findings, the aforementioned analysis largely focuses on the independence of *Supreme Court*. An understudied aspect is whether (and how) a unified government weakens or strengthens the independence of local judiciaries from the interference of local economic and political elites. For instance, local governments in China have constantly used their political and fiscal influence over local judiciaries to press a favorable judicial decision for local firms. Our analysis shows that a judicial recentralization reform launched by Xi Jinping has substantially reduced the winning rates of local firms by weakening local judiciaries' dependence on local governments for fiscal and political support. This is the primary reason why local judiciaries become more confident in issuing decisions unfavorable to local firms after recentralization. To the extent that local judiciaries handle a good portion of lawsuits in China, we believe that these findings also, at least partly, explain the improvement of China's rule of law ranking in the WGI (see Figure 1).

Although our analysis, together with some other studies (Glaeser and Shleifer, 2002; Franco-Vivanco, 2021), shows that unified governments can improve the independence of local judiciaries, it is far from clear that the same results would hold in all settings. At the risk of oversimplification, it seems that the willingness and capacity of the Chinese government to rein in local governments' inappropriate judicial interference play a vital role in the judicial recentralization reform. The Chinese government has identified local judicial protectionism as an area of judicial reform as early as 1999.²¹ Furthermore, not only the central government is under the full control of the CCP, all tiers of local governments receive direct instructions of the CCP's local leadership. This unique institutional feature allows the CCP to implement judicial recentralization without explicit pushbacks. Putting our guesses

²¹The first five-year plan for judicial reform (《人民法院五年改革纲要》) in 1999 stated that “the existence and spread of local protectionism seriously weaken the unity and authority of socialist legal system.”

aside, we also hope to encourage future research that further delineates the conditions under which a unified government strengthens local judicial independence.

Another limitation of our analysis is that our data only allow us to empirically examine the short-term impact of judicial recentralization. It seems that many lawsuits were submitted when plaintiffs did not know about this reform (there were many concurrent judicial reforms introduced by Xi Jinping around 2014) or did not expect the reform to really change anything. This perhaps explains why Subsection 6.2 does not find that firms' strategic adjustment changes our results. Neither do we find that local firms became more willing to settle the conflict outside the courthouse even though local firms were much less likely to win after recentralization (Appendix Table B4). Furthermore, although Table 1 reports that external firms became more likely to file lawsuits after recentralization, this change is not statistically significant. Taken together, these findings lead us to believe that our analysis at best uncovers the short-term impact of judicial recentralization on local protectionism. Further analysis (and data) is needed to find out the longer-term adjustment of firms and local governments to the judicial recentralization program.

Finally, given we focus on judicial independence, our analysis does not discuss the impact of judicial recentralization on other equally important outcomes (except the ones discussed in Section 7). It seems that judicial recentralization may not produce a similar positive impact on these other aspects of rule of law. For instance, earlier research suggests that judicial recentralization does not improve the fairness of administrative lawsuits against the government (Zhou et al., 2021). Hence, additional research is needed to further understand the influence of recentralization on other judicial outcomes.

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Online Appendix

A	Additional Information on Judicial Recentralization	A-1
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A Additional Information on Judicial Recentralization

Since the introduction of the reform and opening-up policy, along with comprehensive economic and social development, China has tried to strengthen its judicial system to meet the public's increasing expectation for better rule of law. The Supreme People's Court promulgated three "Five-year Reform Program for People's Courts" ("人民法院五年改革纲要") in 1999, 2005 and 2009 respectively. These five-year plans covered a series of judicial reforms on the organizational structure of courts, judges' responsibility and accountability, litigation procedures, methods of trial, and enforcement system, etc.

To further deepen the reform and address existing problems within the judicial system, the "Fourth Five-year Reform Program for People's Courts (2014-2018)" ("人民法院第四个五年改革纲要(2014—2018)") was officially published on February 4, 2015.²² The judicial recentralization reform under investigation here is one of the most important components of this reform program, along with such other reforms as the judicial accountability system (司法责任制) and judges' quota system (法官员额制), limiting the power of the adjudication committee, and establishing trans-regional courts and special-purpose courts.

The recentralized management of personnel, financial and material resources of local courts to the provincial level is one major reform to improve the organizational structure of the judiciary and the autonomy of judges, by detaching local courts from the local Party Committees and governments. Given the importance of this reform, it was first experimented in seven provinces in 2014 before introducing the reform to other provinces in 2015 and 2016. Below details the process of the reform dictated by the central government.²³

- June 2014: Shanghai, Jilin, Hubei, Guangdong, Hainan, Guizhou, and Qinghai;
- June 2015: Shanxi, Inner Mongolia, Heilongjiang, Jiangsu, Zhejiang, Anhui, Fujian,

²²A tentative version of this document had been circulated since mid-2014.

²³See the *White Papers on Judicial Reform of Chinese Courts (2013-2018)*. The full text in both English and Chinese can be accessed here: <http://www.court.gov.cn/zixun-xiangqing-144192.html>.

Shandong, Chongqing, Yunnan, and Ningxia;

- March 2016: all remaining provinces.

Interestingly, the seven piloted provinces in 2014 included both richer coastal provinces (e.g., Shanghai and Guangdong) that should have better local judicial systems, medium-income central provinces (e.g., Hubei), and less developed regions in western China (e.g., Guizhou and Qinghai). Hence, it at least seems to us that the central government did not hand-picked provinces that were more likely to see successful experimentation results. Instead, it seems that Beijing sincerely hoped to test the feasibility of judicial recentralization in both richer and poorer regions. Furthermore, we also see a similar balance of coastal richer provinces and inland poorer provinces in the ensuing two waves of reforms in 2015 and 2016.

Although the central government asked provinces to consider the reform in 2014 to 2016, provincial leaders had much discretion on the implementation of reform. In particular, provincial governments decided when and which local courts to be recentralized first based on their local conditions. For instance, earlier research suggests that provinces delayed the recentralization reform in richer prefectures based on case studies of some provinces (Wang, 2020). We acknowledge the possibility of this selection bias and explore which prefecture socioeconomic variables are correlated with an earlier or later reform date in Appendix Table B3. Although our analysis does not find that such socioeconomic indicators as GDP per capita or economic growth rates affect the reform progress, we still control for them in our analysis to further reduce the omitted variable bias. Furthermore, our DID design does not rely on the assumption that judicial recentralization was randomly assigned. Instead, the DID design requires the parallel trend assumption which we test in Figure 4.

Moreover, due to provinces' power on implementing the reform, there may be a delay between the central government's mandate and when a province recentralizes local courts. We plot the reform year coded based on provincial government spending on courts in Appendix Figure A1 (we elaborate on this data and coding scheme in Section 4). For instance,

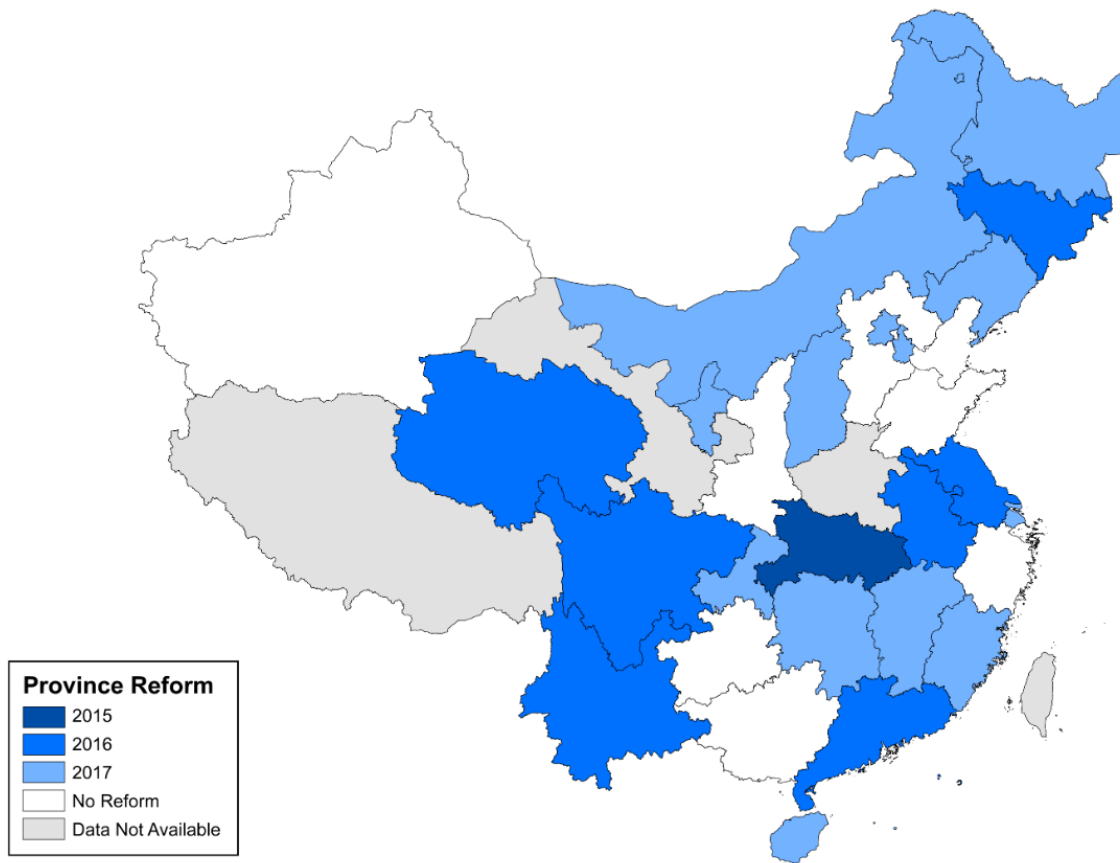


Figure A1: Progress of Judicial Reform at the Province-Level

Guangdong recentralized its Basic and Intermediate People’s Courts in 2016, even though the central government picked Guangdong Province to be one of the pioneers of the reform. Similarly, the central government asked Shandong Province to consider the reform in mid-2015 and Shandong implemented the reform in 2019. Given this difference between the central government’s road map and the actual implementation of the reform, our measurement using expenditure data should better capture the reform progress in each province and prefecture than using the reform year designated in the central road map.

Some readers may wonder why provinces could delay the judicial recentralization reform dictated by the central government. We believe that a primary reason is that provinces indeed need time to flesh out an actionable plan of the reform. In fact, the President of the People’s Supreme Court, Zhou Qiang, acknowledged in his report to the National People’s

Congress in November 2017 that “the recentralization reform needs further institutional re-design (to resolve the new problems and issues during the reform).”²⁴ Most importantly, recentralization requires additional fiscal spending that provincial governments must provide. The provincial government may benefit from slightly delaying the reform to work out a plan to finance the recentralized courts. This perhaps also explains why the central road map only dictated the starting date of the reform, but did not require the completion of the reform by a certain date.

Meanwhile, the Supreme People’s Court also warned that local governments and courts intentionally resisted the reform.²⁵ This concern seems to suggest that those powerful local politicians were more likely to delay the court recentralization. To examine this conjecture, we first utilize the data on party secretaries and mayors of prefectures and test if politically connected prefectures were more likely to resist the judicial recentralization reform. Following [Jia et al. \(2015\)](#) and [Xi et al. \(2018\)](#), we code a prefectural leader as politically connected with the provincial party secretary if the prefectural leader and provincial party secretary (1) share the same *hometown*, (2) attended the same *college*, (3) previously worked in the same *workplace*, and (4) the prefectural leader was *promoted* by the provincial party secretary to his/her current position. Note that we only have the data on prefectural leaders until 2016. Hence, our analysis is confined to the prefecture-year observations until 2017.

We report the results in Appendix Tables [A1](#) and [A2](#) for mayors and party secretaries, respectively. We do not find that cities that had a politically connected mayor or party secretary would launch the recentralization reform earlier than other unconnected cities. Furthermore, other indicators that measure city leaders’ personal characteristics, experience,

²⁴See Zhou Qiang’s report at the National People’s Court [here](#) (accessed on June 13, 2022). The original words in Chinese are the following: 面对改革中出现的新情况、新问题，需要不断探索完善相关政策。比如省以下地方法院财物统管改革有待进一步加强制度设计。

²⁵See Zhou Qiang’s report at the National People’s Court on November 1, 2017 for details. “一是个别地方落实主体责任不力。有的法院领导干部对改革认识不到位，决心不坚定，理解有偏差，存在不担当、不作为、慢作为的情况，导致有些地方改革进度滞后，发展不平衡。”

and seniority also fail to predict the timing of recentralization. These findings suggest that powerful mayors and party secretaries did not use their influence to delay (or expedite) the recentralization reform.

Table A1: Which Prefectures Started the Judicial Recentralization Reform Earlier? (Mayor's Political Power)

A-6

	Hazard = The Prefecture Starts the Judicial Recentralization Reform										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Connection (hometown)	0.143 (0.746)										0.214 (0.799)
Connection (alumni)		0.000 (0.816)									0.418 (0.877)
Connection (workplace)			0.150 (1.121)								0.160 (1.135)
Connection (promotion)				0.114 (0.307)							0.109 (0.471)
Education					0.186 (0.256)						0.219 (0.268)
Age						-0.022 (0.030)					-0.009 (0.034)
Gender							-0.118 (0.354)				-0.107 (0.368)
Tenure length								-0.063 (0.089)			-0.163 (0.152)
First year in office									-0.066 (0.259)		-0.540 (0.425)
Central-level work experience										-0.540 (0.317)	-0.201 (0.341)
Province FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Num. of Prefectures	176	176	176	176	176	176	176	176	176	176	176

Notes: The first four columns measure mayor's connection with the provincial party secretary. Education is a dummy variable indicating whether the mayor holds a college degree (or above). We employ the Cox proportional-hazards model in all regressions and report exponentiated coefficients in this table. Standard errors clustered at the province-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table A2: Which Prefectures Started the Judicial Recentralization Reform Earlier? (Party Secretary's Political Power)

	Hazard = The Prefecture Starts the Judicial Recentralization Reform										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Connection (hometown)	-0.200 (1.038)										-0.142 (1.068)
Connection (alumni)		-0.394 (0.786)									-0.290 (0.785)
Connection (workplace)			0.341 (1.156)								1.050 (1.243)
Connection (promotion)				-0.104 (0.297)							-0.300 (0.463)
Education					0.027 (0.225)						0.073 (0.239)
Age						-0.056 (0.038)					-0.053 (0.042)
Gender							-0.215 (0.534)				0.045 (0.576)
Tenure length								-0.085 (0.095)			-0.197 (0.144)
First year in office									-0.066 (0.247)		-0.250 (0.400)
Central-level work experience										-0.016 (0.489)	-0.093 (0.501)
Province FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Num. of Prefectures	184	184	184	184	184	184	184	184	184	184	184

A-7

Notes: The first four columns measure the city party secretary's connection with the provincial party secretary. Education is a dummy variable indicating whether the mayor holds a college degree (or above). We employ the Cox proportional-hazards model in all regressions and report exponentiated coefficients in this table. Standard errors clustered at the province-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

B Additional Information on Research Design and Data

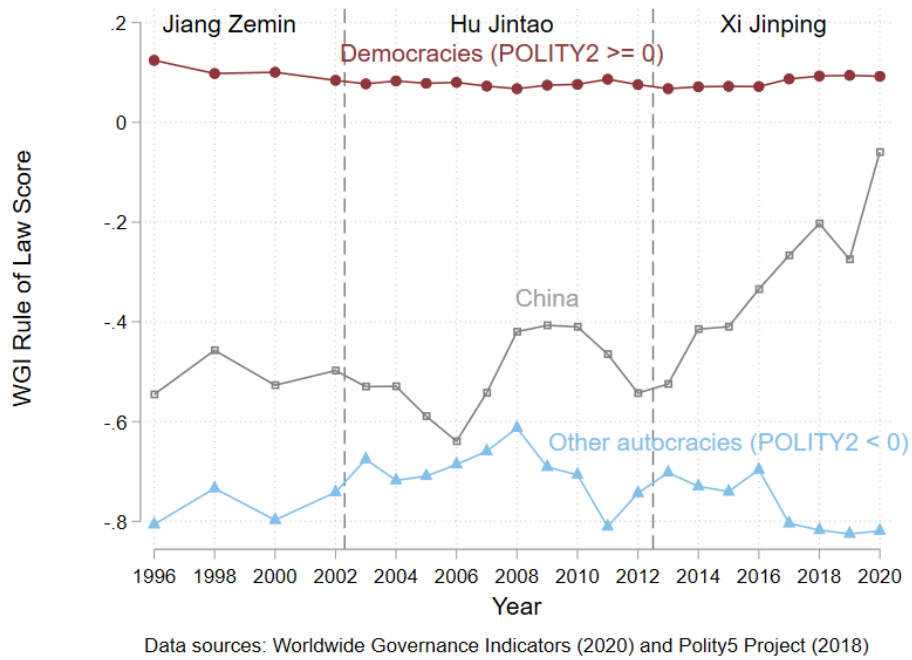


Figure B1: **The WGI Rule of Law Score of China and Other Countries**

Notes: This figure displays the WGI Rule of Law Scores for China (grey squares), democracies (red dots), and other autocracies (blue triangles) from 1996 to 2020. We define autocracies as those countries that receive a negative POLITY2 score and democracies as those that receive a non-negative POLITY2 score from the Polity Project. Since the latest Polity Project (2018) reports the data until 2018, we use the same Polity2 score for 2019 and 2020 as 2018.

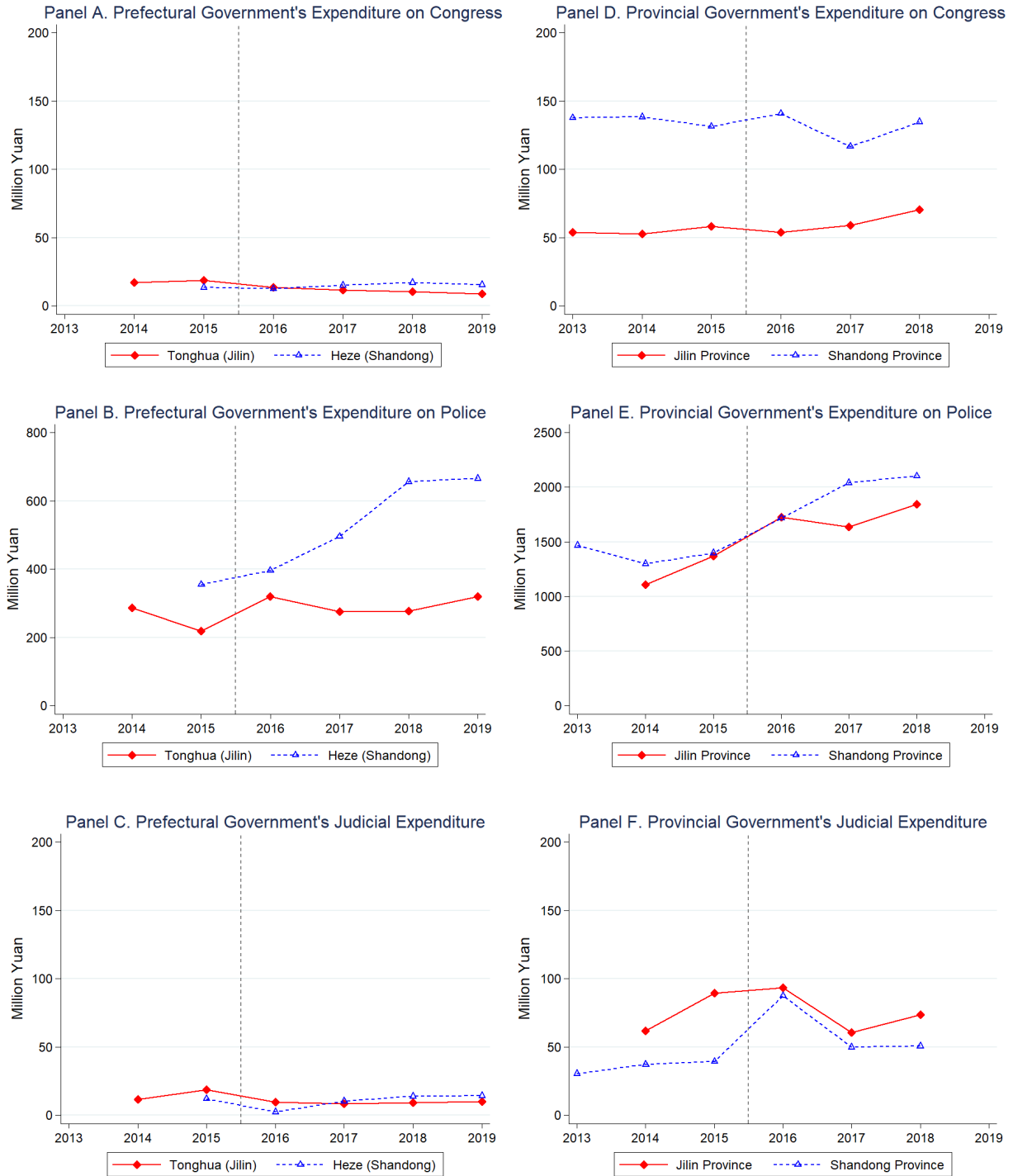


Figure B2: Fiscal Expenditures on Alternative Policy Areas

Notes: This figure displays the government spending on People's Congress (Panels A and D), Police (Panels B and E) and Civil Judicial Affairs (Panels C and Panel F; Unlike courts, Civil Judicial Affairs Bureau is an independent agency responsible for advocating laws and organizing qualification exams for legal professionals, among others.) that should not be affected by judicial recentralization in Tonghua Prefecture and Jilin Province in red diamonds and Heze Prefecture and Shandong Province in blue triangles.

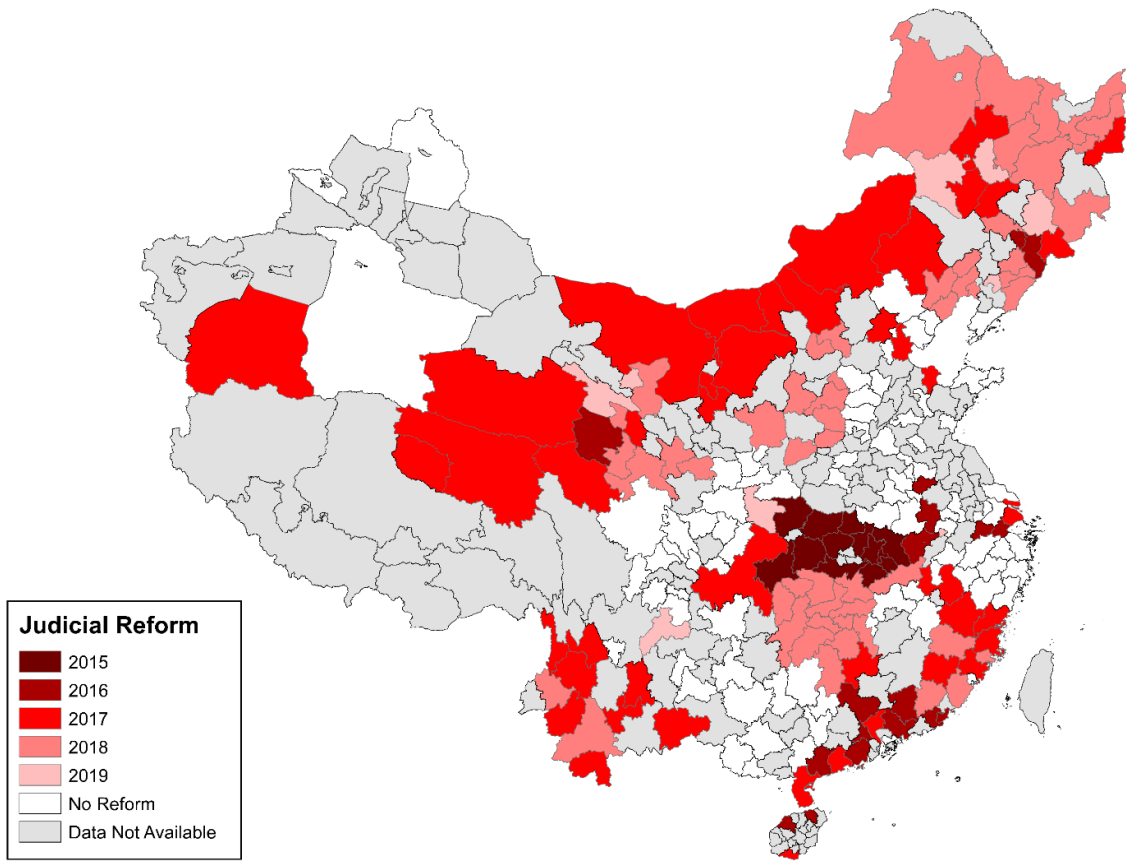


Figure B3: Progress of Judicial Reform at the Prefecture-Level

Table B1: The Missing of Data on Reform Year

	Missing Data on Reform Year (dummy)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Population	0.004 (0.089)									
GDP per capita		-0.037 (0.043)								
GDP growth			0.003 (0.005)							
Fiscal revenue per capita				-0.016 (0.035)						
Number of listed firms					-0.031 (0.031)					
Value-added tax revenue						0.0004 (0.036)				
Fixed asset investment							-0.018 (0.046)			
Unemployment rate								-0.002 (0.025)		
Share of secondary sector in GDP									-0.002 (0.003)	
Share of tertiary sector in GDP										0.001 (0.004)
Province FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Num. of Prefectures	282	282	282	282	282	282	282	282	282	282

Notes: Standard errors clustered at the province-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table B2: Summary Statistics

	Obs	Mean	Std. Dev.	Min	Max	Data Source
<i>Reform & Outcome Variables</i>						
Recentralization (50%, dummy)	1,434	0.178	0.382	0	1	1
Recentralization (100%, dummy)	1,409	0.086	0.280	0	1	1
Recentralization (continuous)	990	0.131	0.269	0	1	1
The local firm wins (full)	1,474	0.468	0.499	0	1	2,3,4
The local firm wins (partial)	1,474	0.720	0.449	0	1	2,3,4
The local firm wins (categorical)	1,474	1.188	0.845	0	2	2,3,4
Enforced within one year	1,375	0.266	0.442	0	1	2,3,4
Enforcement quality	702	0.486	0.500	0	1	2,3,4
<i>Litigation Characteristics</i>						
Local plaintiff	1,614	0.566	0.496	0	1	2,3,4
Local listed firm	1,614	0.669	0.471	0	1	2,3,4
Stake (billion Yuan)	1,515	0.019	0.047	0	0.804	2
Intermediate court	1,614	0.380	0.486	0	1	2,3,4
Trial year	1,614	2016	1.805	2012	2018	2,3,4
Dispute type: contract	1,613	0.647	0.478	0	1	2
Dispute type: loans	1,613	0.063	0.242	0	1	2
Dispute type: accident liability	1,613	0.002	0.050	0	1	2
Dispute type: infringement	1,613	0.033	0.180	0	1	2
Dispute type: others	1,613	0.255	0.436	0	1	2
Plaintiff is listed firm	1,614	0.581	0.494	0	1	2,3,4
Other province	1,614	0.835	0.372	0	1	2,3,4
<i>Firm Characteristics</i>						
ROA (%)	1,585	-0.135	18.579	-131.931	184.751	2
Net profit (billion)	1,610	-0.119	1.168	-7.761	11.450	2
Asset-liability ratio (%)	1,612	57.939	35.147	2.594	433.265	2
Net cash flow (billion)	1,612	0.251	3.555	-39.78	66.615	2
Registered capital (billion)	1,612	1.312	1.949	0.067	25.040	2
Firm age	1,612	17.221	4.976	3	42	2
Number of employees	1,612	4489.653	11446.810	19	187028	2
Government subsidy (%)	1,559	1.414	4.947	0.0002	124.894	2
Business tax (%)	1,346	1.814	3.596	0.001	28.538	2
Political connection	1,614	0.255	0.436	0	1	6
Manufacturing sector	1,614	0.496	0.500	0	1	2
Financial sector	1,614	0.006	0.078	0	1	2
<i>Prefecture Characteristics</i>						
GDP per capita (Yuan)	1,583	98078.22	166714.1	10171	6421762	5
GDP growth (%)	1,583	8.560	2.561	-10.3	18.68	5
Population (million)	1,583	6.658	4.483	0.445	33.92	5
Fiscal revenue per capita (Yuan)	1,583	17302.22	19080.1	666.59	81467.33	5
Fiscal expenditure per capita (Yuan)	1,583	21991.42	23919.63	2618.656	109377.7	5
Number of listed firms	1,614	60.587	80.953	0	308	2
Value-added tax revenue (billion)	1,583	30.586	26.408	-0.222	101.353	5
Fixed asset investment (billion)	1,583	355.016	252.165	2.578	1724.576	5
Unemployment rate (%)	1,582	0.953	0.641	0.031	3.926	5
Share of secondary sector (% of GDP)	1,583	43.628	9.195	19.01	70.6	5
Share of tertiary sector (% of GDP)	1,583	51.773	11.43	20.68	80.56	5

Notes: ROA=Return on Assets. Data Sources: 1. Annual financial reports of prefectural and provincial governments. 2. Wind Financial Database. 3. China Judgements Online (中国裁判文书网), accessed via: <https://wenshu.court.gov.cn/>. 4. The website of Qichacha (企查查), which provides comprehensive firm data including their lawsuit records, accessed via: <https://www.qcc.com/>. 5. China City Statistical Yearbook. 6. CSMAR Figure Characteristic Database.

Table B3: Which Prefectures Started the Judicial Recentralization Reform Earlier?

		Hazard = The Prefecture Starts the Judicial Recentralization Reform												
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
A-13	Population	-0.033 (0.258)											-1.402* (0.791)	
	GDP per capita		-0.146 (0.163)										-0.951 (0.784)	
	GDP growth			0.005 (0.026)									0.011 (0.030)	
	Fiscal revenue per capita				-0.118 (0.134)								0.340 (0.611)	
	Fiscal expenditure per capita					-0.291 (0.237)							-0.675 (0.739)	
	Number of listed firms						-0.102 (0.103)						-0.080 (0.193)	
	Value-added tax revenue							-0.034 (0.107)					0.171 (0.318)	
	Fixed asset investment								-0.038 (0.148)				0.792 (0.514)	
	Unemployment rate									-0.226 (0.231)			-0.057 (0.317)	
	Share of secondary sector in GDP										0.004 (0.010)		0.004 (0.026)	
	Share of tertiary sector in GDP											-0.011 (0.014)	0.011 (0.031)	
	Province FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
	Num. of Prefectures	192	192	192	192	192	192	192	192	192	192	192	192	192

Notes: We employ the Cox proportional-hazards model in all regressions and report exponentiated coefficients in this table. Standard errors clustered at the province-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table B4: Judicial Recentralization and Reconciliation Before Trial

	Reconciliation before trials (dummy)			
	(1)	(2)	(3)	(4)
Recentralization	0.029 (0.076)	0.010 (0.086)	0.0002 (0.079)	-0.092 (0.108)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	0.226	0.0.226	0.226	0.226
Observations	1684	1684	1684	1684

Notes: The outcome is a dummy variable coded as one if the litigants reconcile and withdraw the case before the trial (and so there will not be a trial), and as zero if otherwise (where there would be a trial). We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

C Robustness Checks for Main Results

Table C1: Judicial Recentralization and the Advantage of Local Litigants: Controlling for Additional Firm Attributes

Control for:	The local firm wins (dummy)			
	(1)	(2) Government subsidy (%)	(3) Business tax (%)	(4) Both
Recentralization	-0.333** (0.142)	-0.227* (0.119)	-0.267** (0.126)	-0.455*** (0.143)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls	✓	✓	✓	✓
Firm Controls	✓	✓	✓	✓
Prefecture Controls	✓	✓	✓	✓
Dependent Variable Mean	0.456	0.463	0.455	0.462
Observations	1184	1141	971	937

Notes: The results shown in this table serve as a robustness check for Table 2. We control for two additional attributes of the listed firm, namely business tax and government subsidy, in columns (2) to (4) of this table. Both variables are measured as the ratio to a firm's revenue. We drop these variables from our main analysis because both variables contain many missing values. This table shows that our results are still robust after controlling for them. We use the same set of control variables in this table as Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C2: Dynamic Effects of the Judicial Recentralization Reform

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
More than three years before reform started	-0.060 (0.288)	-0.079 (0.237)	-0.044 (0.248)	-0.373 (0.271)
Three years before reform started	0.292* (0.173)	0.158 (0.185)	0.230 (0.169)	0.148 (0.207)
Two years before reform started	0.004 (0.173)	-0.041 (0.166)	-0.008 (0.155)	0.055 (0.197)
The year reform started	-0.244 (0.207)	-0.294* (0.167)	-0.307* (0.174)	-0.245 (0.169)
One year after reform started	-0.031 (0.144)	-0.242* (0.123)	-0.229* (0.136)	-0.399** (0.187)
At least two years after reform started	0.058 (0.181)	-0.088 (0.135)	-0.077 (0.166)	-0.482 (0.373)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	0.480	0.480	0.480	0.480
Observations	1043	1043	1043	1043

Notes: We plot Figure 4 based on column (4). We use the same set of control variables as those reported in Table 2. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C3: Judicial Recentralization and the Advantage of Local Litigants: Continuous Measurement

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
Recentralization	-0.353*	-0.427**	-0.388**	-0.449*
	(0.197)	(0.183)	(0.194)	(0.234)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	0.465	0.465	0.465	0.465
Observations	823	823	823	823

Notes: The results shown in this table serve as a robustness check for Table 2. We use a continuous measure, the percentage decline in prefectural government’s expenditure on courts, to measure the progress of reform. Hence, the larger magnitude of this drop in expenditure, the more progress that local courts in a prefecture has made. The sample size is smaller since we can only conduct the analysis on prefectures where data on fiscal expenditure on courts is available (i.e., criterion (b)). We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C4: The Effect of Reduction in Prefectural Government Spending on Courts

	The local firm wins (dummy)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reduce by 0%-9.9%	0.117							-0.005
[$N = 448$]	(0.100)							(0.128)
Reduce by 10%-29.9%		-0.165						-0.197
[$N = 18$]		(0.135)						(0.158)
Reduce by 30%-49.9%			-0.044					-0.180
[$N = 7$]			(0.172)					(0.192)
Reduce by 50%-69.9%				-0.199				-0.242
[$N = 37$]				(0.135)				(0.160)
Reduce by 70%-89.9%					-0.189			-0.231
[$N = 62$]					(0.244)			(0.252)
Reduce by 90%-99.9%						-0.500*		-0.457
[$N = 4$]						(0.272)		(0.341)
Reduce by 100%							-0.585**	-0.604**
[$N = 85$]							(0.252)	(0.266)
Prefecture FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Litigation Controls	✓	✓	✓	✓	✓	✓	✓	✓
Firm Controls	✓	✓	✓	✓	✓	✓	✓	✓
Prefecture Controls	✓	✓	✓	✓	✓	✓	✓	✓
Dependent Variable Mean	0.461	0.461	0.461	0.461	0.461	0.461	0.461	0.461
Observations	1333	1333	1333	1333	1333	1333	1333	1333

Notes: This table serves as a robustness check for the results in Table 2. We divide the reduction of prefectural governments' expenditure on courts into different groups and estimate the effect of each group respectively. More specifically, the first group is coded as one if the spending on courts is reduced by less than 10%; the second by more than 10% but less than 30%; and so on and so forth. The final group represents the complete elimination of the prefectural government's spending on courts (i.e., reduction by 100%). The number of cases in each group are included in brackets. We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C5: Judicial Recentralization and the Advantage of Local Litigants: Probit Regression

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
Recentralization	-0.360 (0.223)	-0.501** (0.217)	-0.528** (0.209)	-0.443* (0.241)
Prefecture FE	✓	✓	✓	✓
Trial Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	0.484	0.484	0.484	0.484
Observations	1029	1029	1029	1029

Notes: We test if the main results contained in Table 2 is dependent on the linear functional form of an OLS regression. We do so by employing the Probit model and repeating the analysis in this Appendix table. We cannot add the province-year fixed effects here because the function cannot converge. We control for trial-year fixed effects instead. The coefficient of the centralization reform remains negative and statistically significant. We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C6: Judicial Recentralization and the Advantage of Local Litigants: Less restrictive measure for trial outcomes

	The local firm partially wins			
	(1)	(2)	(3)	(4)
Recentralization reform	-0.061 (0.149)	-0.136 (0.125)	-0.155 (0.127)	-0.360** (0.143)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	0.720	0.720	0.720	0.720
Observations	1184	1184	1184	1184

Notes: We examine whether the results reported in Table 2 are robust to an alternative outcome measure. This new measure for the outcome variable is coded as one if the court at least supports part of the claims of the local firm, and is coded as zero if the local firm completely loses. Although the significance level drops, the coefficient of centralization reform remains negative and is significant at the one percent level in the most stringent specification shown in column (4). We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C7: Judicial Recentralization and the Advantage of Local Litigants: Multinomial Regression

	The local firm partially wins			
	(1)	(2)	(3)	(4)
Recentralization	0.034 (0.537)	-0.168 (0.563)	-0.197 (0.616)	-0.459 (0.761)
	The local firm completely wins			
	(1)	(2)	(3)	(4)
Centralization reform	-0.601 (0.426)	-0.957** (0.415)	-1.025** (0.459)	-1.012* (0.575)
Prefecture FE	✓	✓	✓	✓
Trial Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	1.176	1.176	1.176	1.176
Observations	1184	1184	1184	1184

Notes: In this table, we classify trial outcomes into three categories, including whether the local firm loses completely, wins partially, or wins completely, and repeat the analysis with multinomial regressions. The reference category is the group where local firm loses completely. The results suggest that the centralization reform substantively reduces the chances that local firms win *completely* against an external firm. However, the probability that local firms partially win does not decrease significantly. These results help us understand why the results in Table C6 are less salient than our main results. We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C8: Judicial Recentralization and the Advantage of Local Litigants: Controlling for Listed Firm Fixed Effects

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
Recentralization	-0.345*** (0.125)	-0.572** (0.240)	-0.727*** (0.251)	-0.367 (0.405)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls	✓	✓	✓	✓
Firm Controls	✓	✓	✓	✓
Prefecture Controls	✓	✓	✓	✓
Firm FE		✓	✓	✓
Firm FE × Plaintiff			✓	✓
Firm FE × Local firm				✓
Dependent Variable Mean	0.466	0.473	0.486	0.506
Observations	1082	842	729	674

Notes: We examine whether the results reported in Table 2 are robust to the inclusion of listed firm fixed effects. We do so by controlling for listed firm fixed effects in column (2), and interacting firm fixed effects with whether the listed firm is the plaintiff in column (3) and whether the listed firm is the local firm in column (4). Moreover, for comparison, column (1) reproduces column (4) of Table 2. Careful readers may notice some slight differences between these two columns. This is because we use a different Stata package (i.e., “reghdfe”), which is particularly suitable for controlling for high-dimensional fixed effects, to produce results of this Appendix Table (Correia, 2016). Our results are generally robust, only except for column (4) where the standard error is large. This insignificant coefficient might be the result of fewer observations but large number of fixed effects added. We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C9: Heterogeneous Effects of Judicial Recentralization: Litigation Characteristics

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
Recentralization	-0.243 (0.167)	-0.300** (0.144)	-0.268* (0.147)	-0.296** (0.142)
Recentralization × Other Province	-0.132 (0.144)			
Recentralization × Local Plaintiff		-0.059 (0.115)		
Recentralization × Basic Court			-0.162 (0.121)	
Recentralization × Stake				-1.638 (1.073)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls	✓	✓	✓	✓
Firm Controls	✓	✓	✓	✓
Prefecture Controls	✓	✓	✓	✓
Dependent Variable Mean	0.456	0.456	0.456	0.456
Observations	1184	1184	1184	1184

Notes: We explore the heterogeneous effects of judicial recentralization on litigation attributes in this table. More specifically, we examine whether the effect of recentralization varies if: (a) the external firm is located in a different province; (b) the local firm is plaintiff; (c) the case is trialed in Basic People’s Court; and (d) the amount of compensation requested is high. The effect of centralization on local firm’s winning rate is consistently negative, and we do not find heterogeneity across most dimensions. The only possible exception is column (1). Although the coefficient of recentralization is not significant, it is still negative and relatively large in magnitude. While this result may suggest that the effect is mainly driven by cases where the external firm is from a different province (statistically significant at the 5% level), another explanation is that we have more cases between firms from different provinces than that from the same province in our sample. Furthermore, since “stake” is a continuous variable, we also check potential non-linear interactive effect and sufficient common support in the data following [Hainmueller et al. \(2019\)](#), which confirm the results in column (4). We use the same set of control variables as in [Table 2](#). Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C10: Heterogeneous Effects of Judicial Recentralization: Firm Characteristics

	The local firm wins (dummy)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Recentralization	-0.293*	-0.441***	-0.326**	-0.333**	-0.324**	-0.477***	-0.305**	-0.344**	-0.313**
	(0.156)	(0.158)	(0.145)	(0.150)	(0.152)	(0.118)	(0.150)	(0.146)	(0.140)
Recentralization × Listed Firm as Plaintiff	-0.063								
	(0.118)								
Recentralization × Local Listed Firm		0.135							
		(0.122)							
Recentralization × ROA (%)			-0.001						
			(0.003)						
Recentralization × Asset-liability ratio (%)				0.001					
				(0.187)					
Recentralization × Government Subsidy (%)					-0.006				
					(0.006)				
Recentralization × Business Tax (%)						0.006			
						(0.013)			
Recentralization × Manufacturing Sector							-0.052		
							(0.081)		
Recentralization × Financial Sector								0.098	
								(0.451)	
Recentralization × Political connection									-0.202
									(0.151)
Dependent Variable Mean	0.456	0.456	0.456	0.456	0.456	0.455	0.456	0.456	0.456
Observations	1184	1184	1184	1184	1184	971	1184	1184	1184

Notes: We explore the heterogeneous effects of recentralization on the listed firm's attributes in this table. In particular, we estimate the moderating effect of (1) being the plaintiff; (2) being the local firm; (3) ROA; (4) asset-liability ratio; (5) government subsidy (divided by the total revenue); (6) business tax (divided by the total revenue); (7) from the manufacturing sector; (8) from the financial sector; and (9) firm's political connection. In all columns, we control for the prefecture and province-year fixed effects, as well as the same set of control variables used in Table 2. The coefficient of recentralization reform is consistently negative and does not show clear heterogeneity. Moreover, since the interaction terms considered in column (3) to column (6) are continuous, we further plot the effects and distribution of the data in Appendix Figure C1 following Hainmueller et al. (2019). The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

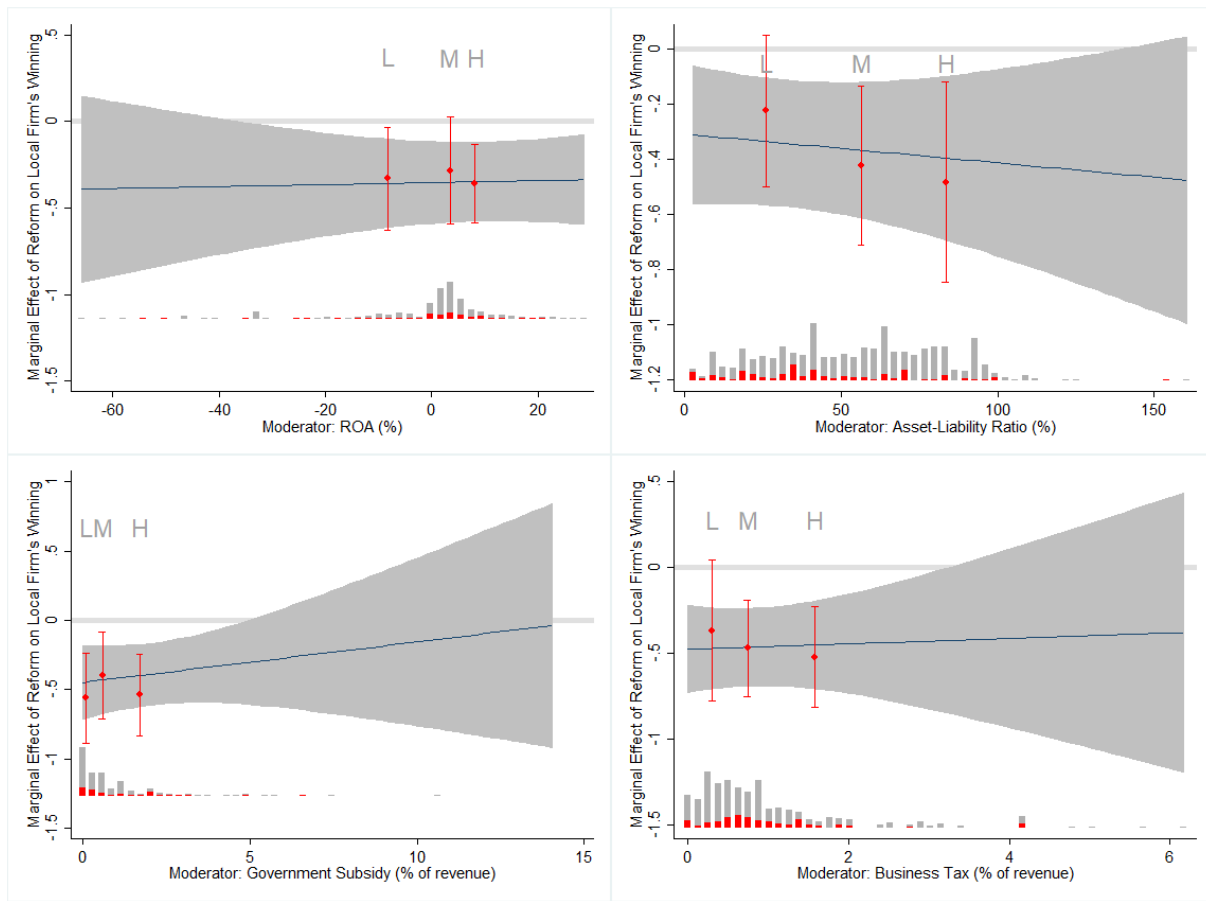


Figure C1: Moderating Effects of Continuous Firm Variables

Notes: This figure serves as an additional check on the moderation effect of continuous firm variables in Appendix Table C10. We plot the effects for low, medium, and high level of the moderator based on tertiles using the binning estimator (red dot). The distribution of data is shown below the estimates, where red bars represent observations in the treatment (reformed) group and gray bars represent observations in the control (unreformed) group. In general, these results confirm the implications from Appendix Table C10 that the judicial reform does not show heterogeneous effects based on firm's profitability or government support. Nevertheless, two attributes should be taken with caution, namely firm's asset-liability ratio and business tax. In both cases, the effect for the high group is much larger than that of the low group (more than doubled for asset-liability ratio), and also increases in statistical significance. This indicates that the effect of centralization is even stronger for firms with higher debt rate or paid more tax. One plausible explanation is that, since firms which contribute more tax or borrow more easily are more likely to be the former beneficiaries of government intervention in trials, they suffer more due to the loss of local protectionism after the reform. Nevertheless, the finding on tax contribution should be interpreted with caution since we have a small drop in sample size due to missing data on this variable.

Table C11: Heterogeneous Effects of Judicial Recentralization: Types of Plaintiff

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
Recentralization	-0.168 (0.142)	-0.204 (0.133)	-0.196 (0.144)	-0.245 (0.167)
Recentralization \times Local non-listed plaintiff	-0.190 (0.176)	-0.183 (0.162)	-0.168 (0.162)	-0.185 (0.165)
RefRecentralizationnorm \times Non-local listed plaintiff	-0.127 (0.163)	-0.158 (0.144)	-0.151 (0.168)	-0.194 (0.171)
Recentralization \times Local listed plaintiff	-0.008 (0.168)	-0.027 (0.151)	-0.073 (0.155)	-0.100 (0.154)
Recentralization + Recentralization \times Local non-listed	-0.358** (0.167)	-0.387** (0.158)	-0.364** (0.155)	-0.430** (0.184)
Recentralization + Recentralization \times Non-local listed	-0.295* (0.150)	-0.362*** (0.134)	-0.347** (0.146)	-0.439*** (0.162)
Recentralization + Recentralization \times Local listed	-0.176 (0.138)	-0.232* (0.132)	-0.269* (0.144)	-0.346** (0.165)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	0.456	0.456	0.456	0.456
Observations	1184	1184	1184	1184

Notes: In this table, we use a 2×2 classification based on whether the plaintiff is a local or a listed firm, and examine whether the effect differs across groups. We drop the category which the plaintiff is an external and non-listed firm as the reference group, so all coefficients should be interpreted with reference to this group. The results suggest that the effect of centralization reform is more salient for cases which the plaintiff has some advantage as being the local firm or the listed firm (or both). We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table C12: Heterogeneous Effects of Judicial Recentralization: Political Connections

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
Recentralization	-0.135 (0.134)	-0.313** (0.140)	-0.145 (0.137)	-0.345** (0.140)
Recentralization × Any political connection	-0.105 (0.151)	-0.202 (0.151)		
Recentralization × Connection at the county level			-0.089 (0.160)	-0.240 (0.213)
Recentralization × Connection at the prefecture level			-0.400*** (0.094)	-0.154 (0.097)
Recentralization × Connection at/above the province level			-0.150 (0.225)	-0.312* (0.183)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls		✓		✓
Firm Controls		✓		✓
Prefecture Controls		✓		✓
Dependent Variable Mean	0.456	0.456	0.456	0.456
Observations	1184	1184	1184	1184

Notes: In this table, we examine whether the effect of judicial recentralization differs by the type of political connection held by the listed firm. We drop the category which the listed firm does not have any political connection as the reference group, so all coefficients should be interpreted with reference to this group. We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

D Results for Alternative Explanations

Table D1: Judicial Recentralization and the Advantage of Local Litigants: Sub-sample of the Cases Admitted Before the Reform

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
Recentralization	-0.104 (0.180)	-0.284* (0.149)	-0.286** (0.121)	-0.468 (0.291)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	0.455	0.455	0.455	0.455
Observations	1022	1022	1022	1022

Notes: In this table, we restrict our sample to the lawsuits accepted before the reform. This mitigates the concern that firms may strategically decide whether to bring cases to courts after the reform. As is shown here, the effect of recentralization is still significant for this sub-sample of cases. Hence, firm's strategic behavior, even if exists, cannot explain our main findings. We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table D2: A Placebo Test

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
recentralization (prefecture of the external firm)	-0.070 (0.066)	-0.062 (0.055)	-0.046 (0.052)	-0.042 (0.053)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Prefecture Controls				✓
Dependent Variable Mean	0.472	0.472	0.472	0.472
Observations	862	862	862	862

Notes: This table reports the results of a placebo test. We use the reform status of the external firm's prefecture as the explanatory variable. Since this court does not try the case, its reform status should have no impact on the outcome of trials. Consistent with this expectation, the coefficients of recentralization are statistically insignificant and close to zero in magnitude. There is a drop in sample size since we cannot identify the reform status for all external firms' local courts. We use the same set of control variables as in Table 2. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table D3: Province-level Reform and the Winning Rate of Local Firms

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
Recentralization	-0.111 (0.065)	-0.100 (0.066)	-0.070 (0.063)	-0.086 (0.055)
Province FE	✓	✓	✓	✓
Trial Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Province Controls				✓
Dependent Variable Mean	0.468	0.468	0.468	0.468
Observations	1262	1262	1262	1262

Notes: Since the province will take over the responsibility of funding local courts after the reform, we identify the start of the reform in a province by pinpointing the year when the provincial government's expenditure over courts increases by at least 100% than the baseline year when the central government allows the province to consider the reform. Although this cutoff, 100%, is arbitrarily selected, doubling the expenditure means that the provincial government is at least starting to finance more local courts. We plot the data of provincial reform progress in Figure [A1](#). We exclude Intermediate Courts in four municipalities (i.e., Beijing, Tianjin, Shanghai, and Chongqing) because these counts were already under the control of the municipal government (equivalent to provincial level) before the reform. We use the same set of control variables as in Table [2](#). Standard errors clustered at the province-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table D4: The Heterogeneous Effect of the Province-level Reform

	The local firm wins (dummy)			
	(1)	(2)	(3)	(4)
Provincial Reform	-0.070 (0.078)	-0.066 (0.079)	-0.024 (0.077)	-0.041 (0.062)
Provincial Reform \times Prefectural Reform	-0.184* (0.100)	-0.147 (0.110)	-0.173 (0.126)	-0.178 (0.136)
Provincial + Provincial \times Prefectural Reform	-0.254*** (0.077)	-0.213** (0.082)	-0.197** (0.092)	-0.220* (0.122)
Province FE	✓	✓	✓	✓
Trial Year FE	✓	✓	✓	✓
Litigation Controls		✓	✓	✓
Firm Controls			✓	✓
Province Controls				✓
Dependent Variable Mean	0.463	0.463	0.463	0.463
Observations	1125	1125	1125	1125

Notes: We further decompose the effect of province-level recentralization reform by interacting the province-level reform with a dummy variable indicating whether the court's prefecture has started the reform. This table shows that the effect of reform is only significant in prefectures which has started the reform, but not in those that have not implemented the reform yet. We use the same set of control variables as in Table 2. Standard errors clustered at the province-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

E Additional Tests on Potential Problems of Recentralization

Table E1: The Recentralization Reform and the Enforcement of Court Rulings

	Enforced within one year			
	(1)	(2)	(3)	(4)
Reform started	0.234 (0.185)	0.328 (0.200)		
Reform started \times Inter-province		-0.136 (0.162)		
Reform started + interaction		0.191 (0.196)		
Reform completed			0.272 (0.314)	0.775** (0.351)
Reform completed \times Inter-province				-0.492*** (0.125)
Reform completed + interaction				0.283 (0.312)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls	✓	✓	✓	✓
Firm Controls	✓	✓	✓	✓
Prefecture Controls	✓	✓	✓	✓
Dependent Variable Mean	0.263	0.263	0.264	0.264
Observations	1110	1110	1091	1091

Notes: To analyze the case enforcement, we create a dummy variable that equals one if the ruling is enforced within one year and equals zero if the ruling is either not enforced or the enforcement takes longer than one year. Turning to the explanatory variables, we examine both the start of the reform, which we use as the main explanatory variable for the analysis in the previous section, and the completion of the reform. We define the latter as the year when the prefectural government *completely* eliminates the expenditure over courts, while, as a comparison, the start of the reform only requires the expenditure to decrease by 50%. This means that the provincial government takes over the full responsibility to fund local courts when the reform is completed. In columns (2) and (4) we further interact the reform variable with a dummy variable that indicates whether the external firm is from another province. These two columns further demonstrate that recentralization improves the enforcement of cases that both litigants are from the same province (but are located in two different prefectures). We use the same set of control variables as those reported in Table 2. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed effects. Interaction = Reform started/completed \times Inter-province.

Table E2: The Recentralization Reform and the Quality of Enforcement

	Enforcement quality			
	(1)	(2)	(3)	(4)
Reform started	0.936*** (0.264)	1.205*** (0.262)		
Reform started \times Inter-province		-0.366 (0.286)		
Reform started + interaction		0.839*** (0.298)		
Reform completed			0.783* (0.457)	1.731** (0.622)
Reform completed \times Inter-province				-0.895*** (0.330)
Reform completed + interaction				0.836* (0.463)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Litigation Controls	✓	✓	✓	✓
Firm Controls	✓	✓	✓	✓
Prefecture Controls	✓	✓	✓	✓
Dependent Variable Mean	0.498	0.498	0.500	0.500
Observations	546	546	536	536

Notes: This table presents a robustness check for the results in Table E1, using the quality of enforcement as the outcome variable. The outcome variable is a dummy that equals one if all items of the court decision are executed and equals zero if otherwise. To this end, we must restrict the sample to those verdicts that have reported the completion of enforcement by the end of 2018. In other words, those cases that are still being enforced by the court are not included here because we do not have the final result of the enforcement stage. We find that recentralization raises the probability of enforcing all items on the verdict especially for those lawsuits whose external firm is not from a different province. We use the same set of control variables as those reported in Table 2. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table E3: Judicial Recentralization and the Court Budget

	(1)	(2)	(3)	(4)	(5)	(6)
	Total revenue (<i>log</i>)	Total spending (<i>log</i>)	Basic salary (<i>log</i>)	Other benefits (<i>log</i>)	Trial spending (<i>log</i>)	Enforcement spending (<i>log</i>)
Recentralization	0.131 (0.191)	0.157 (0.170)	0.211 (0.128)	-0.057 (0.213)	-0.046 (0.180)	0.011 (0.143)
Prefecture FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
Prefecture Controls	✓	✓	✓	✓	✓	✓
Dependent Variable Mean	4.156	4.091	2.103	2.991	1.139	0.273
Num. of Intermediate Courts	175	175	172	172	174	174
Observations	757	747	632	629	694	692

Notes: This table investigates whether judicial recentralization affects the size of fiscal revenue (column (1)), total expenditure (column (2)), the spending on judges' basic salary (column (3)) and other benefits (column (4)), the spending on trials (column (5)), and the spending on case enforcement (column (6)). Other benefits include benefits for judges beyond their basic salary, such as allowance and bonus. Prefecture controls include the population size, GDP per capita, GDP growth rate, fiscal revenue per capita, fiscal expenditure per capita, number of listed firms, value-added tax size, fixed asset investment, unemployment rate, and the share of secondary and tertiary sector in the GDP. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table E4: The Heterogeneous Effect of Recentralization on the Court Budget

	Total revenue (<i>log</i>)		Total spending (<i>log</i>)	
	(1)	(2)	(3)	(4)
Recentralization	-1.481 (1.826)	-0.366 (1.010)	-0.627 (1.434)	0.315 (0.683)
Recentralization × 2014 GDP per capita	0.147 (0.172)		0.072 (0.137)	
Recentralization × 2014 fiscal revenue		0.059 (0.121)		-0.019 (0.084)
Prefecture FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Prefecture Controls	✓	✓	✓	✓
Dependent Variable Mean	4.156	4.156	4.091	4.091
Observations	755	755	745	745

Notes: We explore the heterogeneous effect of the reform on the fiscal revenue and expenditure of local courts in this table. The results do not support the notion that courts in less developed regions are more likely to benefit from the reform. To avoid post-treatment bias, we use the GDP per capita and fiscal revenue size in 2014 as the moderator. Moreover, since both GDP per capita and the fiscal revenue size are continuous variables, we plot the interactive effect in Appendix Figure E1. The patterns are consistent with the results in this table. Prefecture controls include the population size, GDP growth rate, number of listed firms, value-added tax size, fixed asset investment, unemployment rate, and the share of secondary and tertiary sector in the GDP. Standard errors clustered at the prefecture-level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

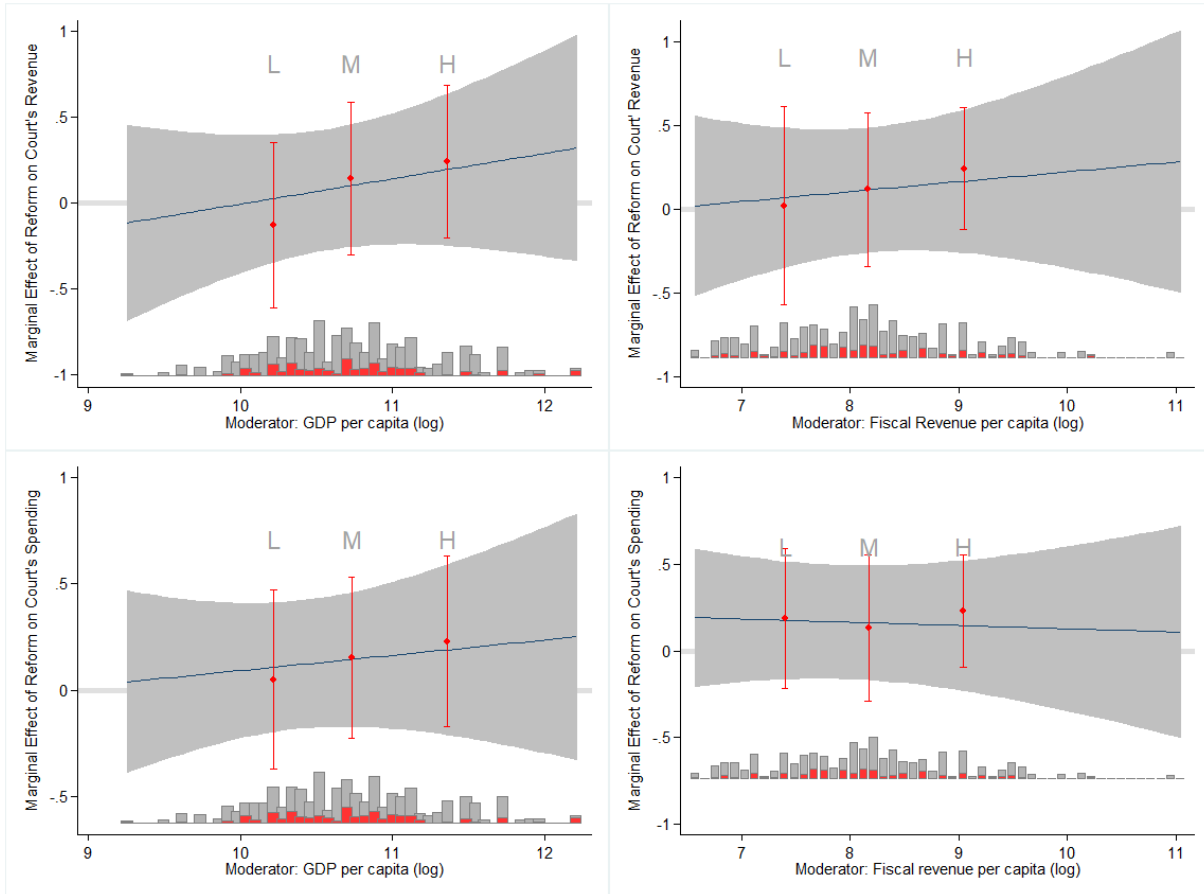


Figure E1: The Effect of Reform Moderated by Local Economic Development

Notes: This figure serves as an additional check for the results in Appendix Table E4. We plot the effects for low, medium, and high level of GDP and fiscal revenue, both in 2014, based on tertiles using the binning estimator (red dot). We show the distribution of data below estimates, where red bars represent observations in the treatment (reformed) group and gray bars represent observations in the control (unreformed) group. These plots confirm the results of Appendix Table E4.

Table E5: The Fiscal Dependence of Courts on the Prefectural Government

	Fiscal dependence		
	(1)	(2)	(3)
Recentralization	-1.011*** (0.083)	-1.439*** (0.351)	-1.428*** (0.357)
Prefecture FE		✓	✓
Province-Year FE		✓	✓
Prefecture Controls			✓
Dependent Variable Mean	0.968	0.968	0.970
Observations	607	607	605

Notes: Fiscal dependence is measured as the share of funding provided by prefectural government divided by the court expenditure. In other words, the higher the fiscal dependence, the more dependent a court is on a prefectural government's funding. This table shows that the reform substantially reduces a court's dependence on the prefectural government. Prefecture controls include the population size, GDP per capita, GDP growth rate, fiscal revenue per capita, fiscal expenditure per capita, number of listed firms, value-added tax size, fixed asset investment, unemployment rate, and the share of secondary and tertiary sector in the GDP. Standard errors clustered at the prefecture level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

Table E6: Judicial Recentralization and the Provincial Expenditures on Other Public Services

	(1)	(2)	(3)	(4)	(5)	(6)
	Education (<i>log</i>)	Research & Development (<i>log</i>)	Culture (<i>log</i>)	Environmental Protection (<i>log</i>)	People's Congress (<i>log</i>)	Police (<i>log</i>)
Recentralization	0.003 (0.009)	0.009 (0.048)	0.052 (0.35)	-0.040 (0.34)	0.017 (0.41)	0.022 (0.34)
Province FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Province Controls	✓	✓	✓	✓	✓	✓
Dependent Variable Mean	6.845	4.626	4.618	5.062	9.162	11.69
Num. of Provinces	24	24	24	24	24	23
Observations	643	643	643	643	609	588

Notes: We investigate whether judicial recentralization influences the size of provincial expenditure on education (column (1)), research & development (column (2)), culture (column (3)), environmental protection (column (4)), people's congress (column (5)), and police (column (6)). Province controls include the population size, GDP per capita, GDP growth rate, fiscal revenue per capita, fiscal expenditure per capita, number of listed firms, and unemployment rate. Standard errors clustered at the province level are reported in parentheses. The significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. FE=Fixed Effects.

References for the Appendix

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