Court Capture, Local Protectionism, and Economic Integration: Evidence from China

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Abstract

This study investigates the judicial and economic impacts of a major reform in China that centralized judicial control by transferring financial and personnel authority over local courts from local to provincial governments. Leveraging novel administrative datasets and exploiting the staggered implementation of the reform, we find evidence of significant reductions in judicial local protectionism, with local defendants' win rates against non-local plaintiffs decreasing by 7.3%. The reform also enhanced judicial quality, evidenced by improved judicial reasoning, fewer appeals, and increased use of evidence examination. Over time, these changes encouraged smaller non-local firms to litigate against larger local firms and attracted non-local investment to reformed regions, fostering greater economic integration and a potential increase in societal economic surplus of up to 1.9% of GDP. However, key limitations remain, including persistent favoritism toward firms with higher-level political connections, inter-provincial protectionism, and concerns about centralized judicial control, such as the promotion of less qualified judges. These findings highlight the potential of judicial centralization to mitigate entrenched biases while revealing its limits in achieving full judicial independence.

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1 Introduction

An impartial legal system that enforces contracts and property rights under the rule of law has long been perceived as a fundamental building block of economic prosperity (Smith, 1937; North, 1986; Besley and Persson, 2011; Acemoglu and Robinson, 2012). Judicial independence is a cornerstone of this system, enabling courts to provide fair trials, resolve disputes, and uphold competitive market environments free from undue political influence (Hamilton, 1788;Hayek, 1960;Buchanan, 1974). However, achieving full judicial independence remains elusive in many developing economies and especially in authoritarian regimes, where political capture of the judiciary is ubiquitous (Glaeser and Shleifer, 2002; La Porta et al., 2008).

In the absence of a sufficiently independent legal system, local governments frequently interfere in judicial decisions to favor local firms in inter-regional commercial disputes, particularly those with political connections (Cooter, 1996; Hay and Shleifer, 1998). Such judicial local protectionism obstructs fair market competition, impedes the formation of profitable business relations, and deters economic integration between regions. Despite extensive studies showing cross-country correlations between court capture and adverse economic outcomes (Djankov et al., 2003; La Porta et al., 2004; La Porta et al., 2008), causal evidence on this topic remains limited. It is empirically challenging to isolate the role of the judicial system, as its functioning often evolves alongside other political and economic institutions, and changes to the judiciary frequently coincide with broader policy shifts.

This paper examines these issues in the context of China, a country where political centralization is juxtaposed with economic decentralization, creating a persistent challenge of judicial local protectionism. Local officials frequently prioritize local firms over non-local competitors, motivated by incentives to safeguard the local tax base or extract personal rents.[1] Such local protectionism has persisted as local governments wield control over the financial and personnel decisions of local courts, allowing them to shape judicial outcomes to favor local interests. Despite the typically optimistic tone of official statements, the Supreme People's Court has acknowledged these challenges, with a former president admitting that "some local officials viewed laws as tools to serve parochial interests,"[2] showing that the issue is recognized not only by the business sector but also within the judiciary.

In 2014, China implemented a significant judicial reform aimed at centralizing financial and personnel control of lower-level courts under provincial governments. This reform was designed to miti-

[1] See: Gong (2004); Xu (2011); Li (2012); Ng and He (2017); Wang (2018).
[2] See Zheng (1994), p.472.

gate interference by local governments in judicial decision-making, thereby fostering cross-regional integration and enhancing political legitimacy (Zhou, 2017; Chen, 2018; Supreme People's Court, 2019). In 2014, China implemented a significant judicial reform aimed at centralizing financial and personnel control of lower-level courts under provincial governments. This reform was designed to mitigate interference by local governments in judicial decision-making, thereby fostering cross-regional integration and enhancing political legitimacy (Ginsburg and Moustafa, 2008). This reform was rolled out in a staggered manner, offering a unique opportunity to examine the effects of partially mitigating judicial capture in an otherwise authoritarian party-state that lacks full judicial independence. It is worth noting that, while this reform marks a significant departure from past practices, its implications remain mixed: by consolidating judicial authority under provincial governments, the reform represents a shift toward "rule by law," where the law serves as a tool for those in power to govern, rather than a transition toward full judicial independence aligned with "rule of law" (Zhang and Ginsburg, 2019; Wang, 2021). While this centralization curtails local governments' interference, it simultaneously expands the scope of influence for higher-level authorities, underscoring the potential trade-offs inherent in such reforms. This raises significant questions about how partial reforms within such systems can affect judicial and economic outcomes.

Leveraging comprehensive administrative data on over 133 million court cases and 75 million business registrations, this paper investigates the judicial and economic consequences of centralizing control over local judiciaries, taking advantage of the rich variation introduced by the decade-long, staggered roll-out of the judicial reform. We examine how the reform has mitigated issues of judicial local protectionism and fostered economic integration *within* provinces. At the same time, we evaluate the reform's limitations, including the continued protection of firms with political ties to higher levels of government, the persistence of inter-provincial protectionism, and potential challenges stemming from reduced access to localized information.

First, we find that the judicial centralization reform has overall led to a significant reduction in local protectionism in court decisions. Following the reform, the win rate of local defendants against external plaintiffs dropped by 7.3%. This effect is especially large for firms that are politically connected to local governments (15.8%), consistent with these firms receiving more local protection pre-reform. Importantly, the baseline reduction reflects two forces: changes in judge incentives and changes in the composition of cases. To isolate the effect of changes in judge incentives, we focus on cases filed before the reform but adjudicated at different times—some just before the reform and others just after. This comparison allows us to examine outcomes for otherwise similar cases and reveals that the same judge

rules significantly less pro-locally after the reform, indicating that the reform directly induces changes in judge behavior. These changes in judge incentives in turn also influence firms' litigation decisions, with smaller non-local plaintiffs, who tend to have lower win rates, becoming more likely to sue larger local firms post-reform. This compositional shift biases the baseline estimate downward, making the observed overall reduction in local protectionism appear smaller than the actual effect driven solely by changes in judge incentives.

Across various measures commonly used in the legal literature, we find consistent evidence that the judicial reform has led to consistent improvements in the judicial decision making process. First, appeal rates decreased for external plaintiffs while remaining unchanged for local defendants. Second, judges became more likely to approve requests for evidence examination and expert witness testimony. Third, court verdicts provided more detailed judicial reasoning in judgment files (with higher word counts). Fourth, judges became less likely to rely on discretionary codes in their reasoning. Collectively, these findings contradict the alternative interpretation that the reform merely allowed judges to "sell" decisions equally to both sides, instead demonstrating improvements in judicial quality driven by the reform.

Despite reducing judicial protectionism and improving judicial quality, the reform neither fully separates the judicial and executive branches of government nor establishes effective checks and balances on top-level political leaders. Instead, it centralizes personnel and financial control over local courts within provincial governments. Consistent with the typical practices of authoritarian regimes in expanding judicial power (Ginsburg and Moustafa, 2008), this judicial centralization is better understood as a move toward *"rule by law"* rather than a transition toward *"rule of law."*

Motivated by the extensive literature on the trade-offs between centralized and decentralized governance, we empirically examine the shortcomings of the reform. We have four key findings. First, firms connected to higher-level (provincial and central) governments remain largely shielded from the reform's impact on protectionism and maintain their edge in inter-regional commercial lawsuits. Second, local officials with professional ties to provincial governments continue to wield some influence over local courts. Third, inter-provincial commercial lawsuits still exhibit signs of local protectionism post-reform. Fourth, while judges promoted by provincial governments post-reform tend to exhibit less local bias, they demonstrate lower ability compared to those promoted by lower-level governments before the reform, suggesting reduced access to local information. Taken together, these findings suggest that, while the reform has substantially reduced judicial local protectionism, it remains constrained by significant limitations when compared to the ideal of full judicial independence.

In the second part of the paper we evaluate the reform's net impact on economic integration in light of its mixed judicial effects. To do so, we create a novel panel dataset of inter-regional investment networks in China by tracking yearly changes in firms' shareholding structures from the universe of business registration records with 75 million registered firms. We find that, on net, inward investments by non-local businesses increase by 11.2% following the judicial reform. This effect is stronger for investments made by non-local firms that have experienced lawsuits post-reform, suggesting that firms' awareness of the reform's effectiveness drives their responses. The increase in inward investments is also more pronounced in regions that adopted the reform later. We further show that the investment response stems primarily from non-local firms entering local markets directly, accompanied by a *decline* in the formation of joint ventures with local trade partners. Our finding echoes Coase (1937): as the judicial reform lowers the transaction costs in dealing with local firms, non-local firms are incentivized to operate independently in local markets rather than merging with local partners. Using a simple model à la Melitz (2003), we estimate that the increased economic integration from this reform could raise China's economic surplus by 1.9% of GDP.

This study relates to three strands of literature. First, it contributes to the long-standing discussion on law and economic development. While substantial theoretical work and cross-country literature have highlighted the relationship between judicial independence and economic prosperity (Smith, 1937; Hamilton, 1788; Hayek, 1960; Buchanan, 1974; North, 1986; Glaeser and Shleifer, 2002; La Porta et al., 2004, 2008; Besley and Persson, 2011; Acemoglu and Robinson, 2012), there has been surprisingly little rigorous evidence on how alleviating court capture affects judicial outcomes and economic activities.[3] Notable exceptions are Mehmood (2022a,b), which show that changes in the selection procedure of judges in Pakistan influenced pro-government rulings and real estate investments. Our contribution is to exploit exogenous variation in judges' incentives (rather than their selection) and demonstrate that reducing local court capture in China's civil law system can, at least partially, mitigate local protectionism, thus fostering cross-regional economic integration.[4]

Our study also sheds new light on the political-economic roles of legal institutions in authoritarian regimes. As summarized by Ginsburg (2000); Ginsburg and Moustafa (2008); Moustafa (2014),

[3] In contrast, a growing empirical literature explores the economic impacts of courts' speed in processing cases (Chemin, 2009; Visaria, 2009; Ponticelli and Alencar, 2016; Boehm and Oberfield, 2020; Rao, 2021; Li and Ponticelli, 2022).

[4] Our systematic analysis of the universe of Chinese civil lawsuits also adds to several smaller studies on the legal impacts of the judicial organizational reform in China, including field interviews of judges (Wang, 2021), analyses of around 1,000 cases involving publicly listed firms (Zhao and Zhang, 2022; Lei and Li, 2022), and around 4,000 administrative litigation cases (Zhou et al., 2021). More broadly, this paper contributes to the wider discussion on judicial biases in China (Gong, 2004; Li, 2012; Wang, 2013; Ng and He, 2017; Wang, 2018).

the conventional wisdom holds that courts in such regimes are little more than window dressing for dictators; however, recent studies suggest that more nuance may be warranted. Specifically, when explaining China's growth in recent decades, judiciaries are often not viewed as a key contributor. As politicians compete for career advancement based on local economic growth (Montinola et al., 1995; Qian and Weingast, 1997; Li and Zhou, 2005; Xu, 2011), they exercise local protectionism (Zhou, 2004, 2014), and routinely help important local firms bypass the law (Bai et al., 2020a).[5] Our findings support the observations made by Zhang and Ginsburg (2019) and show that, unlike the institutional forces of previous decades, China's judicial centralization since 2014, despite its various limitations, has nonetheless empowered local legal institutions to play meaningful roles in economic development within an increasingly authoritarian regime that otherwise has limited judicial independence.

Finally, our paper, by studying a reform that effectively centralized control over local judiciaries, also speaks to the long-standing debate on the political economy of centralization vs. decentralization (World Bank, 2003; Bardhan, 2002; Mookherjee, 2015). On one hand, our results, which show that centralized control of local courts significantly reduces local protectionism, exemplify the classic "race to the bottom" issue under decentralization (Blanchard and Shleifer, 2001; Sonin, 2003; Cai and Treisman, 2004; Ponomareva and Zhuravskaya, 2004; Slinko et al., 2005; Young, 2000). On the other hand, our finding that provincial governments promote lower-quality judges, compared to lower-level governments, provides empirical support for the theoretical literature highlighting the importance of decentralized information in governance (Cremer et al., 1994; Seabright, 1996; Besley and Case, 1995).

The remainder of this paper proceeds as follows. Section 2 describes the institutional background. Section 3 discusses our data. Section 4 investigates the impacts of the reform on judicial local protectionism. Section 5 examines various potential limitations of the reform. Section 6 presents the economic impacts of the reform and quantifies the welfare implications. Section 7 concludes.

2 Institutional Background

In this section, we describe China's judicial system, provide background information on judicial local protectionism in China, and detail the judicial organizational reform.

[5] In fact, as the country has grown more authoritarian over the past decade, it is believed by some that China has also become more resistant to the rule of law (Minzner, 2011, 2015, 2018; Ringen, 2016; Zhang, 2016; Shirk, 2018).

2.1 China's Judicial System

According to the Organic Law of the People's Courts that went into effect in 1980, China has a four-level court system, which we illustrate in Supplementary Figure S.1. At the national level, there is the Supreme People's Court; at the provincial level, there are 32 High People's Courts; at the prefectural level, there are 404 Intermediate People's Courts; and at the county/district level, there are 3,111 Basic People's Courts. In this paper, we refer to prefectural, county, and district level courts collectively as *local* courts.

When one firm brings a lawsuit against another firm, the trial is heard in the defendant's jurisdiction by default. Therefore, inter-regional commercial cases generally consist of external plaintiffs and local defendants. For the majority of civil lawsuits (97%), the first hearing happens at the level of the county basic court.[6] For a small share of civil lawsuits seeking exceptionally large damages, the first hearing happens at the level of the prefectural intermediate court or even the provincial high court. After the first instance, one appeal can be made to the corresponding upper-level court, up to the Supreme People's Court.[7]

China's legal system is largely a civil law system, with some features of the Great Qing Code and various other historical systems. Following civil law traditions, there are neither juries nor established, legally-binding precedents. Instead, judges play the dominant role in trials; they act as chief investigators, establish facts, apply the provisions of the applicable code, and make the final rulings.

Given these features, judicial quality in China relies heavily on the incentives of the judges in local courts, and judicial independence is compromised if local judges are captured by the local governments. Figure 1a illustrates the incentive structure of China's local courts prior to 2014. Each local court receives professional guidance from the upper-level courts, such as suggestions on interpretations of new codes and guidelines on sentencing rules, which are suggestive and non-binding for the local courts. In contrast, *de facto* control over the local courts is held by the corresponding levels of local governments. Specifically, for each local court, its budget, which includes salaries and bonuses for the judges and court clerks and covers other court operational costs, needs to be approved by the corresponding local government. Similarly, the local government (the local People's Congress, in particular) has the final say in the local court's personnel decisions, such as promoting a judge to a higher rank or appointing a new president of the court.

[6] The percentage number is calculated using the raw data provided by China Judgments Online.

[7] For example, if a civil case is handled by a county basic court, the plaintiff or the defendant has the right to appeal to the corresponding prefectural intermediate court.

Not surprisingly, when a judge's income and career development are both determined by the local government officials, it is difficult for the judge to remain shielded from local political influences when making judicial decisions.

2.2 Economic Decentralization and Local Protectionism in China

China's governance juxtaposes political centralization with economic decentralization, where local officials make the majority of economic decisions and compete for promotion opportunities. As pointed out by previous studies (Li and Zhou, 2005; Xu, 2011), this institutional arrangement is vulnerable to local protectionism, as local officials have strong incentives to favor local firms over external ones. This could be driven by both the pursuit of career advancement and the extraction of personal rents.

Local government officials can exercise local protectionism by influencing the local courts. In principle, courts are supposed to be independent organizations that can fairly resolve conflicts between local and non-local firms. In reality, since local courts are highly reliant on local officials for personnel and financial decisions, they are frequently captured by local governments. A common scenario is that the local governments expect judges to favor local defendants against non-local plaintiffs, especially when the local defendants are economically significant or politically connected. Anecdotally, it is widely acknowledged that many large and influential firms are simply "undefeatable" in their home courts; this is the origin of many Chinese internet memes ridiculing the futility of going against these firms in court.[8]

This type of judicial local protectionism has long been widely recognized as a fundamental problem of China's judicial system, not only by legal scholars, but also by many judges, including the president of the Supreme People's Court (Zheng, 1994). Perhaps most telling is a national survey of local judges conducted by the Supreme People's Court immediately before the judicial organizational reform in 2014. The survey found that 68% of the local judge respondents listed local protectionism as the major reason for biased rulings.[9]. These qualitative observations are also corroborated by quantitative studies documenting that connected local firms tend to obtain systematically more favorable court rulings (Ang and Jia, 2014; Lu et al., 2015; Xu, 2020; Chen and Xu, 2021).

[8] For example, it is reported that Huawei has never lost a case in its home court in Longgang, Tencent has an 88% win rate in its home court in Nanshan, and ByteDance has a 98% win rate in its home court in Haidian. Netizens came up with nicknames for these firms describing their home court advantages. Source: `https://bbs.mysipo.com/thread-1109742-1-1.html`

[9] See: `https://www.cecc.gov/judicial-independence-in-the-prc`

2.3 The Judicial Organizational Reform

Because of the severity of judicial local protectionism, in November 2013, the Central Committee of the Communist Party of China published a document entitled "Decision on Several Major Problems regarding Comprehensively Deepening the Reform," which explicitly stated that China should "reform its judicial organizational structure, push for unified financial and personnel management of local courts at the provincial level, try to make the local judiciaries independent from the local governments, and ensure the proper enforcement of the rule of law."[10] Noteworthy is that, "improving local business environment" is prominently stated as a main objective of the reform, which is in turn cited frequently in local courts' reports and discussions about the reform's effectiveness.[11]

Following the central government's guidelines, in 2014, the Supreme People's Court formally launched the judicial organizational reform, which aims to systematically alleviate court capture by decoupling local courts from local governments.As shown in Figure 1b, the reform deprives the county and prefectural governments of their financial and personnel controls over the corresponding local courts; instead, all such controls are consolidated to the provincial governments.[12] After this reform, county and prefectural governments have seen a reduction in their formal influence over local courts, limiting the scope for local court capture. The Supreme People's Court often highlights this reform as a significant step in China's legal development, a view supported by a range of qualitative studies from legal scholars (Zhou, 2017; Chen, 2018; Supreme People's Court, 2019; Zhang and Ginsburg, 2019).

In addition to curbing local protectionism, the reform is widely perceived as an effort to establish the Party leadership as a champion of legality, thereby strengthening political legitimacy.[13] The economic incentives to foster cross-regional integration and the political incentives to promote legitimacy align closely with the general motivations behind authoritarian regimes' decisions to expand judicial power, as summarized by Ginsburg and Moustafa (2008).[14] An interesting question arises as to why the reform was implemented in 2014 rather than earlier, given that widespread local protec-

[10] Source: `http://www.gov.cn/jrzg/2013-11/15/content_2528179.htm`

[11] See: `https://www.gdcourts.gov.cn/xwzx/yw/content/post_1045183.html`

[12] It is substantially less costly for the central government to ensure compliance of the 31 provincial governments than the over 2,800 county-level governments. Such administrative feasibility considerations could be why the reform did not consolidate judicial controls all the way to the central government (Zhang and Ginsburg, 2019).

[13] Relatedly, Xi's first major policy agenda was the anti-corruption campaign, which has earned him widespread support within China.

[14] The other goals mentioned are: (1) establishing social control and sidelining political opponents, (2) strengthening administrative compliance within the state's own bureaucratic machinery and solving coordination problems among competing factions within the regime, and (3) implementing controversial policies to create political distance from the regime's core elements.

tionism and its hindrance to economic integration had been recognized since the 1990s. As explained in Zhang and Ginsburg (2019), the timing of the reform is tied to China's underlying political trends. During the first three decades of China's economic reform, the country largely followed a decentralized economic development model. A judicial organizational reform that effectively weakened local governments would likely have faced strong local opposition. However, since Xi Jinping took office in 2013, political power has been significantly centralized. This centralization reduced the scope of local opposition while also creating an incentive for the central government to address local abuses of political power.

It is important to note, however, that consolidating judicial control to the provincial level does not fully separate the judicial and executive branches of government, especially at the highest levels. As such, the reform is better understood as a shift toward partial and local judicial independence in the sense of *rule by law*, rather than *rule of law*—meaning the law is used by the top leadership as a tool of governance, rather than standing above the government. Consequently, both provincial and central governments may continue to exert influence over the judicial process.

For these reasons, the reform may suffer from several important shortcomings. First, with more judicial control consolidated to the provincial level, the degree of favoritism towards provincially and centrally connected firms could remain substantial or even be heightened. Second, local politicians that are connected to the provincial governments might be able to lobby for continued protection for their local firms. Third, even though provincial governments are more detached from local adjudication than lower-level governments, they might still exercise protectionism when the plaintiff is from outside the province. Fourth, provincial governments might be less informed about local judges' ability, thereby hampering meritocracy in judge promotion. We will investigate these shortcomings explicitly in Section 5.

2.4 Staggered Roll-out of the Reform

The judicial organizational reform has been rolling out at the prefectural level in a staggered fashion since 2014. Specifically, the roll-out schedule was explicitly designed by the Supreme People's Court (SPC) to cover economically representative regions in each year, so that the knowledge generated by early adopters could be generalized to the rest of the country.[15] Such policy experimentation is a feature commonly seen in many of China's high-stakes policy reforms, and is a well-established institutional

[15] For more detailed discussions on the deliberations to conduct representative policy experimentation, see the press conference by the judicial reform committee: `http://www.gov.cn/xinwen/2014-06/15/content_2701248.htm`

arrangement to help reduce policy uncertainty and inform the policymakers on the optimal protocols for policy implementation (Wang and Yang, 2021).

Figure 2a illustrates the detailed roll-out schedule of the judicial organizational reform. One hundred fifty-two local courts were selected by the Supreme People's Court as pilot sites in 2014, before the reform was gradually implemented in the rest of the country. As the figure shows, in each year since 2015, the roll-out plan includes a geographically representative sample of provinces and prefectures simultaneously covering the East, Middle, and West of the country. By the end of 2021, more than 70% of China's 3500 local courts had undergone the reform, and the SPC plans to cover the entire country by the end of 2027. The large scale of the reform, combined with its unusually long roll-out schedule, provides rich variation that can be exploited for causally identifying the reform's impacts.

Our baseline econometric specification is a Difference-in-Differences (DiD) approach exploiting the staggered reform roll-out. The validity of this design hinges on the assumption that the timing of reform adoption is orthogonal to the underlying trends in the outcome variables, including local judicial outcomes and inter-regional investments. While other political and economic factors (such as local fiscal conditions and central-local patronage relationships) may correlate with the sequence of the reform roll-out, as long as these factors did not change discontinuously around the time of the reform, they are unlikely to confound our DiD estimates. To assess the validity of our DiD assumption, in all subsequent analyses, we formally test for pre-reform trends in all outcomes using event study models.

Furthermore, to better understand the variation in the timing of local implementation of the reform, we estimate various versions of hazard models that explicitly investigate what local characteristics can predict the sequence of reform roll-out. We construct three sets of pre-reform local characteristics, covering judicial conditions (number of inter-regional commercial lawsuits, local defendants' win rates in inter-regional commercial lawsuits), economic conditions (GDP per capita, FDI, local government's budget, non-agricultural GDP share), and political conditions (local leaders' connections to their provincial government). As shown in Appendix Table A.1, across all specifications, none of the constructed indicators can systematically predict the roll-out schedule of the reform. We take this as reassuring evidence that alleviates potential concerns regarding the selection issues that might interfere with our baseline DiD approach.

2.5 Other Related Reforms During the Same Period

During the roll-out of the judicial organizational reform, several other changes also took place in China's judicial system, including those that attempt to reduce the cost of filing lawsuits, hold judges permanently accountable for their judicial decisions, introduce general guidelines and AI assistance for sentencing decisions, etc. These other judicial changes are generally viewed as less significant in comparison to the high-stakes organizational reform; more importantly, most of these other changes were implemented nationwide simultaneously and all were completed within three years. By contrast, the judicial organizational reform took significantly longer to roll out.[16] It is therefore unlikely for the impacts of the judicial organizational reform to be confounded by the existence of these other contemporaneous judicial changes. Nevertheless, we collected detailed information on the roll-out schedules for every notable judicial changes since 2014 that has any sub-national variation in the timing of adoption.[17] In addition, we also measured the rotation of central inspection teams in local governments, commonly used as a proxy for the anti-corruption campaign happening in China during this period.

As we discuss in Section 4, controlling for these other reform policies has no detectable impact on our main findings.

3 Data

In this paper, we compile, to the best of our knowledge, the most comprehensive dataset to date on commercial lawsuits in China. We combine this with novel data on China's inter-regional business investment network. Our two main data sources cover the universes of court judgment files and business registration records in China, which are explained in Sections 3.1 and 3.2, respectively. We complement these two datasets with several additional sources of data, which we discuss in Section 3.3. In Section 3.4, we also present some simple descriptive statistics and graphical patterns.

[16] Anecdotally, many observers believe that the judicial organizational reform was rolled out in a staggered fashion precisely because of its exceptionally high stakes, which is consistent with the fact that the Chinese government frequently relies on gradual experimentation to reduce uncertainty for the most important policies (Wang and Yang, 2021).

[17] Specifically, this includes the judge quota reform, which sidelined low-ability judges nationwide; the trans-regional jurisdiction reform, which assigned away courts for trials with government entities as defendants; and the SPC circuit court reform, which established branches of the SPC in certain provinces.

3.1 Universe of Court Verdicts

In 2013, as part of its efforts to improve judicial transparency and provide (non-binding) precedents for judges, the Supreme People's Court established an official website called *China Judgment Online* (CJO) and required local courts at all levels to publicize both contemporary and historical verdicts on this website. While there is a backlog in digitizing and disclosing historical verdicts, local courts are obligated to disclose all contemporary judgment files within seven days of trial completion, with exemptions granted for special cases such as those involving national security or juvenile delinquency. Screenshots of the CJO website and a sample court judgment file are provided in Supplementary Figure S.2.

We collected the universe of court verdicts in China between 2014 and 2021 from CJO, through continuous, daily scraping of the website since 2018. The data include 133 million judgment files, from which we identified more than 6 million civil lawsuits between firms.[18] These firm-to-firm civil lawsuits are the focus of this paper. For each judgment file, we extracted the following information: court in charge, trial and ruling dates, name of the judge and other court clerks, name of the plaintiff, name of the defendant, basic facts about the case, summary of trial process, claims made by plaintiff, whether the plaintiff's claims were supported by the court, obligations to pay court fees, and judicial reasoning provided by the judge.

A key variable for our analysis is the win rate of each party in a lawsuit. In civil practice, court fees are supposed to be paid by the losing party, so how court fees are shared between the plaintiff and the defendant reflects the extent to which each party wins or loses in a lawsuit, from the perspective of the court.[19] Therefore, we follow the legal literature to extract detailed information on each party's obligation to pay court fees, and measure the win rate of each side using the share of the opposing side's obligation to pay court fees: $WinRate_j = \frac{CourtFee_i}{CourtFee_i + CourtFee_j}$. As reported in Table A.2, the average win rate of local defendants in cases filed by external plaintiffs is 0.45, with a standard deviation of 0.46.

For robustness, we construct two alternative measures of judicial outcomes. First, as shown in Supplementary Figure S.3, the distribution of $WinRate$ is bimodal – in most cases (74%), the court either completely supports or fully rejects all claims made by the plaintiff. Motivated by this pattern,

[18] We keep all court verdicts for cases that were tried between 2014 and 2021 and released by the *China Judgments Online* before August, 2022.

[19] For instance, a plaintiff that wins completely would be ordered to pay 0% of the court fees, whereas an even split of the fees implies that each side won 50%.

we keep the subsample of cases with such "clear-cut outcomes" (i.e., complete wins or complete loses), and measure judicial outcomes using a discrete variable. Second, given that "monetary damage" is usually the most important claim in commercial lawsuits, we calculate the total amount of monetary value that local defendants are obligated to pay to external plaintiffs based on court orders, and use it as another proxy for judicial outcomes.[20] As will be discussed in Section 4.1, our baseline findings are strengthened with these alternative outcome measures.

In addition to measuring judicial outcomes, we also follow the legal literature and measure the "quality" of judicial decisions in four different ways. The first is the appeal rate after the first verdict, for both plaintiff and defendant;[21] The second is the court approval rate of requests to examine evidence or invite an expert witness, for both plaintiff and defendant;[22] The third is the richness of judicial reasoning in the judgment file (measured by word count);[23] The fourth is the frequency of citing discretionary codes in the judgment file.[24]

The Quality and Reliability of CJO Data. The judgment files in the CJO data are generally believed to be highly reliable, as any manipulation of such publicized administrative records would require three-way collusion among the plaintiff, the defendant, and the local court. In fact, the information reflected in commercial court judgment files is widely used in the credit rating process by all the major commercial banks. Furthermore, in recent years China has adopted an open justice system whereby civil cases are live-streamed when requested by plaintiffs Cai et al. (2022). As millions of trial recording videos are publicly accessible on the internet, it has become even more unlikely that the court judgment files could be systematically manipulated.

A potential concern of the CJO data is local courts' incomplete disclosure, whereby some judgment files are missing from the public records. This could happen for two reasons: (1) in the early years of the CJO, local courts may not have publicized all cases on the website (Ahl et al., 2019; Liebman et al., 2020); and (2) in 2021, it was reported that the CJO deleted a batch of "politically sensitive" criminal

[20] Another possible measure of win rate is "how many of plaintiffs' claims were supported by the court," but it is less than ideal for at least two reasons. First, some claims are a lot more important than others, so simply counting the number of claims supported could be misleading when the court supports unimportant claims while dismissing important ones. Second, different cases could have very different numbers of claims (i.e., some firms file many unimportant claims while others don't), and the count of claims would thus not be comparable across different cases.

[21] A lower appeal rate is commonly used as a proxy for higher judicial quality (Baye and Wright, 2011).

[22] Allowing forensic evidence examination is associated with more fair trials (Edmond and Roberts, 2011).

[23] Longer judicial reasoning has been documented to correlate with decision quality (Liu, 2018).

[24] A verdict is potentially more distorted if the judge imposes excessive discretion in his judicial reasoning (Liu and Li, 2019).

cases from the website.[25]

However, neither issue is likely to affect the analysis in this paper, for several reasons. First, the bulk of the missing cases documented in the literature were actually backlogs due to local courts' capacity constraints in the early years, and these files were added to the CJO later on;[26] in fact, during the early years after CJO became online in 2013, many local courts throughout the country are incentivized to digitize the backlog of cases, with the digitization rate being an important KPI for local judges' promotion.[27] Second, we have been scraping the CJO website daily since 2018 for any updates, and all cases that were deleted after posting—including the batch of criminal cases deleted in 2021—are captured by our data. Third, we cross-validate our data with the national-level official statistics in the China Statistical Yearbooks from 2015 to 2021, which were published by the National Bureau of Statistics. As shown in Supplementary Figure S.4, our data have an average missing rate of 21% in first trial civil lawsuits; the missing rate fell below 10% in more recent years as local courts gained more technological capacity to digitize case files.[28] A substantial share of these missing files belong to the exempted cases relevant for national security or involving juveniles. These missing rates are also consistent with our alternative calculations using the gaps in the court cases' reference IDs, which are assigned uniquely to each case and follow consecutive numbers for all cases. Fourth and finally, to further alleviate the concern of endogenous missing data, we also directly test whether the roll-out of the judicial organizational reform was correlated with file missing rates, and find no such evidence (Appendix Table A.3).

As widely recognized in China, the judicial files publicly available through CJO have catalyzed several high-profile social movements concerning issues deemed "politically sensitive" by the central government. Consequently, and regrettably, since 2024, the CJO has been replaced by an internal database for judicial employees that is no longer available to the public. This regression in judicial data transparency does not affect the analysis in this paper.

[25] As reported by various media outlets, this issue is mainly concentrated in criminal cases, especially for cases related to state security. Source: `https://www.rfa.org/mandarin/yataibaodao/renquanfazhi/ql-07162021074351.html`

[26] For example, Liebman et al. (2020) find that 45% of documents were missing in 2014. Using more complete data scraped in 2022, we find that 60% of those missing cases have already been added to the website and are therefore included in our sample.

[27] Source:`https://www.chinacourt.org/article/detail/2014/08/id/1426709.shtml`

[28] We can only calculate the missing rates for first trials of civil lawsuits (as opposed to retrial after appeals) because the China Statistical Yearbook only reports the number of first trials of civil cases. Nevertheless, it's unlikely that omitting appealed cases would substantially affect the missing rate, since first trial cases account for over 90% of all the verdicts.

3.2 Universe of Business Registrations

Our firm-level data is from *Tianyancha*, a company that offers access to the universe of China's business registration records. These records are licensed by the National Enterprise Credit Information Publicity System, and maintained by the State Administration for Industry and Commerce (SAIC). Supplementary Figure S.5 shows a screenshot of *Tianyancha*'s webpage, from which we scraped business registration information. The business registration data cover every firm that was ever registered in China in the past four decades; as of the end of 2021, the data includes over 75 million entries (including branches of firms). For each registered firm, we have detailed information on its location, ownership type, legal representatives, shareholders and their holdings, executives, value of registered capital, industry code, year of establishment, and all historical changes/updates to any of the above items. These data have been used by several recent papers that examine firms' entry decisions in China (Allen et al., 2019; Bai et al., 2020b, 2021; Shi et al., 2021).

Two main variables are constructed using the business registration data. First, based on the location information, we are able to define "local" vs. "non-local" firms, and thereby identify all commercial lawsuits with non-local plaintiffs and local defendants. These lawsuits form our sample to analyze local protectionism.

Second, based on the shareholding structure at the time of each firm's registration, as well as subsequent changes, we identify investments made in each firm in each year from either business or individual investors. For business investors, we use each firm's location information to define whether it is "local" or "non-local." For individual investors, we first use the business registration data to trace each investor's shareholdings in other firms, and then assign each individual investor to the location that accounts for the largest share of his shareholdings. We then use the location information to label individual investors as "local" or "non-local." This procedure enables us to construct a county-to-county equity investment network in China over the past four decades.

We use both the number of external investments and their total amounts to measure cross-regional investments. Using the former as an outcome variable mitigates the potential measurement error concerns regarding the latter. In principle, registered capital should capture the total capital injected by the investors at the time of registering a firm; however, during our sample period, firms in China were not legally required to provide proof of the full amount of registered capital at the time of registration.

While firms have incentives not to completely misrepresent their registered capital,[29] this variable may contain measurement error, as there are low legal and financial stakes for inaccurate accounting (Shi et al., 2021).

We use the number of external investments as a main outcome variable, because of its accuracy, for measuring cross-region integration. We continue to use the total investment amount as a another main outcome because there is no obvious reason that the measurement error would be systematically correlated with the roll-out schedule of the judicial organizational reform. Importantly, as explained in Section 6.4, how the number of external investments responds to the judicial reform is a model-based sufficient statistic for the reform's overall economic impact; hence, our main welfare conclusion is unaffected by this potential measurement issue.

3.3 Other Complementary Data Sources

In addition to the two main data sources discussed above, we make use of three additional complementary datasets.

First, we hand-collected information on the roll-out schedule of the judicial organizational reform from eight volumes of the "Yearbook of Judicial Reforms in China" between 2013 and 2020. The yearbooks were published by the Supreme People's Court every year, summarizing the design, implementation, and effectiveness of judicial reforms in China. Each yearbook contains a chapter for each province, which provides a detailed timetable of reform roll-out. The roll-out information was further corroborated using information from local courts' websites and local governments' fiscal expenditure records—as shown in Appendix Table A.4, the major financial sources for local courts changed from local governments pre-reform to provincial governments post-reform.[30]

Second, to proxy for firms' connections to various levels of government, we scraped the Chinese Government Procurement Database, a website maintained by the Ministry of Finance, and collected over 3.5 million procurement contracts issued by all levels of Chinese government between 2013 and 2021. We define firms that had contracts with a certain government as "connected" to that government, and those without contracts as "unconnected."

Third, to verify whether the judicial reform's impact on court rulings translates into impact on

[29] There is limited incentive to under-report a firm's registered capital, as this information may be used by potential partners as a reference to the firm's overall economic capacity; in some industries, there are also minimum requirements for registered capital for entry. Firms also have limited incentives to over-report, as registered capital is also the legal amount for which shareholders can be held liable to external creditors.

[30] For robustness check, we also collected the rollout schedules for other judicial changes during the reform period.

enforcement of judgments, we scraped the website of *Credit China*, which publicizes a complete list of "defaulters" in China, including firms and individuals who have failed to fulfill court orders. By linking every case of judicial non-compliance with the corresponding commercial lawsuit, we are able to identify the verdicts that were not fully executed.

3.4 Descriptive Statistics

Appendix Table A.2 presents summary statistics of the main variables, including information extracted from the commercial lawsuit verdicts, basic characteristics of the firm litigants involved in the commercial lawsuits, and information on inter-regional investment flows constructed based on firms' shareholding records. For each variable, we report the number of observations, mean, standard deviation, and 5th and 95th percentiles values.[31]

As a motivation for the subsequent econometric analyses, Figure 2b plots the time-series patterns of reform roll-out and judicial local protectionism, both aggregated to the national level. The orange bars represent the cumulative number of prefectures that had adopted the reform by a given year, and the black line shows the difference in local defendants' average win rates between courts that were treated by 2021 and those that remained untreated at that time.[32] The figure shows that, as the reform rolled out to more courts, local defendants' average win rates in reformed courts dropped sharply and steadily, relative to local defendants' average win rate in the courts that remained non-reformed by 2021.

We then use the blue dash line to represent the difference in the total number of external investments received by counties that were treated by 2021 and those that remained untreated at that time. Figure 2b shows that, as local courts in more counties experienced the reform, there was a widening gap between the investment flows into reformed counties and the flows into counties that remained non-reformed by 2021.

Figure 2b suggests that, at the aggregate level, the reform roll-out is strongly correlated with both court rulings and investment flows. In subsequent sections, we investigate the causal impacts of the judicial organizational reform on these two outcome variables by exploiting the staggered reform roll-

[31] It is worth noting that the baseline average win rate of the local defendants is 0.45. This could be due to the possibility that, prior to the reform, non-local firms would not dare challenge local firms unless they had a very strong case. A related interpretation is that many cases that appeared strong *ex ante* (taking the local courts' biases into account) are settled outside of court, causing the win rates to converge to 50%-50% in equilibrium (Priest and Klein, 1984).

[32] The fact that the black line starts at 3% in 2014 could be indicative that the unreformed regions (as of 2021) had lower levels of judicial biases to start with.

out, and we discuss a battery of possible confounding interpretations.

4 Judicial Impacts of the Reform on Local Protectionism

This section examines the reform's judicial impact on local protectionism. First, we discuss baseline findings on changes in local firms' win rates against non-local firms (Section 4.1). Second, we decompose the baseline effects into "intensive margin changes in judges' incentives" and "extensive margin changes in case compositions" (Section 4.2). Finally, we explore how the reform has affected the quality of judicial decisions (Section 4.3).

4.1 Local Firms' Win Rates in Inter-Regional Lawsuits

For the baseline analysis, we aggregate the data to a court-semiyear panel, and exploit the staggered roll-out of the reform between 2014 and 2021 to estimate the following Difference-in-Differences (DiD) model:

$$Y_{it} = \alpha \cdot Reform_{it} + \phi_i + \lambda_t + \gamma_p t + \epsilon_{it} \tag{1}$$

where Y_{it} is the outcome of interest for local court i at time t, where each time period is six months; $Reform_{it}$ is a dummy variable that equals one if court i had already gone through the reform at time t, and zero otherwise. ϕ_i and λ_t stand for court fixed effects and semi-year fixed effects, respectively; and $\gamma_p t$ controls for province-year trends. The standard errors are clustered at the prefecture level, since the reform rolled our at this level.For robustness, we also present alternative DiD results based on disaggregated case level data in the appendix.

Table 1 Panel A presents the baseline DiD results, obtained from estimating Equation 1 using court-semiyear level data. The results in Column (1) show that, after the reform, local courts rule significantly less favorably toward local defendants; their average win rate against external plaintiffs falls by 3.3 percentage points, representing a 7.3% reduction from the baseline average win rate. The impacts of the reform appear to be homogeneous for both contract and non-contract disputes.

As a placebo test, Table 1 Panel B replicates Panel A with a different outcome variable: the average win rate of local defendants against *local* plaintiffs. As shown in Column (1), the baseline pattern doesn't exist for this placebo sample, indicating that the baseline result is specific to the reduction of local protectionism, rather than other confounding factors that affect all plaintiffs/defendants.

4.1.1 Robustness of Baseline Results

The baseline findings are robust to alternative data structures, variable constructions, control variables, estimations, and standard errors. In Appendix Table A.5, we conduct a robustness check, where we do not aggregate the data to the court-semiyear level, but instead directly estimate the DiD model at the *case* level. The results in Table 1 become even stronger under this alternative, case-level specification, suggesting that the reform's impact is larger for local courts that have higher case loads.

In Appendix Table A.6, we replace the baseline outcome variable with alternative measures of local defendants' rate of winning. In Panel A, we focus on the sample with only clear-cut rulings (i.e., all of the non-local plaintiff's claims are either fully supported or fully rejected), and we find that the judicial reform led to more complete wins for non-local plaintiffs against local defendants. In Panel B, we focus on the monetary claim awarded by the court, which is typically the most important claim in commercial lawsuits, and we find that the reform helped non-local plaintiffs win more monetary claims against local defendants.

As explained in Section 2, Appendix Table A.7 reports robustness checks controlling for the rollout schedules of other contemporaneous reform policies. These include the judge quota reform, which sidelined low-ability judges nationwide; the trans-regional jurisdiction reform, which assigned away courts for trials with government entities as defendants; the circuit court reform, which established branches of the SPC in certain provinces; as well as the rotation of central government inspection teams during the anti-corruption campaign. The baseline estimates remain unchanged, indicating that our main findings indeed reflect the impacts of the judicial organizational reform.

In Appendix Table A.8, we estimate the DiD point estimates of the two-way fixed effect model (Equation 1) following the approach suggested by Callaway and Sant'Anna (2021), our results remain the same. To be more conservative with our statistical power, in Panel A of Appendix Table A.9, we also calculate wild bootstrapped confidence intervals with provincial clustering.

4.1.2 Dynamics and Distributions of Reform Impacts

To understand the dynamics of the reform and gauge the validity of our DiD design, we also estimate an event study model:

$$Y_{it} = \sum_{T} \beta_{1T} T_{it} + \phi_i + \lambda_t + \gamma_p t + \epsilon_{it} \tag{2}$$

where T_{it} represents the event study dummy variables: T_{it} equals one if, at time t, T periods (6T months) have passed since court i experienced the reform, and zero otherwise. For all the baseline event studies, we account for heterogeneous treatment effects, following Sun and Abraham, 2021.

Figure 3a plots the event study coefficients obtained from estimating Equation 2. For the baseline specification, we observe a flat pre-trend before the reform, and a salient reduction in local defendants' win rate immediately after the reform. The reform's impact is persistent over more than three years after the reform. In contrast, the event study estimates for local-local cases, and no significant break in trends is observed around the timing of reform adoption. For robustness, Appendix Figure A.1 presents alternative event study specifications following Borusyak et al. (2021) and Callaway and Sant'Anna (2021), our results remain.

In Figure 3b, we further decompose the DiD results by the win rate of the local defendants. Specifically, using case-level data, we assign each ruling into one of four bins based on the local defendant's win rate: 0-25%, 25-50%, 50-75%, and 75-100%. We then separately fit Equation 1 on indicators for whether the local defendants' win rate belongs to each bin, and plot the DiD coefficients from these four regressions. This exercise shows that the judicial reform has *inframarginal* effects on local protectionism: as a local court goes through the reform, local defendants' win rates become significantly less likely to fall in the 75-100% bin (massive win) and much more likely to fall in the the 0-25% bin (massive loss), with relatively small changes for the two bins in between. This finding is consistent with the interpretation that, before the reform, local protectionism significantly distorted judicial decisions in favor of a subset of local firms, and the reform essentially reversed the rulings for these cases. To the extent that non-local firms are risk-averse, this type of infra-marginal home bias could be more distorting than a scenario where the rulings are slightly and equally tilted in favor of all local firms.

4.2 Change in Judges' Incentives vs. Case Compositions

Our baseline findings on local defendants' win rates could come from two sources. First, the judicial organizational reform could change the incentives of the judges, so that the same case would be adjudicated differently before and after the reform. Second, after observing the intensive margin effects, external firms could adjust their litigation decisions accordingly (i.e., external firms might become more willing to sue local firms), thereby changing the composition of commercial lawsuits. In this section, we separately examine these two channels and show that our baseline effects are primarily driven by the change in judges' incentives. If anything, the changes in case composition tend to work against our baseline findings, as the reform has encouraged litigation by external plaintiffs that were

otherwise less likely to win.

First, to isolate the change in judges' incentives, we focus on the subset of lawsuits that were filed within six months before the local court implemented the judicial organizational reform, and compare the rulings that were made before and after the reform. As shown in Appendix Table A.10, this comparison holds constant the composition of cases: the cases that received rulings before and after the reform are balanced in the characteristics of the plaintiffs and defendants. Therefore, comparing these two sets of cases would allow us to exclude the compositional changes and single out the change in the incentives of judges. As shown in Table 2, for the same court and judge, rulings made right before the reform appeared to be significantly more favorable to local defendants, compared to rulings made shortly after the reform. The effect size is significantly larger than the baseline DiD estimates, confirming that the change in judges' incentives is the main driving force behind our baseline findings.[33]

Second, we investigate the reform's impact on the composition of cases that are filed. As shown Appendix Figure A.3a, after a local court adopted the reform, the number of cases in which external plaintiffs sue local defendants increased steadily in the subsequent years. This is consistent with reduced local judicial protectionism encouraging external plaintiffs to sue local defendants. Table 3 Column (1) quantifies this effect: lawsuits between external plaintiffs and local defendants increased by 6.3% following the reform. In addition to the total number of cases, the types of plaintiffs and defendants involved in these inter-regional lawsuits also shifted significantly. As documented in columns (2) through (7) of Table 3, after the reform, the plaintiffs on average had 6.4% less registered capital and 16.4% fewer employees (noisy), and were 7.2% younger in terms of firm age. In contrast, the local defendants were on average 15.1% larger as measured by total capital, 30.8% larger as measured by total employees, and 4.7% older in terms of firm age. These results are consistent with the scenario that, prior to the reform, many small external firms did not bother to sue large local firms due to low perceived chances of winning, whereas they were encouraged to try such lawsuits after the reform.

Since the reform leads to more cases with weak external plaintiffs and strong local defendants, to the extent that such cases tend to have a lower win rate for the plaintiffs (which explains why many plaintiffs did not file such lawsuits prior to the reform), adding such cases to the composition would likely create a downward bias in our baseline estimate. This is also consistent with the fact that the impact of the change in judges' incentives, which is free from this change in case composition, appears to be substantially larger than the baseline effect.

[33] The exploitation of within-judge behavioral changes is similar in spirit to Ash et al. (2022).

4.3 Quality of Judicial Decisions

We interpret the results documented in Section 4.1 as evidence for the court becoming less locally-biased after the judicial organizational reform. However, an alternative interpretation is that, after the reform, as local courts are no longer pressured by local governments to favor local firms, the courts may simply make lower-quality decisions, or even start "selling" verdicts to whichever party is willing to pay a higher bribe. If that is the case, the court is simply replacing one bias with another, and the observed declines in local defendants' win rates may not suggest improvements in the quality of judicial decisions.

To examine this alternative hypothesis and better understand the mechanisms through which the reform affected local defendants' win rates, we investigate how the reform has affected the *quality* of judicial decisions. Following the legal literature, we measure the quality of judicial decisions in four different ways. First, a lower appeal rate is generally used as a proxy for better-quality rulings (Baye and Wright, 2011). Second, when either party requests examination of key evidence or testimony of an expert witness, a higher approval rate is deemed more fair (Edmond and Roberts, 2011). Third, the decision is generally deemed more legally sound if there is a higher word count in the judgment file that explains the judicial reasoning behind the verdict (Liu, 2018). Fourth, judicial reasoning that frequently cites "discretionary codes" are typically deemed less legally sound (Liu and Li, 2019).[34]

Table 4 shows that, across all four quality measures, there are significant improvements in judicial quality after a court goes through the judicial organizational reform. First, appeal rates drop by 15.4% for external plaintiffs while remaining unchanged for local defendants.[35] A second improvement in judicial quality is that judges have become 24.7% more likely to approve external plaintiffs' requests for evidence examination and expert witness testimony, while not changing approval rates for local defendants. A third measure of improved quality is that judges are providing more detailed legal

[34] "Discretionary codes" are moral remedies that judges can resort to when there are loopholes in the formal legal codes. For example, a discretionary code in Chinese law is the "fairness" principle, which requires the judge to make a ruling that is fair to both parties. Abusing discretionary codes is known as a way to bypass the law and favor a certain party.

[35] In Appendix Table A.11, we restrict the outcome to "appeal rate for plaintiff/defendant when losing (defined by win rate below 50%)," and show that the results are qualitatively consistent with our baseline. Specifically, after the reform, external plaintiffs are less likely to appeal even if they lose. In addition, after the reform,local defendants are no more likely to appeal even if they lose. The decrease in appeals by external plaintiffs is especially suggestive of the reform's role in improving the quality and fairness of judicial decisions. The unchanged appeal rate by local defendants is also helpful for ruling out a potential alternative interpretation, that perhaps the external plaintiffs are discouraged from appealing because they perceive the higher-level courts as even more protective of local firms after the reform, and thus even less likely to reverse the initial verdict. However, this interpretation is inconsistent with the fact that, after the reform, local defendants are no more likely to appeal, even when they have lost.

reasoning for the rulings in publicized judgment files (as reflected by a 7.5% increase in the word count for legal reasoning). Fourth, judges have become 11.2% less likely to cite discretionary codes when justifying their rulings.

Taken together, these results suggest that the quality of judicial decisions improved significantly following the judicial organizational reform. Specifically, these results are consistent with the scenario that, prior to the reform, courts treated external plaintiffs unfairly by denying their rightful requests to examine evidence and call witnesses, making rulings with insufficient legal foundation, and exercising too much discretion in decision-making. The reform seems to have reduced these judicial biases that were previously imposed on external plaintiffs.

To understand the dynamics of these results, in Appendix Figure A.4, we plot the event study coefficients that correspond to the DiD results presented in Table 4. For all four outcome variables, we observe flat trends leading up to the reform, and then salient improvements after the reform was implemented. The effect sizes seem to be stable or even increasing over time, which helps explain why the baseline results on win rates appear to be persistent.

5 Limitations of Centralizing Judicial Control

While Section 4 reports reduced judicial local protectionism following the judicial reform, It is important to note that this reform does not entirely insulate judicial decisions from political influence; instead, it transfers judicial control to higher levels of government, potentially creating new distortions.

In this section, we investigate the reform's potential limitations. Specifically, we find that the reform (a) fails to shield protectionism for firms connected to the provincial or central government (Section 5.1); (b) failed to prevent protectionism in inter-provincial lawsuits (Section 5.2); (c) was undermined in regions where the local officials were able to lobby the provincial government (Section 5.3); and (d) led to the promotion of less capable judges (Section 5.4). We find no evidence that the reform induced longer delays in rulings (Section 5.5) or reduced the rate of compliance with court orders (Section 5.6).

5.1 Reform Fails to Shield Protectionism for Highly Connected Firms

As the reform consolidates the judicial control to the higher-level governments, a natural question is whether the reform alleviates or exacerbates judicial favoritism toward firms with high-level connections. To answer this, in Columns (1) through (3) of Table 5, we investigate the heterogeneity in the baseline result with respect to the local defendant's political connections. We define a firm as politically connected to local government if it has won at least one procurement contract from the county or prefectural governments since 2014. Similarly, a firm is defined as connected to a provincial or central government if it has won procurement contracts from these higher levels of government. Our results show that the judicial reform reduces unconnected local defendants' win rates by 7.3%; reduces locally connected local defendants' win rates by 15.8%; and has no detectable impact on the win rates of provincially or centrally connected local defendants.[36] These findings suggest that the reform's impact is most salient on the locally connected firms, which lost their local protection as a result of the reform. Despite the consolidation of judicial power at the provincial level, there is no evidence for the exacerbation of judicial favoritism towards provincially and centrally connected firms.

In a similar spirit, we examine whether the reform has had differential effects on the locally owned SOEs versus the provincially and centrally owned SOEs.[37] As shown in Appendix Table A.12, locally owned SOEs have significantly lower win rates after the reform, while the provincially and centrally owned SOEs do not experience any noticeable changes in win rates following the reform. These results are consistent with the findings in Table 5, showing that the reform has enabled local judiciaries to operate more independently from the *local* governments, while the provincial and central governments maintain similar (but not higher) levels of favoritism as they had pre-reform.

5.2 Protectionism Remains for Inter-Provincial Lawsuits

Because the reform consolidates fiscal and personnel controls over local courts to the provincial level, it should mitigate local protectionism to a greater extent for those cases involving firms from the same province (but different counties) than for cases with plaintiffs from outside the province. This hypothesis is confirmed by the results in Columns (4) and (5) of Table 5: inter-county cases within the same province tried in the reformed local courts saw a reduction in the defendants' win rate of

[36] These coefficients are pair-wise significantly different at the 1% level.

[37] Based on the shareholding information in the business registration records, we identify both traditional SOEs and firms in which SOEs hold majority stakes.

5.3 percentage points, while inter-provincial cases saw a reduction in win rate of only 1.8 percentage points.[38]

The smaller but still significant effect documented in Column (5) can be interpreted in two ways. On the one hand, the shrinkage in effect size implies that the reform leaves open the scope for provincial governments to exercise local favoritism. On the other hand, the non-zero effect size suggests that, after the reform, the degree of protectionism experienced a decline even for inter-provincial cases. This could be due to two potential reasons. First, it might be more difficult for firms to influence provincial governments, relative to influencing lower level (county and prefectural) governments. Second, the return to protecting an individual firm may be lower for provincial governments than for county and prefectural governments.

5.3 Courts Remain Locally Captured in Connected Jurisdictions

While the reform consolidated the financial and personnel controls over the local courts to the provincial level, it remains possible that certain local governments maintain some degree of indirect control over the local judiciaries through lobbying the provincial governments.

To examine this possibility, we proxy a local government's ability to lobby the provincial government by whether the local government leader has served in the provincial government earlier in his/her career. As shown in Appendix Table A.13, the effectiveness of the reform in alleviating local protectionism is significantly weaker if the local leader has had work experiences in the provincial government. This is consistent with the interpretation that past work experience provides channels for a local leader to influence the provincial government, who can in turn help pressure the local courts to favor local firms.

5.4 Provincial Governments Promote Less Capable Judges

A classic limitation of centralized control is that arms-length decision-makers may lack the decentralized information necessary to make informed decisions. In our context, provincial governments could be less informed about the ability of local judges. As a result, by shifting the personnel controls to the provincial governments, the reform may simultaneously alleviate local favoritism and reduce meritocracy in judicial promotions. This would hinder the career advancements of not only locally-biased

[38] The two coefficients are statistically different at the 1% level.

judges but also high-ability ones.

Appendix Table A.14 investigates the impacts of the reform on patterns of judges' promotion. As shown in Column (1), judges who gave more wins to local defendants (against non-local plaintiffs) received more promotions prior to the reform, but not afterward. This confirms the hypothesis that the reform reduced the rewards for locally biased judges.[39] In Column 2, using the number of high-stakes cases handled per year as a proxy for a judge's ability, we find suggestive evidence that, after the reform, lower-quality judges have a marginally increased likelihood of being promoted. Qualitatively, the effect is consistent with the hypothesis that the lack of decentralized information hinders judicial meritocracy; quantitatively, it is small in magnitude (1.4% increase in promotion likelihood).

5.5 No Change in Delays in Court Decisions

There is also the possibility that, despite improving judicial quality, the reform may have slowed down judicial decisions if the judges face a quantity-quality trade-off in handling cases. To examine this possibility, we estimate how the duration of a case (number of days from filing to verdict) has changed in response to the reform. As Appendix Figure A.3c shows, there is no significant change in the duration of trials, indicating that the local judges improved the quality of judicial decisions without sacrificing quantity.

5.6 No Change in Enforcement of Court Orders

Because the provincial governments are politically more distant from local law enforcement agencies, there is a concern that the enforcement of rulings against local defendants may decline. If this is the case, then the nominal reduction in local protectionism may not be consequential. Empirically, we find no evidence that supports this hypothesis. Specifically, we utilize a unique dataset from *Credit China*, which documents every case of non-compliance with court orders, and publicizes the non-compliant party as a "defaulter." By linking this non-compliance record to all commercial lawsuits in our data, we compare the quality of judicial enforcement before and after the reform. As shown in Appendix Table A.16, the non-compliance rate did not change significantly in response to the reform. When further decomposing the outcome variable into "partial non-compliance" and "complete non-compliance," as reported by *Credit China*, the null result remains. These patterns provide evidence

[39] In a similar spirit, Appendix Table A.15 demonstrates that more locally-biased courts received more funding prior to the reform, but not afterward.

against the “weakened enforcement" hypothesis.

6 Net Impacts on Economic Integration

As reported in Sections 4 and 5, the judicial organizational reform has mixed impacts on judicial outcomes. On one hand, it systematically alleviates local protectionism, which might encourage economic integration. On the other hand, it faces several shortcomings that may constrain its ability to fully support economic integration. In this section, acknowledging these countervailing forces, we evaluate the net impacts of the reform on economic integration.

Specifically, in Section 6.1, we leverage novel panel data on the universe of inter-regional investment in China, and examine the reduced-form relationship between the judicial organizational reform and inter-regional investment flows. In Section 6.2, we document that the reform has encouraged new entries into local markets while discouraging protection-seeking investments into politically connected local firms. In Section 6.3, we investigate the spillover effects between treatment and control regions. In Section 6.4, we construct a simple model of cross-location business investments and derive model-based sufficient statistics to map our reduced-form estimates into the overall economic consequences of reducing judicial local protectionism.

6.1 Inter-Regional Investment

As explained in Section 3.2, based on the annual changes in each firm’s shareholding structure, we are able to trace every investment received by each firm to its source, which was either an individual investor or a firm investor. Aggregating this information at the county-year level, for each county, we obtain the yearly total equity investments received by all local firms from outside investors, which is the main outcome variable that we focus on in this section.[40]

There is considerable evidence suggesting that, in an effort to enhance the local business environment, local judiciaries frequently meet with business associations and entrepreneurs to raise awareness about the reform and share the courts’ commitment to promoting impartial rulings.[41] Given the prominence of the judicial organizational reform among businesses and investors, the degree of in-

[40] Note that here we can no longer disaggregate the time dimension to every six months, because many firms only update their shareholding information annually.

[41] For example, see: `https://www.gdcourts.gov.cn/xwzx/yw/content/post_1045183.html`.

vestment responses likely depends on how convinced investors are about the reform's effectiveness in mitigating judicial local protectionism.

In Table 6, we estimate Equation 1, using "inward investment" as the outcome variable. As revealed by Columns (1) and (2) of Panel A, regions that adopted the reform in earlier years (i.e., 2014 to 2017) did *not* experience statistically significant gains in inward equity investment, while regions that adopted the reform in later years (i.e., 2018 to 2021) saw an additional 13.3% in inward investment amount. Column (3) of Panel A pools all regions together as shows an average 11.2% increase in inward investment amount for the full sample.[42] Columns (1) to (3) of Panel B show that these findings are robust to using the total number of inward investment.[43] These patterns are consistent with a scenario where as the reform expands nationwide, outside investors gradually update their expectations about reform effectiveness, and adjust their investment decisions accordingly.[44]

Related to the heterogeneity with respect to early vs. late reformed regions, we also find evidence that firms learn about the reform's effectiveness through their own litigation experiences. Specifically, after firms have had trial experiences in a post-reform region, they become more likely to invest in other reformed counties. Empirically, for all reformed counties, we focus on the subsample of firms that experienced lawsuits either one year before or after the adoption of the reform. As shown in Appendix Table A.17, compared to firms engaged in pre-reform trials, those that experienced post-reform trials—including both the winners and the losers—become significantly more likely to make new investments into other reformed counties. This result corroborates the narrative that firms learn/update about reform effectiveness before adjusting their investment decisions.

An important aspect of the investment response is whether non-local firms choose to enter the local market by establishing new branches (new entry) or by investing in existing local firms (joint ventures). In the case of new entry, these newly established branches, despite being legally registered as local entities, often face the challenge of engaging extensively with local workers and entrenched incumbent firms that have deeper roots and stronger political connections. Such dynamics can lead to

[42] In Panel B of Appendix Table A.9, we present alternative confidence intervals constructed from wild bootstrap with province-level clustering.

[43] It is also worth noting that the magnitude of the investment response is even larger than the proportional decline in the local defendants' win rate. Losing a lawsuit is very costly; hence, a more impartial court can not only better protect an external plaintiff's legal rights when a dispute occurs but also deter the local firm from taking advantage of its external partners in the first place, which further encourages economic integration. As a result, the response of investment to the reforms could be larger than that shown by the judicial outcome alone, as Table 6 shows.

[44] These findings are more pronounced in industries that involve more legal uncertainties (Appendix Table A.18), echoing the rich cross-country evidence documented in the FDI literature (Globerman and Shapiro, 2002; Li and Resnick, 2003; Li, 2021).

heightened transaction costs and contractual disputes. By reducing judicial local protectionism, the reform has the potential to lower these transaction costs, thereby encouraging more inward investment through direct entry.[45] The reform's impact on joint ventures, however, is less straightforward. On one hand, partnering with politically connected local firms can serve as a substitute for direct entry, shielding non-local investors from local judicial biases. By making direct entry more feasible, the reform could reduce the relative attractiveness of joint ventures. On the other hand, joint ventures themselves might benefit directly from the reform through improved investor protection, potentially enhancing their appeal to non-local investors.

In Columns (4) and (5) of Table 6, we evaluate these predictions by separately analyzing the reform's effects on inter-regional new entries and joint venture formations.[46] The results reveal a substantial and statistically significant increase in new entries, coupled with a relatively modest change in joint ventures. Notably, we observe a *decline* in investments into locally connected firms for joint venture formation after the reform (as defined in Section 5.1), as shown in Column (6) of Table 6. These findings align with the hypothesis that the reform lowers transaction costs for non-local firms, enhancing their ability to directly enter local markets without needing to seek legal protection by partnering with politically connected local firms.[47]

Figure 4 shows the dynamics of the investment effects following the methods proposed by Sun and Abraham (2021). As shown in panels (a) and (b), for both the early and late adopters of the reform, there are no pre-trends in investments prior to the reform. After implementing the reform, the early adopters did not experience any modest gain in inward investment until three years later, while the late adopters saw much faster onset of reform impacts, as well as larger magnitudes of treatment effects. Similarly, as illustrated in panels (c) and (d), both the establishment of new branches and the formation of joint ventures had flat trends prior to the reform, but demonstrate opposite trajectories afterwards.

6.2 Other Heterogeneities in Investment Responses

Another interesting and related dimension of heterogeneity concerns the types of local industries receiving investments from outside. While lower transaction costs should unambiguously encour-

[45] Technically, non-local firms may structure their local entries as "local firms" in the legal sense. However, the degree of judicial protection and favoritism depends not on their de jure locality status but on their de facto political connections, which new establishments typically lack compared to incumbent local firms.

[46] The corresponding event studies are shown in Appendix Figures 4c and 4d.

[47] As further detailed in Columns (3) and (4) of Table 7, the observed increase in investment is primarily driven by non-tradable sectors rather than tradable ones, which supports the interpretation that the new entries are non-local firms targeting local markets.

age more within-industry entry by non-local investors that aim to expand their business to the local market (e.g., a restaurant chain may open a new branch in the local area), the impact of the reform on inter-regional investments into complementary industries with input-output connections is more ambiguous. According to the logic of Coase (1937), a non-local firm should simply transact with its complementary upstream and downstream local firms if the cost of doing so is low, and only consider integrating with the local firms when transaction costs are high. This logic implies that the judicial reform may actually reduce non-local firms' investments into local firms in complementary upstream and downstream industries.

As shown in Columns (1) and (2) of Table 7, our baseline investment results are indeed entirely driven by inter-regional investments within the same industry as the external entrepreneurs.[48] In stark contrast, inter-regional investments across adjacent industries (defined as the five upstream and five downstream industries that are the most strongly connected via input-output linkages) actually decrease significantly following the reform. These findings are highly consistent with the judicial organizational reform having reduced non-local firms' transaction costs of dealing with local business partners, thereby promoting direct entry and reducing non-local firms' incentives to integrate with complementary local firms.

6.3 Spillover Effects

To interpret the implications of the DiD results on investment response, one potential issue is that the DiD estimator may over-state the aggregate economic impact as entrepreneurs substitute investments away from the control locations and into the treated locations in response to the reform. Specifically, because the DiD strategy compares the before-after differences in non-local investments between treated and control locations, the estimator in principle cannot distinguish between new investments that would not have been made absent reform and the substitution of investments away from the control locations towards treated locations. Hence, the DiD estimator may incorrectly attribute the distributional effect resulting from cross-location substitution of economic activities to the aggregate gains experienced by the treated locations.

We show this issue is unlikely to be empirically relevant because the treatment effect we find is indeed due to better regional economic integration rather than to substitution of economic activities. Specifically, we exploit the fact that the reform's roll-out varies at the prefecture level, meaning all

[48] The event studies corresponding to these results are presented in Appendix Figures A.5a and A.5b

counties within the same prefecture have the same treatment status over time. Column (1) of Appendix Table A.19 estimates the reform's impact on investment flows from outside counties within the same prefecture.[49] Results show that the baseline investment response is predominantly driven by these intra-prefectural investment flows; the total amount (and total number) of investment from other counties in the same prefecture increased by 19.4% (12.8%) following the reform. In contrast, Columns (2) and (3) show that investment flows from external prefectures or provinces do not experience significant increases. These patterns provide direct evidence for the limited degree of substitution in non-local entrepreneurs' destination choices.

Analogously, another concern that could potentially compromise our interpretation is that the investment response by non-local entrepreneurs could affect local investments. This effect could arise either because non-local entrepreneurs substitute away from investing locally toward investing non-locally into other counties, or because external investments crowd out (or crowd in) local investments. In Column (4) of Appendix Table A.19, we repeat the DiD exercise using intra-county investments as the outcome variable; the result shows that the judicial organizational reform does not affect local entrepreneurship differentially between treated and control prefectures. In Appendix Table A.20, we further examine the response of different types of local investments, including the entry by new local firms (Column (1)); local investments to existing local firms (Column (2)), to adjacent industries (Column (3), again defined as the five upstream and five downstream industries that are the most strongly connected via input-output linkages), and to the same industry as the existing firm (Column (4)). We also separately examine the local investment response in tradable and non-tradable sectors. Along all of these margins, we find no evidence that the judicial organizational reform has an impact on local entrepreneurship. Taken together, these results point against the empirical relevance of this concern.

The fact that the investment responses are driven by increased intra-prefectural investment flows also helps explain the quick onset of these effects. Because the reform roll-out varies at the prefecture level, firms in a treated county are fully aware that other counties within the same prefecture have also been treated. Therefore, investors and entrepreneurs can extrapolate the changes in their own county and update their beliefs about the legal environment in the rest of the prefecture. This is consistent with our earlier finding that the reform's impact appears very quickly: the judicial impact starts to appear after six months, while the economic impact starts to appear after one year.

[49] The corresponding event studies are presented in Appendix Figures A.5c and A.5d.

6.4 Welfare Implications

To conduct a back-of-the-envelope calculation for the aggregate economic impact of the reform, in this section we present a simple model of inter-jurisdiction investments with endogenous entry, along the lines of Melitz (2003). In the model, the judicial reform translates into a reduction in the cost of cross-region investments by external entrepreneurs.[50] We show that the investment response to the reform, as estimated in Section 6.1, is a sufficient statistic for the economic benefits of the judicial reforms and the associated elimination of protectionism-induced distortions. We use the sufficient statistic to map our reduced form estimates to aggregate economic surplus.

6.4.1 Model Setup

A unit mass of non-local entrepreneurs can choose to enter a location (county) and serve the local consumers. The local consumer has separable preferences over products (goods and services) from non-local firms:

$$U = \int_{\varphi \in \Phi} u\left(q\left(\varphi\right)\right) - p\left(\varphi\right) q\left(\varphi\right) \, \mathrm{d}F\left(\varphi\right), \tag{3}$$

where Φ is the set of non-local entrepreneurs (indexed by φ) that serve the location, $u\left(q\right) \equiv \frac{\epsilon}{\epsilon-1} q^{\frac{\epsilon-1}{\epsilon}}$ is utility derived from each firm φ. The consumer preferences (3) imply the following demand function for each firm:

$$q^{*}\left(p\right) = \arg\max_{q} \left\{u\left(q\right) - pq\right\} = p^{-\epsilon}; \tag{4}$$

the parameter ϵ captures demand elasticity. We focus on products by non-local firms and omit local products from preferences in (3); this simplification is motivated by the evidence that the judicial reform does not affect local entrepreneurship.

When firms make entry and pricing decisions, each entrepreneur draws a cost index $\varphi \leq 1$ from distribution $F\left(\varphi\right) = \varphi^{\gamma}$ and decides whether to pay the fixed entry cost f to produce in location n with marginal cost $\tau\varphi$. $\tau \geq 1$ is a cost shifter that depends on judicial fairness; a more locally biased justice system raises non-local firms' production costs through higher τ. We simply refer to τ as the degree of "local protection" and provide a microfoundation below.

After entry, firms engage in monopolistic pricing, choosing prices that maximize variable profits.

[50] Admittedly, the modeled impact of the reform is simplistic and misses other potential politico-economic implications. Nevertheless, as discussed in Section 4, we find clear evidence of the reform having a positive impact on judicial quality, and we find no evidence for the reform having an impact along several other dimensions. While we naturally cannot fully rule out the all other effects of the reform, our finding is indicative that these other effects are unlikely to be of first-order importance when compared to the judicial benefits.

The expected profit net of entry costs (i.e., producer surplus) is:

$$\Pi \equiv \int_0^1 \max\left\{\pi\left(\varphi\right) - f, 0\right\} \, \mathrm{d}F\left(\varphi\right), \tag{5}$$

where the maximization inside the integral of (5) indicates the entry decision after drawing the cost index φ, and $\pi\left(\varphi\right)$ is the variable profits:

$$\pi\left(\varphi\right) \equiv \max_p \left(p - \tau\varphi\right) q^*\left(p\right). \tag{6}$$

The equilibrium price $p\left(\varphi\right)$ is the maximizer of (6). Define $\bar{\varphi}$ as the cost index for which $\pi\left(\bar{\varphi}\right) = f$. Because of the fixed entry cost f, only entrepreneurs with sufficiently low costs ($\varphi \leq \bar{\varphi}$) will enter.

The judicial cost shifter τ can be microfounded by moral hazard as follows. To serve the local market, a non-local firm must employ production resources locally, including hiring managers and workers, buying production inputs, and entering into contracts with local firms. With probability ρ, these local entities steal a fraction δ of output, in which case the non-local firm can litigate in court and reclaim the stolen output with probability s. The moral hazard friction is isomorphic to having an effective marginal cost multiplier $\tau \equiv \frac{1}{1-\rho\delta(1-s)}$. Under a more locally biased court, the firm has a lower probability of reclaiming stolen output and thus a higher marginal cost of production. We note that $\tau = 1$ under a fair judicial system ($s = 1$) that perfectly enforces property rights.

Given the degree of local protection $\{\tau\}$, an equilibrium is a set of entry decisions, prices $\{p\left(\varphi\right)\}$, quantities $\{q\left(\varphi\right)\}$, and variable profits $\{\pi\left(\varphi\right)\}$, such that a firm enters iff $\pi\left(\varphi\right) \geq f$, $\pi\left(\varphi\right)$ solves (6), prices are the maximizers of (6), and quantities are consistent with the consumer demand function $q\left(\varphi\right) = q^*\left(p\left(\varphi\right)\right)$.

Notably absent from the model are the selection margins of entrepreneurs choosing among a set of potential locations or choosing between investing locally and non-locally. The model also does not feature any local firms, thereby shutting down potential spillover effects from non-local entry investments to local entrepreneurship. These simplifications are motivated by our reduced-form findings in Appendix Table A.19, showing that there is no detectable investment substitution from non-treated to treated prefectures or any impact on local investments. Below, we focus on the empirically relevant case and exclude these considerations as we formalize our arguments for welfare calculations. In Section Appendix B.2, we generalize the model to incorporate the location choice margin.

6.4.2 Welfare Impact of the Judicial Reform

Through the lens of the model, a judicial reform reduces local protection τ and can affect consumer and producer surplus through two channels. First, the reform reduces the production costs and prices of non-local firms, thereby affecting the associated consumer surplus and profits. Second, higher profits lead to more entry, through a higher cutoff cost index $\bar{\varphi}$ (as firms with costs $\varphi \leq \bar{\varphi}$ will enter).

As we show in the appendix, the response of consumer surplus (equation 3) to judicial reform (a decline in τ) can be decomposed as

$$
\begin{aligned}
-\frac{\mathrm{d}\ln U}{\mathrm{d}\ln\tau} &= \frac{-1}{\int_0^{\bar{\varphi}} u(\varphi)\, F(\varphi)} \left(\underbrace{\int_0^{\bar{\varphi}} \frac{\mathrm{d}u(\varphi)}{\mathrm{d}\ln\tau}\, \mathrm{d}F(\varphi)}_{\substack{\text{lower production} \\ \text{costs among entrants}}} + \underbrace{\frac{\mathrm{d}\int_0^{\bar{\varphi}} u(\varphi)\, \mathrm{d}F(\varphi)}{\mathrm{d}\bar{\varphi}} \frac{\mathrm{d}\bar{\varphi}}{\mathrm{d}\ln\tau}}_{\substack{\text{entry of marginal firms given} \\ \text{a higher cutoff cost index}}} \right) \\
&= \underbrace{(\epsilon - 1)}_{\substack{\text{lower production} \\ \text{costs among entrants}}} + \underbrace{(\gamma - \epsilon + 1)}_{\text{new entrants}} = \gamma,
\end{aligned}
\tag{7}
$$

where $u(\varphi)$ is the equilibrium consumer surplus obtained from a non-local firm with cost index φ (the maximand of 4).

Two key elasticities, ϵ and γ, determine the response of consumer surplus along each separate channel. The demand elasticity ϵ governs how consumer surplus responds to lower production costs and prices among the existing entrants. The cost distribution's shape parameter γ captures the relative productivity between marginal and average entrants; hence, along with the demand elasticity ϵ, γ controls how the surplus responds to marginal entrants. When the inverse marginal cost has a Pareto distribution, as is the case here, the net effect of these two channels can be summarized simply by the shape parameter γ, as the demand elasticity drops out.

The appendix conducts an analogous decomposition for producer surplus Π and total sales of non-local firms $R \equiv \int_0^{\bar{\varphi}} p(\varphi)\, q(\varphi)\, \mathrm{d}F(\varphi)$. Our next result shows that the judicial reform's impact on the number of non-local investors, $\mu \equiv F(\bar{\varphi})$, is a sufficient statistic for the impact on consumer surplus, producer surplus, and total sales by non-local firms.

Proposition 1. $\frac{\mathrm{d}\ln\mu}{\mathrm{d}\ln\tau} = \frac{\mathrm{d}\ln U}{\mathrm{d}\ln\tau} = \frac{\mathrm{d}\ln\Pi}{\mathrm{d}\ln\tau} = \frac{\mathrm{d}\ln R}{\mathrm{d}\ln\tau}$.

We can therefore use the empirical measurement of how the number of external investors responds

to the reform to assess the overall economic impact of the reform.[51] Our difference-in-differences estimator in Table 6 shows that, when counties in a prefecture experienced the reform, the number of outside entrepreneurial investments received by those counties increased by 11.4% relative to the control group, with a slightly smaller increase in investment value. Proposition 1 implies that economic surplus accrued to non-local products and services experiences the same proportional gains from the reform.

The judicial organizational reform has thus led to substantial improvements in cross-county economic integration in treated prefectures. Our treatment effect estimates imply that, if adopted throughout China, the judicial organizational reform could lead to over a $200 billion increase in annual cross-county investments in terms of registered capital. Because registered capital only measures entrepreneurial investments at the beginning of a firm's life cycle, the subsequent economic impact is likely to be substantially larger in magnitude. As a back-of-the-envelope calculation, we assume the judicial reform affects cross-county economic linkages equally at all of a firm's life cycle stages, so that we can extrapolate our estimates proportionally to the entire economy. Since non-local firms from outside counties account for for 17.1% of total firm count (and 15.3% by registered capital), Proposition 1 implies the judicial reform, by facilitating more inter-regional investment flows (within each province), can potentially expand GDP by 1.9% if adopted throughout China.

7 Conclusion

China's high-stakes judicial organizational reform consolidated the financial and personnel controls over local courts to the provincial level, thereby partially shielding local courts from the capture of local governments. This paper shows that, despite its various limitations, the reform has systematically reduced judicial local protectionism and facilitated economic integration in the country.

To reach this conclusion, we compiled novel administrative data covering millions of commercial lawsuit verdicts and business registration records and exploited the staggered roll-out of the reform between 2014 and 2021. We find that the reform has significantly reduced judicial local protectionism in China, as reflected by a 7.3% reduction in local defendants' average win rate against external plaintiffs. This effect is particularly salient for firms connected to local governments, which is consistent with these firms receiving more protection prior to the reform. We decompose the reform impacts into

[51] We use the response of the number of external investments, instead of the total investment amount, to assess the overall economic impact of the reform because, as described in Section 3.2, the latter variable is subject to measurement error.

intensive and extensive margins. On the intensive margin, we document that, for similar cases, the same judge rules significantly less favorably toward local defendants after the reform. On the extensive margin, we find that reform has led an increasing number of small, young, and external firms to sue their large, established, and local counterparts, thereby changing the composition of civil lawsuits in China.

Across a series of measures of judicial quality widely used by legal scholars, we find clear evidence that, after the reform, local courts not only reduced favorable rulings toward local firms but also improved the quality of judicial decisions. We find that external plaintiffs became less likely to appeal the verdict (no change for local defendants); courts became more likely to allow external plaintiffs to examine evidence or invite an expert witness (no change for local defendants); judges provided more detailed judicial reasoning in the judgment files; and judges were less likely to cite discretionary codes in judicial reasoning. Taken together, these results consistently suggest that the reform has led local courts to reduce their favoritism toward local defendants and to improve the quality and fairness of their decisions.

We emphasize that the reform is better understood as a move towards judicial centralization to help promote the *rule by law* at the local level, rather than an embrace of complete judicial independence and the *rule of law*. Following this insight, we document several notable limitations of the reform. First, firms that are connected to the provincial and central governments are immune from the removal of protectionism. Second, local officials who are connected to the provincial governments can undermine the effectiveness of the reform in their jurisdictions. Third, local protectionism remains salient in inter-provincial commercial lawsuits after the reform. Finally, the provincial governments tend to promote less biased but also lower-quality judges, compared to the promotion decisions of lower-level governments before the reform. These findings indicate that, while centralizing the control of local judiciaries has significantly reduced judicial local protectionism, failing to promote complete judicial independence gives rise to important side effects that may undermine the overall effectiveness of the reform.

Further, we exploit administrative data on the universe of inter-regional investment in China and find that external individual and business investors contribute an additional 11.2% capital towards investing in local firms after the reform, suggesting that the reduction of judicial local protectionism has systematically facilitated better economic integration in China. Through the lens of a simple model of external investments à la Melitz (2003), we show that the response of external investments to the judicial reform is a sufficient statistic for assessing the reform's overall economic impact. Our

back-of-the-envelope calculation suggests that, by improving cross-regional economic integration, a nation-wide roll-out of the reform can potentially lead to overall economic gains of as much as 1.9% of China's GDP.

Our findings have important implications for understanding contemporary Chinese political economy. In traditional conceptualizations of the Chinese economy, judiciaries play a limited role because they were viewed as subordinates of the party-state. Many observers of China believe that, as the authoritarian regime consolidated its political control over the past decade, it has turned further against the law, leaving the government's political power unbound by the judicial system. Our findings suggest more a more nuanced perspective. Unlike the institutional forces of the previous decades, China's judicial reform since 2014, despite its various notable limitations, has nonetheless empowered local legal institutions to play an increasingly important role in economic development. This turn toward legalism at the local level, despite political centralization, represents an important yet under-appreciated change in China's delicate politico-economic equilibrium.

As emphasized in our analysis, by consolidating judicial control at the provincial level, China's judicial organizational reform does not fully decouple the judicial and executive branches or impose real checks and balances on top-level political leaders. Nevertheless, the reform represents a significant move toward legalism in China. Going forward, important questions remain regarding the sustainability of the coexistence between a centralized authoritarian government and independent local judiciaries, whether the movement toward legalism can progress further, and how this shift will affect the fate of an authoritarian regime and the long-term welfare of its citizens. Answering these questions will deepen our understanding of China, as well as other authoritarian regimes worldwide, such as Pakistan, Egypt, and Turkey, which have recently undergone similar judicial reforms.

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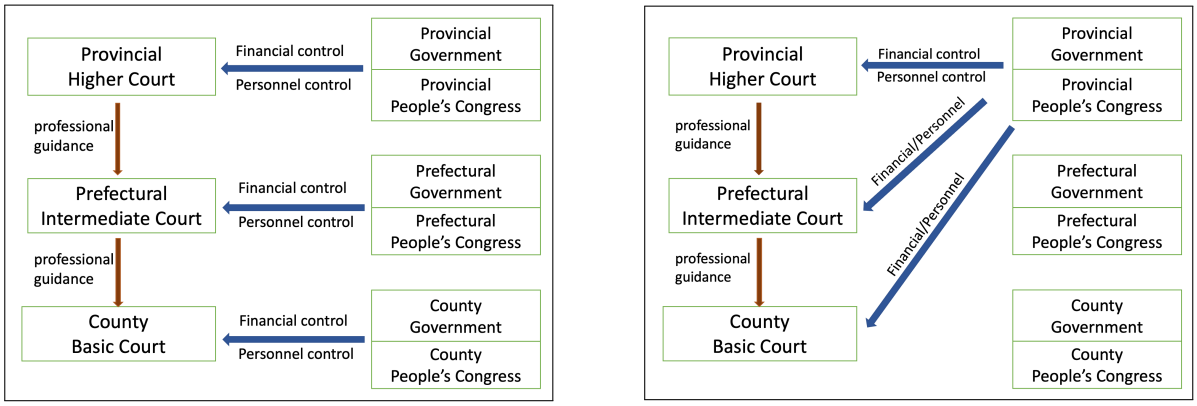
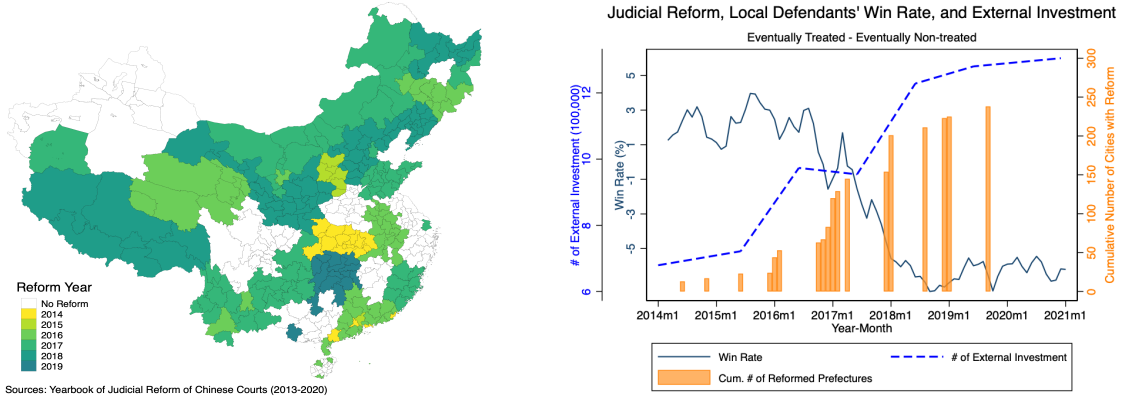
Figures and Tables



(a) Before Reform **(b)** After Reform

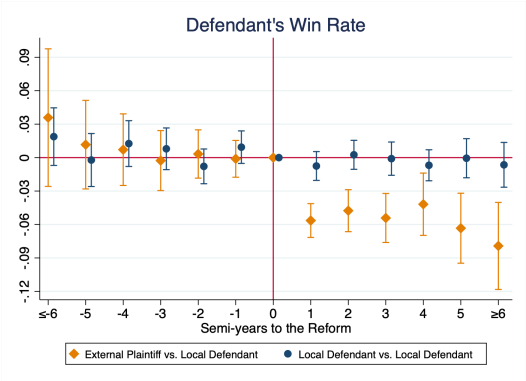
Figure 1: Panels (a) and (b) demonstrate the administrative structure of China's local judiciaries *before* and *after* the judicial organizational reform, respectively



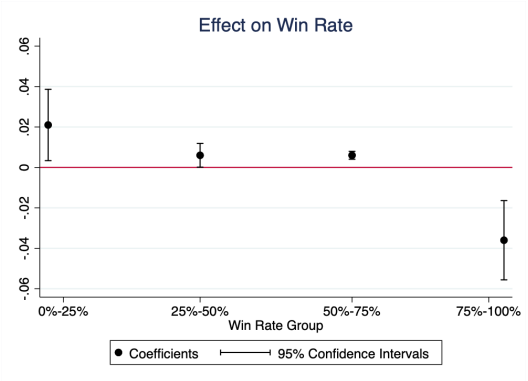
(a) Geographical Expansion

(b) Defendants Win Rate & External Investment

Figure 2: Panel (a) illustrates the roll-out schedule of the judicial organizational reform across the country. In Panel (b), the orange bars represent the number of prefectures that have already adopted the judicial organizational reform in a given year, and the black line plots the difference in local defendants' average win rates (against external plaintiffs) between eventually treated courts and eventually non-treated courts (as of 2021). The blue dash line plots the difference in inward investment flows between eventually treated counties and eventually non-treated counties.

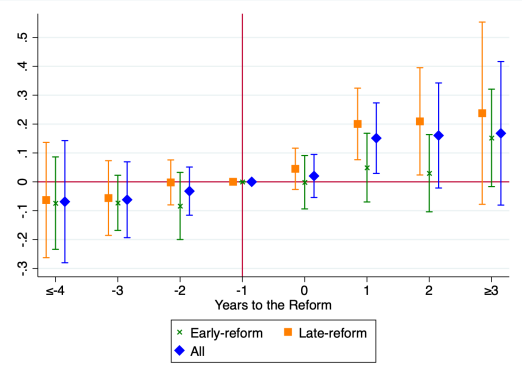


(a) Baseline vs. Placebo

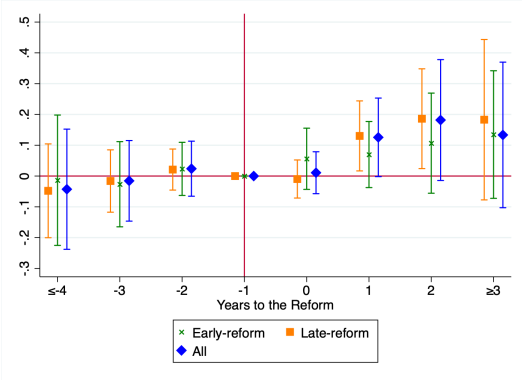


(b) Reform Effect across Groups

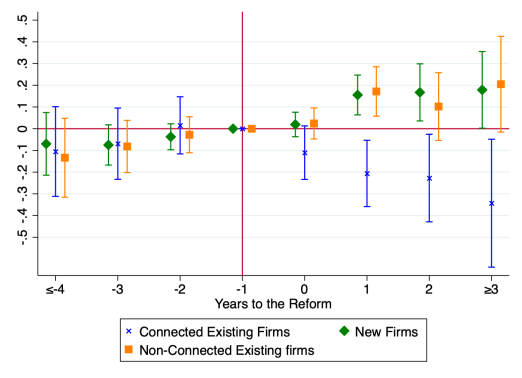
Figure 3: In Panel (a), we plot the event study coefficients (and the corresponding 95% confidence intervals) for the baseline results in Column (2) of Panel A and Panel B of Table 1, respectively. All event studies are estimated following the approach suggested by Sun and Abraham (2021). In Panel (b), we estimate how the reform affected the likelihood of local defendants' win rates falling into different quartiles; the estimates plotted (as well as their 95% confidence intervals) come from four separate DiD regressions.



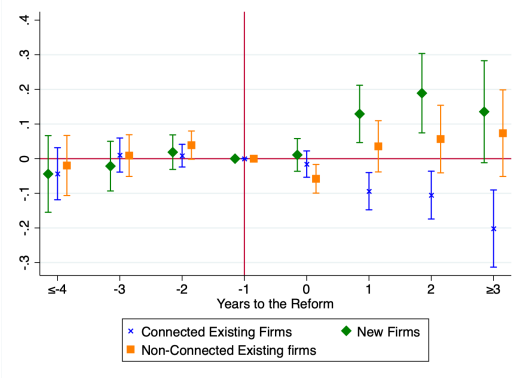
(a) Amount of External Investment



(b) Number of External Investments



(c) Amount of External Investment



(d) Number of External Investments

Figure 4: Notes: Panel (a) plots the event study estimates corresponding to Columns (1) to (3) of Table 6 (as well as 95% confidence intervals) using samples of early-reform regions, late-reform regions, and the full sample, respectively. Panel (b) repeats the exercises using the number of inward investments as the outcome variable. Panels (c) and (d) plot the event study estimates for non-local firms investing in new local branches (new entry) vs. investing in existing locally connected firms (joint venture) vs. investing in existing local firms without government connections, corresponding to Columns (4) to (6) of Table 6. All event studies are estimated following the approach suggested by Sun and Abraham (2021).

Table 1: Judicial Organizational Reform and Local Defendants' Win Rate

	All Cases		Contract Dispute		Non-Contract Disputes	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. External Plaintiff v.s. Local Defendant						
Post Reform	-0.033***	-0.035***	-0.033***	-0.036***	-0.038***	-0.037***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)	(0.011)
Mean of Outcome	0.45	0.45	0.44	0.44	0.52	0.52
Court FE	Y	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	N	Y	N	Y	N	Y
Observations	45,934	45,934	45,210	45,210	28,331	28,331
R-Squared	0.213	0.221	0.207	0.214	0.212	0.215
Panel B. Local Plaintiff v.s. Local Defendant						
Post Reform	0.006	0.005	0.006	0.004	0.008	0.007
	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)
Mean of Outcome	0.39	0.39	0.38	0.38	0.39	0.39
Court FE	Y	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	N	Y	N	Y	N	Y
Observations	49,086	49,086	47,559	47,559	42,825	42,825
R-Squared	0.253	0.256	0.239	0.242	0.217	0.219

Notes: Panel A reports the baseline DiD estimates on judicial outcomes in inter-regional commercial lawsuits, with data aggregated to court-semiyear level. Specifically, for each court and each half-year period, we take the local defendant's average case-level win rate (a continous measure between 0 and 1) as the outcome variable. Column (1) and (2) focuses on the average win rates of all local defendants in all inter-regional commercial lawsuits. Columns (3) and (4) investigate the local defendants' average win rates in contract dispute cases. Columns (5) and (6) investigate the average win rates of local defendants in non-contract dispute cases. Number of observations changes across columns since there are singletons for certain court semi-year observations (e.g., some local courts do not have any non-contract dispute cases in some semi-years). Panel B reports the results of parallel exercises focusing on cases between local firms. Case-level analysis yields similar outcomes. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table 2: Changes in Judge Incentives after Reform

	Local Defendant's Win Rate	
	(1)	(2)
Post Reform	-0.089***	-0.095***
	(0.028)	(0.030)
Mean of Outcome	0.44	0.44
Court FE	Y	N
Semi-Year FE	Y	Y
Judge FE	N	Y
Provincial Time Trend	Y	Y
Observations	38,875	32,244
R-Squared	0.220	0.501

Notes: This table focuses on the subset of cases that were filed within six months before the local court adopted the reform, and compares the rulings made before and after the reform. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table 3: Changes in Case Compositions after Reform

	External Plaintiff Cases Ratio	Regis. Capital (Million)		# of Employees		Age	
		Plaintiff	Defendant	Plaintiff	Defendant	Plaintiff	Defendant
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post Reform	0.023***	-5.550***	12.125***	-93.789	157.502***	-0.667***	0.476***
	(0.005)	(1.919)	(1.658)	(73.663)	(40.022)	(0.139)	(0.095)
Mean of Outcome	0.36	85.92	79.85	570.94	509.92	9.25	9.92
Court FE	Y	Y	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y	Y	Y
Prov. Time Trend	Y	Y	Y	Y	Y	Y	Y
Observations	45,934	995,053	1,169,873	578,435	733,299	1,181,034	1,443,400
R-Squared	0.581	0.049	0.045	0.067	0.044	0.066	0.063

Notes: This table reports the impacts of the judicial reform on the composition of commercial lawsuits. Column (1) presents the DiD estimate for the share of inter-regional commercial lawsuits. Columns (2), (4), and (6) present the DiD estimates on external plaintiffs' registered capital, number of employees, and firm age. Columns (3), (5), and (7) repeat the same exercises for local defendants. Clustered standard errors at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table 4: Judicial Organizational Reform and Judges' Decision Quality

	Appeal Rate		Evidence Examination		Judicial	Discretionary
	Plaintiff	Defendant	Plaintiff	Defendant	Reasoning	Codes
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reform	-0.012***	-0.001	0.026***	0.003	36.705***	-0.034***
	(0.004)	(0.003)	(0.004)	(0.003)	(7.759)	(0.007)
Mean of Outcome	0.078	0.042	0.105	0.076	490.353	0.304
Court FE	Y	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y	Y	Y
Observations	45,934	45,934	45,934	45,934	45,934	45,934
R-Squared	0.160	0.138	0.149	0.154	0.291	0.276

Notes: This table reports the impacts of judicial organizational reform on the quality of judicial decisions. Columns (1) and (2) present the DiD estimates for appeal rates, for external plaintiff and local defendant, respectively. Columns (3) and (4) present the DiD estimates for courts' approval rates for evidence examination requests by the plaintiff and defendant, respectively. Column (5) presents the DiD estimate for word count in judicial reasoning. Column (6) presents the DiD estimate for the frequency of the judge citing discretionary codes in judicial reasoning. Clustered standard errors at the prefecture level are reported in parentheses. * significant at 10% ** significant at 5% *** significant at 1%.

Table 5: Judicial Organizational Reform, Political Connection, and Inter-Provincial Protectionism

	Non-Connected	Locally Connected	Connected to Prov./Central	Intra-Province	Inter-Province
	(1)	(2)	(3)	(4)	(5)
Post Reform	-0.033***	-0.076***	-0.010	-0.053***	-0.018**
	(0.006)	(0.015)	(0.029)	(0.007)	(0.008)
Mean of Outcome	0.45	0.48	0.55	0.40	0.50
Court FE	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y	Y
Observations	45,648	17,284	8,061	42,554	37,881
R-Squared	0.220	0.258	0.356	0.191	0.209

Notes: Columns (1), (2), and (3) investigate the average win rates of local defendants that are not politically connected, politically connected to county/prefectural governments, and provincial/central governments, respectively. Columns (4) and (5) investigate the local defendants' average win rates in intra- and inter-provincial lawsuits, respectively. Number of observations change across columns since there are singletons for certain court-semiyear observations. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table 6: Judicial Organizational Reform and External Investment

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A Amount of Investments (log)						
	Early-Reform	Late-Reform	All	New Entries	Non-Connected Jnt. Ventures	Connected Jnt. Ventures
Post Reform	0.074	0.133***	0.112***	0.118***	0.091**	-0.148***
	(0.047)	(0.047)	(0.032)	(0.033)	(0.041)	(0.057)
Observations	14,402	23,791	31,373	31,335	30,553	22,293
R-Squared	0.938	0.943	0.944	0.941	0.825	0.504
Panel B Number of Investments (log)						
Post Reform	0.066	0.120**	0.114***	0.118***	0.065*	-0.087***
	(0.044)	(0.050)	(0.038)	(0.037)	(0.034)	(0.022)
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y	Y	Y
Observations	14,402	23,791	31,373	31,335	30,553	22,293
R-Squared	0.917	0.905	0.911	0.911	0.863	0.701

Notes: This table reports the impacts of judicial organizational reform on inward investments at the county level. Panel A focuses on the total amount of investments, while Panel B investigates the total number of investments. Columns (1), (2), and (3) analyze investments in early-reform regions, late-reform regions, and the full sample, respectively. Columns (4), (5), and (6) examine investments in new entries, non-connected joint ventures, and locally connected joint ventures, respectively. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

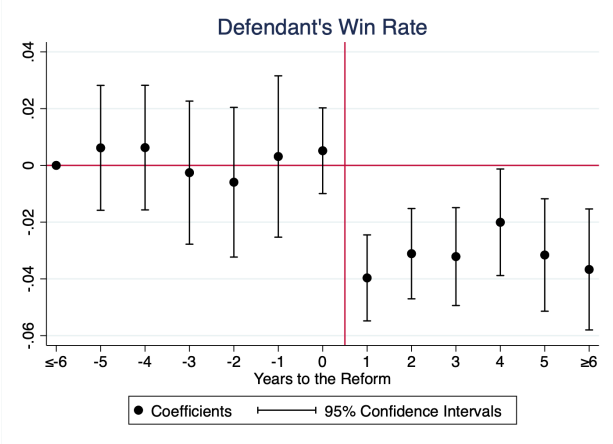
Table 7: Judicial Organizational Reform and External Investment - Other Heterogeneities

	(1)	(2)	(3)	(4)
	Panel A. Amount of Investment (log)			
	Complementary Industries	Same Industry	Tradable	Non-tradable
Post Reform	-0.122***	0.137***	0.067	0.162***
	(0.037)	(0.040)	(0.044)	(0.031)
Observations	30,830	30,377	29,602	30,517
R-Squared	0.877	0.824	0.783	0.897
Equality P-value	0.000		0.010	
	Panel B. Number of Investment (log)			
	Complementary Industries	Same Industry	Tradable	Non-tradable
Post Reform	-0.102***	0.076**	0.041	0.082***
	(0.036)	(0.034)	(0.029)	(0.025)
Observations	30,830	30,377	29,602	30,517
R-Squared	0.885	0.870	0.859	0.920
Equality P-value	0.000		0.000	
County FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y

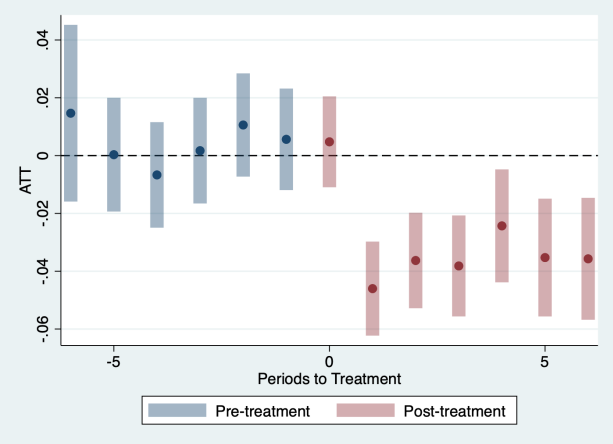
Notes: This table reports the heterogeneous impacts of the reform on different types of investments. Panels A and B focus on the number and total amount of investments, respectively. Column (1) investigates investments in complementary industries — the 5 upstream and downstream industries that are the most strongly connected via input-output table in 2018, while Column (2) shows the results for investments in same industry. Columns (3) and (4) investigate investments in tradable and non-tradable sectors, respectively. P-values of every two coefficients' equality test are reported. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Appendix A APPENDIX

ONLINE APPENDIX



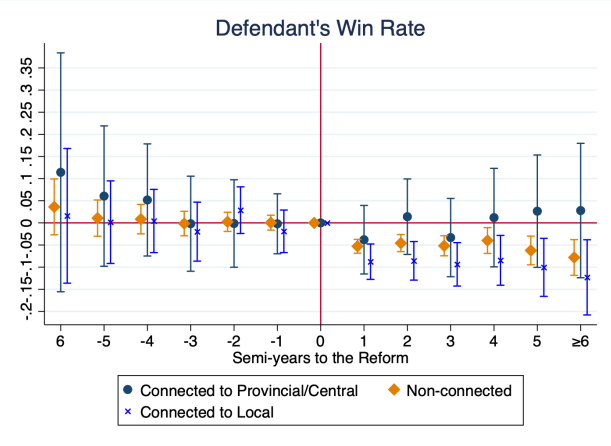
(a) Adjusted following Borusyak et al., 2021



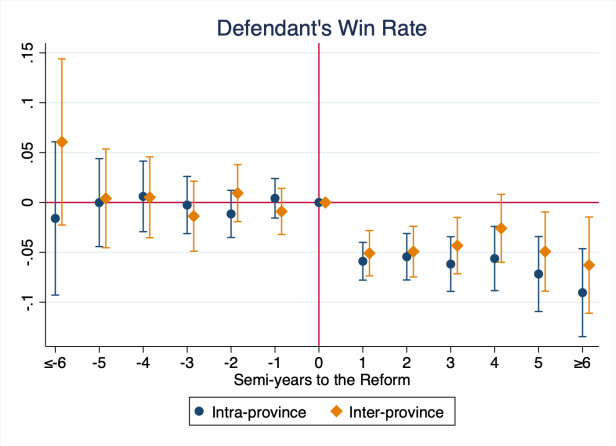
(b) Adjusted following Callaway and Sant'Anna, 2021

Figure A.1: Judicial Organizational Reform and Judicial Impacts - Alternative Estimators

Notes: Panel (a) plots the baseline event study coefficients (as well as 95% confidence intervals), following the approach suggested by Borusyak et al. (2021). Panel (b) plots the baseline event study coefficients (as well as 95% confidence intervals), following the approach suggested by Callaway and Sant'Anna (2021).



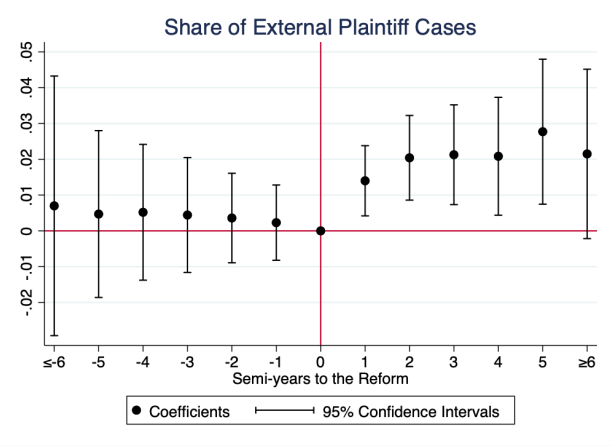
(a) Connected v.s.Non-Connected



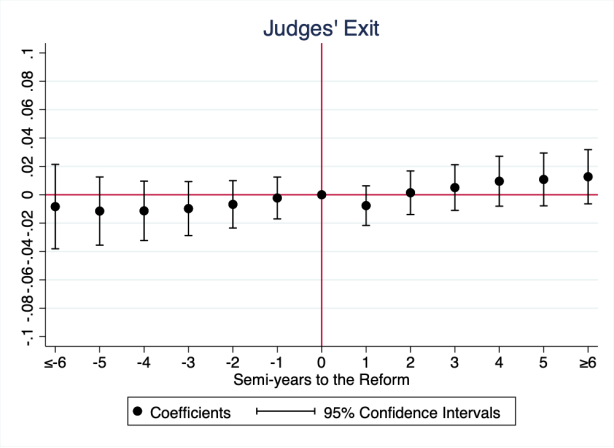
(b) Intra- v.s Inter-province

Figure A.2: Judicial Organizational Reform and Judicial Impacts - Heterogeneity

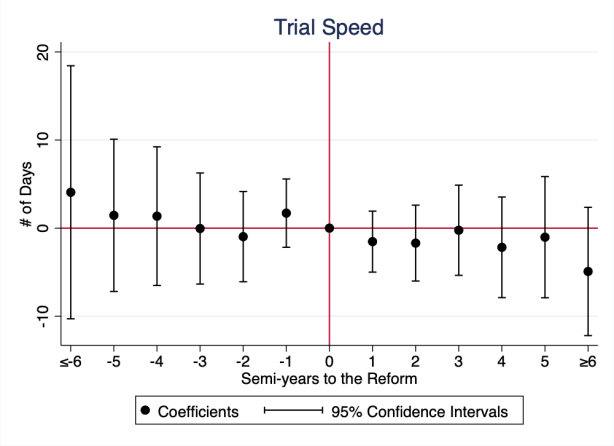
Notes: Panel (a) plots the event study estimates corresponding to Columns (1), (2) and (3) of Table 5, Panel (b) plots the event study estimates corresponding to Columns (4) and (5) of Table 5. All event studies are estimated following the approach suggested by Sun and Abraham (2021).



(a) Judicial Organizational Reform and Share of External Plaintiff v.s. Local Defendant Cases



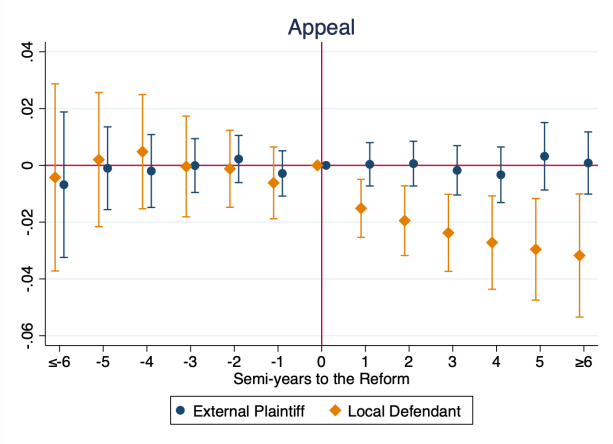
(b) Judicial Organizational Reform and Judges' Exit



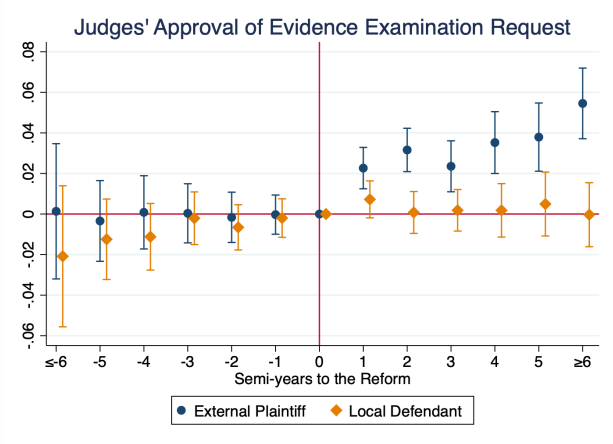
(c) Judicial Organizational Reform and Trial Speed

Figure A.3: Judicial Organizational Reform, Cases Composition, Judge Exit and Trial Efficiency

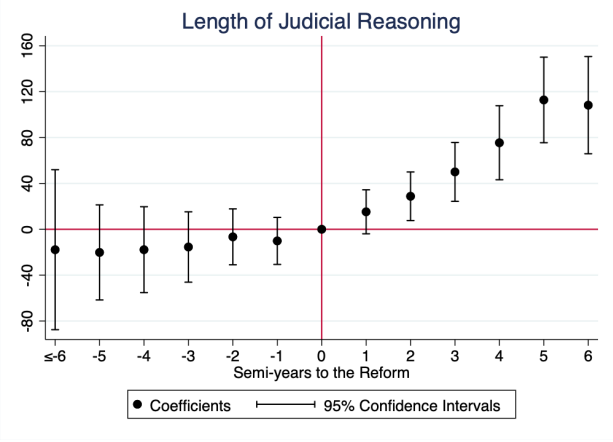
Notes: Panel (a) plots the event study coefficients (as well as 95% confidence intervals) using share of outcome external plaintiff against local defendant cases over all cases between companies as outcome variable. Panel (b) plots the event study coefficients (as well as 95% confidence intervals) using the probability of judges' exit as outcome variable. Panel (c) plots the event study coefficients (as well as 95% confidence intervals) using the duration of a case (number of days from filing to verdict) as outcome variable. All event studies follow the approach suggested by Sun and Abraham (2021).



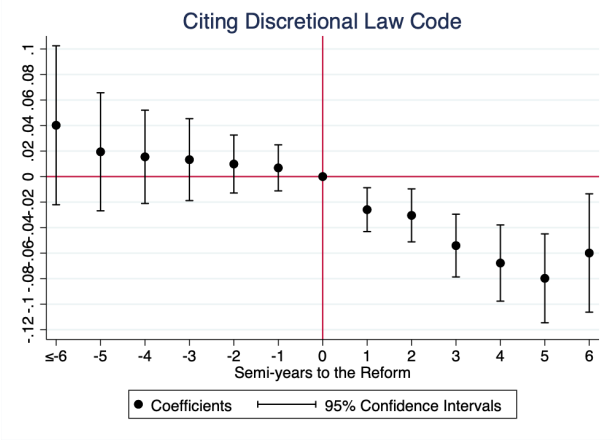
(a) Appeal



(b) Judges' Approval of Evidence Examination

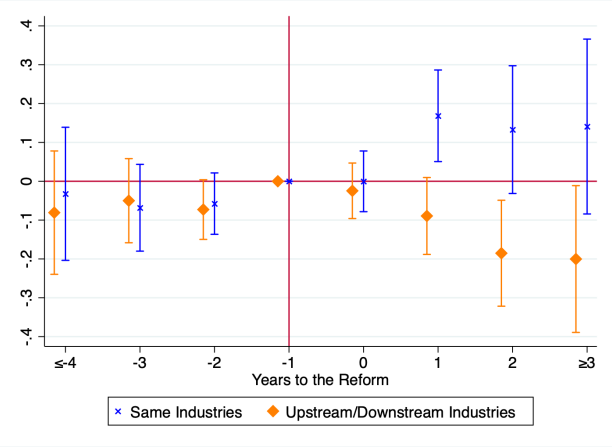


(c) Length of Judicial Reasoning

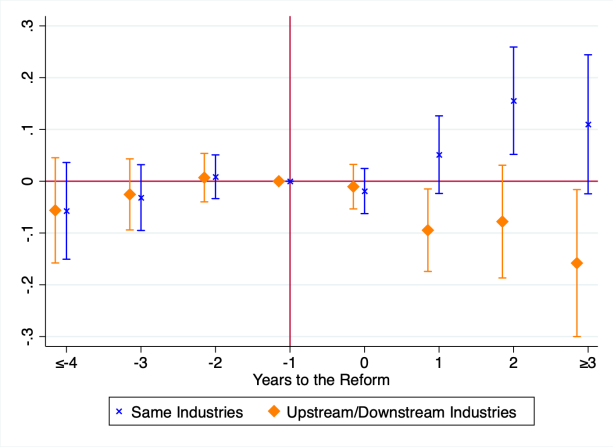


(d) Judge Citing Discretional Law Code

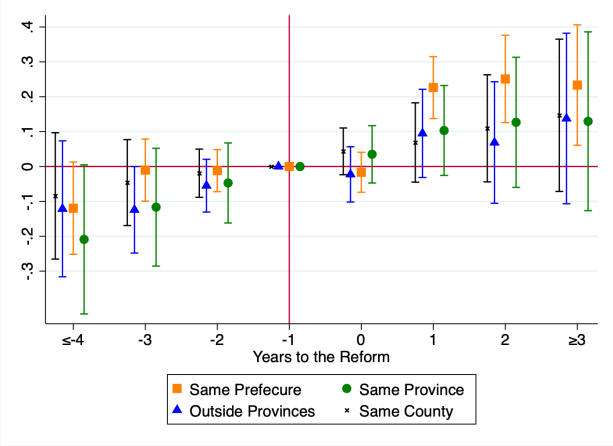
Figure A.4: Notes: In Panel (a), we plot the event study estimates for appeal rate separately for the local defendants and external plaintiffs. In Panel (b), we plot the event study estimates for approval rate for evidence examination request separately for the local defendants and external plaintiffs. In Panel (c), we plot the event study estimates for the word count for judicial reasoning in court verdicts. In Panel (d), we plot the event study estimates for the frequency of citing discretionary codes in judicial reasoning. All event studies are estimated following the approach suggested by Sun and Abraham (2021).



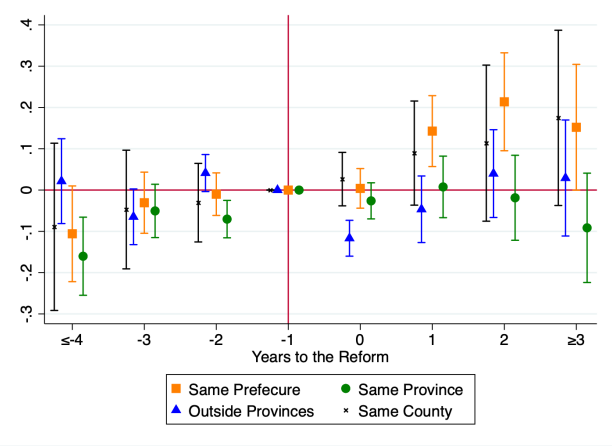
(a) Amount of External Investment



(b) Number of External Investment



(c) Amount of External Investment



(d) Number of External Investments

Figure A.5: Judicial Organizational Reform and Economic Impacts - Heterogeneity

Notes: Panels (a) and (b) plot the event study estimates corresponding to Columns (1) and (2) of Table 7, these two figures plot the event study estimates for non-local firms investing in local firms in the same industries vs. investing in local firms in complementary upstream and downstream industries. Panel (c) plots the event study estimates corresponding to Columns (1) to (4) in Panel A of Table A.19. Panel (d) plots the event study estimates corresponding to Columns (1) to (4) in Panel B of Table A.19. All event studies follow the approach suggested by Sun and Abraham (2021).

Table A.1: Determinants of Reform Adoption: Hazard Model

	Reform Adoption (=1)		
	(1)	(2)	(3)
	Logit	Weibull	Exponetial
# of Local v.s. External Cases	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)
Avg. Win Rate Local Defendant	0.877	-0.719	-1.086
	(1.764)	(0.740)	(0.750)
GDP Per Capita	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Local Gov Budget	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
FDI	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
GDP Share of Non-agricultural Sectors	0.027	-0.228	-0.277
	(2.064)	(1.309)	(1.303)
Local Leader Connected to Provincical	-0.217	-0.090	-0.112
	(0.267)	(0.246)	(0.247)
Observations	980	1,432	1,432
Province FE	YES	YES	YES
Year FE	YES	YES	YES

Notes: The table presents estimated results of the determinants influencing the timing of reform adoption. We employ various hazard models that examine the impact of local judicial, economic, and political factors. The dependent variable in this analysis takes a value of 1 if a prefecture adopted the reform in year t, and 0 otherwise. Once a prefecture adopts the reform, it is dropped from the sample. For judicial factors, we control for the number of commercial lawsuits between local firms and external firms, as well as the average win rate of local defendants in inter-regional civil lawsuits. In terms of economic factors, we include variables such as GDP per capita, the prefectural government's expenditure budget, the amount of FDI, and the share of non-agricultural sectors in GDP. Additionally, we incorporate political factors by including an indicator that reveals whether a local government leader has previously held positions in the provincial government. In Column (1), we estimate the results using logit model. From Columns (2) to (3), we use Weibull hazard model and Exponential hazard model respectively. Standard errors clustered at province level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A.2: Summary Statistics of Key Variables

	Obs.	Mean	Std. Dev.	5th pctl	95th pctl
Panel A. Civil Lawsuits Between External Firms and Local Firms					
Local Defendant's Win Rate	1,171,734	0.45	0.46	0.00	1.00
Politicially Connected Defendant (yes=1)	1,171,734	0.09	0.28	0.00	1.00
Plaintiff Appeal (yes=1)	1,171,734	0.07	0.25	0.00	1.00
Defendant Appeal (yes=1)	1,171,734	0.04	0.18	0.00	0.00
Appr. of Pltf's Evidence Exam. Req. (yes=1)	1,171,734	0.09	0.29	0.00	1.00
Appr. of Dfdt's Evidence Exam. Req. (yes=1)	1,171,734	0.06	0.24	0.00	1.00
Length of Judicial Reasoning	1,171,734	488	528	51.00	1,487
Citing Discretionary Law Code (yes=1)	1,171,734	0.24	0.43	0.00	1.00
Defaulters (yes=1)	1,171,734	0.02	0.15	0.00	0.00
Panel B. Firm litigant Characteristics					
Registered Capital (Million CNY)	2,165,008	82.64	146	1.00	370
Firm Age	2,624,509	9.62	6.40	2.00	22.00
Number of Employees	1,311,849	536	2,801	2.00	1,986
Panel C. Inter-county Investment Flow Data					
Number of External Investment	31,373	725	1,476	16.00	2,473
Amount of External Investment (100 Mill. CNY)	31,373	25.02	88.92	0.01	101

Notes: This table reports summary statistics of key variables. Panel A presents the summary statistics for variables constructed using court verdicts of commercial lawsuits between external firm plaintiffs and local firm defendants that were trialed between 2014 and 2021 and released by the *China Judgements Online* before August 2022. Panel B shows the summary statistics of characteristics of firm litigants' involved in lawsuits in Panel A. We retrieve firm characteristics by matching firm names in the judgements to business registration records from *Tianyancha.com*. Panel C reports the summary statistics for number of external investments and amount of external investment for each county from 2010 to 2020. To construct these two variables, we follow each firm's initial shareholding structure and its subsequant changes, and then aggregating this information at the county-year level using business registration records from *Tianyancha.com*

Table A.3: Judicial Organizational Reform and Missing Rate of Court Verdicts

	Missing Rate	Missing Rate
	(1)	(2)
Post Reform	0.017	0.017
	(0.026)	(0.029)
Mean of Outcome	0.21	0.21
Province FE	Y	Y
Year FE	Y	Y
Observations	217	217
R-Squared	0.817	0.817

Notes: This table reports the impacts of the judicial organizational reform on missing rate of court verdicts. We first calculate the number of civil cases in our database using verdicts that were trialed between 2014 and 2020 and released by the *China Judgment Online* before August, 2022, and then aggregate this information at province-year level. Second, we collect the official statistics using several sources, including provincial statistics yearbooks, the annual work reports of provincial high courts, and news reports from provincial high courts' official websites. Finally, we construct the missing rate for each province in each year using the gap between the number of cases in our dataset and the official statistics. Standard errors are reported below the coefficients. Column (1) reports the results with robust standard errors. Column (2) presents the results with standard errors clustered at province level. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.4: Judicial Organizational Reroform and Government Spending On Court System

	Provincial Gov's Spendings On Court System (log)	Local Gov's Spendings On Court System (log)
	(1)	(2)
Post Reform	0.417***	-1.171***
	(0.138)	(0.238)
Year FE	Y	Y
Province FE	Y	N
County FE	N	Y
Privincial Time Trend	Y	Y
Observations	212	15,451
R-Squared	0.848	0.807

Notes: This table examines the correlation between government spending on the court system and the implementation of judicial organizational reform. Column (1) presents data on expenditure by provincial governments, while Column (2) focuses on spending by prefectural/county governments. Clustered standard errors are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.5: Judicial Organizational Reform and Local Defendants' Win Rate (Case-level Analysis)

	All Cases		Contract Dispute		Non-Contract Disputes	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Post Reform	-0.038***	-0.038***	-0.036***	-0.035***	-0.040**	-0.046**
	(0.009)	(0.010)	(0.008)	(0.009)	(0.015)	(0.019)
Court FE	Y	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	N	Y	N	Y	N	Y
Observations	1,171,694	1,171,694	969,968	969,968	201,563	201,563
R-Squared	0.079	0.082	0.072	0.074	0.135	0.139
Panel B						
Post Reform	-0.031***	-0.025***	-0.030***	-0.024***	-0.035***	-0.037**
	(0.008)	(0.010)	(0.007)	(0.009)	(0.013)	(0.018)
Judge FE	Y	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	N	Y	N	Y	N	Y
Observations	1,146,751	1,146,751	945,081	945,081	179,690	179,690
R-Squared	0.256	0.256	0.248	0.249	0.378	0.379
Mean of Outcome	0.45	0.45	0.44	0.44	0.52	0.52

Notes: This table replicates the Panel A of Table 1 using case-level data. In Panel A, we control for court FE and year-month FE. In Panel B, we replace court FE with a more demanding judge FE. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.6: Judicial Organizational Reform, Local Defendants' Win Rate and Monetary Value

	All Cases	Contract Dispute	Non-Contract Disputes
	(1)	(2)	(3)
Panel A Win Rate Dummy			
Post Reform	-0.175*** (0.007)	-0.173*** (0.008)	-0.178*** (0.014)
Mean of Outcome	0.45	0.44	0.52
Observations R-Squared	43,517 0.233	42,653 0.227	24,017 0.245
Panel B External Plaintiff's Unsupported Monetary Damages Claim (1,000 CNY)			
Post Reform	-37.185*** (9.632)	-35.471*** (9.927)	-28.518** (14.113)
Mean of Outcome	300.78	315.90	222.91
Observations R-Squared	41,057 0.187	39,982 0.187	20,004 0.224
Court FE Semi-Year FE Privincial Time Trend	Y Y Y	Y Y Y	Y Y Y

Notes: This table replicates the Panel A of Table 1. In Panel A, We focus on cases where there is a complete win or a complete loss. In Panel B, we calculate the total amount of damages claimed by external plaintiffs that are ultimately unsupported by court decisions and use it as another proxy for judicial outcomes. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.7: Other Judicial Reforms and Local Defendants' Win Rate

	All Cases		Contract Dispute		Non-Contract Disputes	
	(1)	(2)	(3)	(4)	(5)	(6)
Judicial Organizational Reform	-0.034***	-0.034***	-0.034***	-0.034***	-0.036***	-0.036***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Trans-Regional Jurisdiction Reform	0.002	-0.011	-0.011	0.003	-0.012	-0.012
	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)	(0.012)
Judge Quota Reform	0.012	0.006	0.006	0.005	0.000	0.000
	(0.009)	(0.010)	(0.013)	(0.009)	(0.010)	(0.013)
Circuit Court	-0.009	0.004	0.004	-0.006	0.006	0.006
	(0.009)	(0.009)	(0.017)	(0.009)	(0.009)	(0.017)
Anti-Corruption	0.010	0.001	0.001	0.007	-0.002	-0.002
Mean of Outcome	0.45	0.45	0.44	0.44	0.52	0.52
Court FE	Y	Y	Y	Y	Y	Y
Seimi-year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	N	Y	N	Y	N	Y
Observations	45,934	45,934	45,934	45,210	45,210	45,210
R-Squared	0.213	0.221	0.221	0.207	0.214	0.214

Notes: This table investigates the effects of judicial organization reform, along with other legal reforms and policy shocks, on judicial outcomes in interregional commercial lawsuits, using data aggregated at the court-semiyear level. Column (1) and (2) focuses on the average win rates of all local defendants in all inter-regional commercial lawsuits. Columns (3) and (4) investigate the local defendants' average win rates in contract dispute cases, respectively. Columns (5) and (6) investigate the average win rates of local defendants in non-contract dispute cases. Number of observations changes across columns since there are singletons for certain court-semiyear observations (e.g., some local courts do not have any non-contract dispute cases in some semiyears). Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.8: Judicial Organizational Reform and Local Defendants' Win Rate (Semi-parametric DiD)

	All Cases	Contract Dispute	Non-Contract Disputes
	(1)	(2)	(3)
Post Reform	-0.036	-0.036	-0.055
	(0.008)***	(0.008)***	(0.013)***
Mean of Outcome	0.44	0.44	0.52
Court FE	Y	Y	Y
Semi-Year FE	Y	Y	Y
Provincial Time Trend	Y	Y	Y

Notes: This table replicates the Panel A of Table 1 by estimating the two-way fixed effect model (1) following the approach suggested by Callaway and Sant'Anna, 2021. Standard errors clustered at the prefecture level are reported in parentheses. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.9: Judicial Organizational Reform, Judicial Impacts, and Economic Impacts

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Judicial Impacts						
	Defendants' Win Rate (External Plaintiffs v.s Local Defendants)			Defendants' Win Rate (Local Plaintiffs v.s Local Defendants)		
	All Cases	Contract Disputes	Non-Contract Disputes	All Cases	Contract Disputes	Non-Contract Disputes
Post Reform	-0.035 [-0.047, -0.023]	-0.036 [-0.050, -0.024]	-0.037 [-0.065, -0.012]	0.006 [-0.022, 0.030]	0.005 [-0.026, 0.031]	0.004 [-0.018, 0.033]
Mean of Outcome	0.45	0.44	0.52	0.44	0.44	0.44
Court FE	Y	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y	Y	Y
Observations	45,934	45,210	28,331	49,086	49,086	47,559
Panel B. Economic Impacts						
	Amount of Investment (log)			Number of Investment (log)		
	Early-Reform	Late-Reform	All	Early-Reform	Late-Reform	All
Post Reform	0.074 [-0.101, 0.188]	0.133 [0.007, 0.264]	0.112 [0.022, 0.204]	0.066 [-0.162, 0.206]	0.120 [-0.043, 0.271]	0.114 [0.002, 0.227]
Mean of Outcome	10.409	10.650	10.592	5.618	5.761	5.726
County FE	Y	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y	Y	Y
Observations	14,402	23,791	31,373	14,402	23,791	31,373

Notes: The Panel A of this table replicates Table 1 using alternative Wild bootstrap standard errors clustered at province level. The Panel B of this table replicates Table 6 using alternative Wild bootstrap standard errors clustered at province level. Province clustered Wild bootstrap confidence intervals at 95% level are reported in square brackets.

Table A.10: Intensive Margin - Changes of Plaintiffs and Defendants in Cases Received Rulings Before and After Reform

	Regis. Capital (Million CNY)		# of Employees		Age	
	Plaintiff	Defendant	Plaintiff	Defendant	Plaintiff	Defendant
	(1)	(2)	(3)	(4)	(5)	(6)
Received Rulings After Reform	13.426	8.395	7.342	-144.363	0.134	8.395
	(14.451)	(7.459)	(65.787)	(118.364)	(0.367)	(7.596)
Mean of Outcome	84.58	84.93	495.45	648.41	10.70	10.55
Court FE	Y	Y	Y	Y	Y	Y
Semi-Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y	Y	Y
Observations	23,193	32,256	21,002	22,760	31,981	32,256
R-Squared	0.159	0.137	0.392	0.181	0.144	0.137

Notes: This table test the changes in characteristics of the plaintiffs and defendants in cases that received rulings before and after the reform. Columns (1), (3), and (5) present the DiD estimates on external plaintiffs' registered capital, number of employees, and firm age. Columns (2), (4), and (6) repeat the same exercises for local defendants. Clustered standard errors at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.11: Judicial Organizational Reform and Appeal Rate When Losing

	Appeal Rate When Losing (=1)	
	Plaintiff	Defendant
	(1)	(2)
Post Reform	-0.004*	-0.002
	(0.002)	(0.001)
Mean of Outcome	0.04	0.03
Court FE	Y	Y
Semi-Year FE	Y	Y
Provincial Time Trend	Y	Y
Observations	45,934	45,934
R-Squared	0.113	0.109

Notes: This table reports the baseline DiD estimates on appeal rate in inter-regional commercial lawsuits, with data aggregated to court-semiyear level. Column (1) focuses on external plaintiffs' appeal rate conditional on they're losing the case (i.e. plaintiffs' win rate is smaller than 0.5). Column (2) investigates local defendants' appeal rate conditional on they're losing the case (i.e. defendants' win rate is smaller than 0.5). Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.12: Judicial Organizational Reform and Local Defendants Win Rate- Heterogeneity

	Non-SOEs	Local SOEs	Provincial/Central SOEs
	(1)	(2)	(3)
Post Reform	-0.034***	-0.051***	-0.015
	(0.006)	(0.014)	(0.025)
Mean of Outcome	0.44	0.46	0.54
Court FE	Y	Y	Y
Semi-Year FE	Y	Y	Y
Provincial Time Trend	Y	Y	Y
Observations	45,241	20,191	9,742
R-Squared	0.218	0.253	0.346

Notes: This table reports the baseline DiD estimates on judicial outcomes in inter-regional commercial lawsuits, with data aggregated to court-semiyear level. Column (1) focuses on the average win rates of local defendants that are non-SOEs. Column (2) investigate the average win rates of SOE defendants that are owned by county/prefecture governments or are connected to these SOEs within 3 steps in equity network. Column (3) investigate the average win rates of SOE defendants that are owned by provin-cial/central governments or are connected to these SOEs within 3 steps in equity network. Number of observations change across columns since there are singletons for certain court-semiyear observations (e.g., some local courts do not have any SOE defendants in some semiyears). Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.13: Judicial Organizational Reform, Provincial Capture, and Local Defendants Win Rate

	Win Rate	
	All	Contract Disputes
	(1)	(2)
Post Reform × Prov Capture	0.026*	0.024*
	(0.014)	(0.014)
Post Reform	-0.056***	-0.054***
	(0.016)	(0.016)
Mean of Outcome	0.45	0.44
Court FE	Y	Y
Semi-Year FE	Y	Y
Observations	45,934	45,210
R-Squared	0.213	0.208

Notes: This table explores the interaction between judicial organizational reform and the local government's ability to lobby the provincial government. We proxy a local government's ability to lobby the provincial government by whether the local government leader (party secretary and mayor) has served in the provincial government earlier in his/her career. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.14: Judge's Promotion and Judicial Organizational Reform

	Promotion (=1)	
	(1)	(2)
Biased Judge × Post	-0.018***	
	(0.006)	
Biased Judge	0.018***	
	(0.004)	
Low Ability Judge × Post		0.014**
		(0.007)
Low Ability Judge		-0.038***
		(0.004)
Post	0.017***	-0.001
	(0.005)	(0.006)
Mean of Outcome	0.13	0.13
Judge Chararcteristics	Y	Y
Court FE	Y	Y
Year FE	Y	Y
Provincial Time Trend	Y	Y
Observations	190,609	190,609
R-Squared	0.079	0.080

Notes: This table explores the relationship between judge's promotion and judicial organizational reform. In column (1), we utilize the pre-reform sample to estimate judge fixed effects on the win rate of local defendants against external plaintiffs. We consider a judge as more biased if the coefficient of the fixed effect is higher than the mean. In column (2), we measure judges' ability using the number of cases with economic values exceeding the 95th percentile that a judge handles per year. A judge is considered to have low ability if the number of high-stake cases is below the mean. Judge characteristics include the total number of cases handled and the case type compositions. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.15: Court Budget and Judicial Organizational Reform

	Courts' Budget Per Judge (10,000 CNY)		
	Before Reform	After Reform	No Reform
	(1)	(2)	(3)
Courts' Bias	11.879***	-0.251	12.870**
	(3.070)	(2.040)	(6.461)
Court Chararcteristics	Y	Y	Y
Year FE	Y	Y	Y
Provincial Time Trend	Y	Y	Y
Observations	2,538	3,817	2,290
R-Squared	0.565	0.827	0.389

Notes: This table explores the relationship between courts' budget and judicial organizational reform. We utilize the pre-reform sample to estimate court fixed effects on the win rate of local defendants against external plaintiffs for courts experienced the reform. For courts didn't adopt the reform during the study period, we use all sample to estimate court fixed effects. We consider a court as more biased if the coefficient of the fixed effect is higher than the mean. Court Chararcteristics include the number of court cases, appeal rate, and the case compositions. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.16: Judicial Organizational Reform and Ruling Enforcement

	Non-compliance Rate	Complete	Partial
	(1)	(2)	(3)
Post Reform	0.003	0.002	0.001
	(0.002)	(0.002)	(0.001)
Mean of Outcome	0.023	0.017	0.005
Court FE	Y	Y	Y
Semi-Year FE	Y	Y	Y
Provincial Time Trend	Y	Y	Y
Observations	45,934	45,913	45,862
R-Squared	0.181	0.165	0.108

Notes: This table reports the impacts of the judicial organizational reform on judicial enforcement. Columns (1) presents the DiD estimates for all types of "non-compliance". Columns (2) and (3) present the DiD estimates for “complete non-compliance" and “partial non-compliance" respectively. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.17: Judicial Organizational Reform, Firm's Awareness, and Investment Behavior

	Investing Outside Home County (=1)		
	(1)	(2)	(3)
	All	100% Win	100% Lose
With Lawsuit Post-Reform	0.034***	0.039***	0.030***
	(0.002)	(0.003)	(0.004)
Mean of Outcome	0.14	0.14	0.14
Controls	Y	Y	Y
Home Prefecture FE	Y	Y	Y
Observations	203,386	81,820	59,707
R-Squared	0.010	0.014	0.014

Notes: This table presents the correlation between firms that have encountered lawsuits following the reform and their investments made outside their home county. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.18: Judicial Organizational Reform, Investors' Legal Dependence and Investment

	(1)	(2)	(3)	(4)
	Amount of Investment (log)		Number of Investment (log)	
	More dependent	Less dependent	More dependent	Less dependent
Post Reform	0.224***	0.075**	0.139***	0.094**
	(0.035)	(0.033)	(0.037)	(0.039)
Observations	31,084	31,147	31,084	31,147
R-Squared	0.912	0.928	0.895	0.886
County FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y

Notes: This table reports the heterogeneous impacts of the reform with respect to external investors' industrial sectors. We employ a data-driven approach to designate industrial sectors with a higher percentage of firms engaged in commercial lawsuits as legally more demanding industries (e.g., finance, real estate, leasing services), and conversely, those with lower involvement as legally less demanding industries (e.g., retail, agriculture, mining). Columns (1) and (2) investigate the amount of investments made by external investors in legally more demanding industries and made by investors from legally less demanding industries, respectively. Columns (3) and (4) focus on the number of external investments. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.19: Judicial Organizational Reform and External Investment - Spillover

	(1)	(2)	(3)	(4)
Panel A				
	Amount of Investment (log)			
	Same Prefecture	Outside Prefecture	Outside Province	Same County
Post Reform	0.194***	0.073	0.063	0.034
	(0.034)	(0.047)	(0.041)	(0.041)
Mean of Outcome	10.140	8.950	9.386	12.640
Observations	30,178	29,956	31,183	32,313
R-Squared	0.893	0.768	0.886	0.880
Panel B				
	Number of Investment (log)			
	Same Prefecture	Outside Prefecture	Outside Province	Same County
Post Reform	0.128***	0.012	0.047	0.057
	(0.039)	(0.035)	(0.035)	(0.055)
Mean of Outcome	5.416	3.8048	4.379	7.159
Observations	30,178	29,956	31,183	32,313
R-Squared	0.890	0.869	0.888	0.909
County FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y

Notes: This table explores the spillover effects of the judicial reform on different types of investments. Panel A shows results for the total amount of investments, Panel B shows results for the number of investments. Column (1) presents the DiD estimates for inter-county investments within the same prefecture, Column (2) presents the DiD estimates for inter-prefectural investments within the same province, Column (3) presents the DiD estimates for inter-provincial investments, Column (4) presents the DiD estimates for intra-county investments. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.20: Judicial Organizational Reform and Local Investment - Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
	Amount of Investment (log)					
	New Firms	Existing Firms	Complementary Industries	Same Industry	Tradable	Non-tradable
Post Reform	0.040	0.064	0.061	0.065	0.036	0.054
	(0.041)	(0.043)	(0.049)	(0.041)	(0.056)	(0.042)
Observations	32,307	31,983	31,851	32,292	31,525	31,849
R-Squared	0.879	0.801	0.793	0.882	0.748	0.866
Panel B						
	Number of Investment (log)					
	New Firms	Existing Firms	Complementary Industries	Same Industry	Tradable	Non-tradable
Post Reform	0.058	0.072	0.046	0.069	0.054	0.040
	(0.055)	(0.054)	(0.045)	(0.053)	(0.062)	(0.034)
Observations	32,307	31,983	31,851	32,292	31,525	31,849
R-Squared	0.908	0.893	0.906	0.912	0.875	0.932
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Provincial Time Trend	Y	Y	Y	Y	Y	Y

Notes: This table reports the heterogeneous impacts of the reform on different types of local investments. Panels A and B focus on the number and total amount of investments, respectively. Columns (1) and (2) investigate investments in new entries and joint ventures, respectively. Column (3) investigate investments in complementary industries-the 5 upstream and 5 downstream industries that are the most strongly connected via input-output table in 2018, while Column (4) shows the results for investments in same industry. Columns (5) and (6) investigate investments in tradable and non-tradable sectors, respectively. Standard errors clustered at the prefecture level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Appendix B Model Appendix

Appendix B.1 Derivations of Results in the Main Text

We first solve for the equilibrium.

Pricing. Given consumer demand (11), all firms charge a constant markup $\frac{\epsilon}{\epsilon-1}$. Equilibrium prices, quantities, and variable profits follow

$$p(\varphi) = \frac{\epsilon}{\epsilon-1}\tau\varphi, \qquad q(\varphi) = \left(\frac{\epsilon}{\epsilon-1}\tau\varphi\right)^{-\epsilon}, \qquad \pi(\varphi) = \frac{\left(\frac{\epsilon}{\epsilon-1}\tau\varphi\right)^{1-\epsilon}}{\epsilon}.$$

Entry. A firm with cost index φ chooses to enter iff the variable profit $\pi(\varphi)$ exceeds the fixed cost of entry f. All firms with cost indices below $\bar{\varphi} \equiv \frac{\epsilon-1}{\epsilon}(\epsilon f)^{1/(1-\epsilon)}/\tau$ will enter. For notational simplicity, let $\kappa \equiv \frac{\epsilon-1}{\epsilon}(\epsilon f)^{1/(1-\epsilon)}$.

Expected Net Profit. The expected profit net of entry cost by a firm choosing location i is

$$\begin{aligned}
\Pi &= \int_0^{\bar{\varphi}} \frac{\left(\frac{\epsilon}{\epsilon-1}\tau\varphi\right)^{1-\epsilon}}{\epsilon} - f\,\mathrm{d}F(\varphi) \\
&= \int_0^{\kappa/\tau} \gamma\epsilon^{-\epsilon}(\epsilon-1)^{\epsilon-1}\left(\tau^{1-\epsilon}\varphi^{\gamma-\epsilon} - \kappa^{1-\epsilon}\varphi^{\gamma-1}\right)\mathrm{d}\varphi \qquad (8) \\
&= \frac{\epsilon^{-\epsilon}(\epsilon-1)^{\epsilon}}{\gamma-\epsilon+1}\tau^{-\gamma}\kappa^{\gamma-\epsilon+1} \qquad (9)
\end{aligned}$$

Consumer Surplus. The consumer surplus derived from each variety is

$$u^*(\varphi) \equiv u(\varphi) - p(\varphi)q(\varphi) = \frac{\left(\frac{\epsilon}{\epsilon-1}\tau\varphi\right)^{1-\epsilon}}{\epsilon-1}.$$

The total consumer surplus derived from all nonlocal firms is

$$\begin{aligned}
U &= \int_0^{\bar{\varphi}} u(\varphi) - p(\varphi)q(\varphi)\,\mathrm{d}F(\varphi) \\
&= \left(\frac{\epsilon}{\epsilon-1}\right)^{1-\epsilon}\int_0^{\kappa/\tau}\frac{(\tau\varphi)^{1-\epsilon}}{\epsilon-1}\gamma\varphi^{\gamma-1}\,\mathrm{d}\varphi
\end{aligned}$$

$$= \frac{\left(\frac{\epsilon}{\epsilon-1}\right)^{1-\epsilon}\gamma\kappa^{\gamma-\epsilon+1}}{(\epsilon-1)(\gamma-\epsilon+1)}\tau^{-\gamma}$$

Total Revenue. The total revenue of nonlocal firms is

$$\begin{aligned} R &\equiv \left(\frac{\epsilon}{\epsilon-1}\right)^{1-\epsilon}\int_0^{\bar{\varphi}}(\tau\varphi)^{1-\epsilon}\,\mathrm{d}F(\varphi) \\ &= \frac{\left(\frac{\epsilon}{\epsilon-1}\right)^{1-\epsilon}\gamma\kappa^{\gamma-\epsilon+1}}{(\gamma-\epsilon+1)}\tau^{-\gamma} \end{aligned}$$

Judicial Reform. The response of consumer surplus to a change in τ is

$$\begin{aligned} \frac{\mathrm{d}\ln U}{\mathrm{d}\ln\tau} &= \frac{1}{U}\left(\int_0^{\bar{\varphi}}\frac{\mathrm{d}u(\varphi)}{\mathrm{d}\ln\tau}\mathrm{d}F(\varphi)+\frac{\mathrm{d}\int_0^{\bar{\varphi}}u(\varphi)\,\mathrm{d}F(\varphi)}{\mathrm{d}\bar{\varphi}}\frac{\mathrm{d}\bar{\varphi}}{\mathrm{d}\ln\tau}\right) \\ &= \frac{1}{U}\left((1-\epsilon)\int_0^{\bar{\varphi}}u(\varphi)\,\mathrm{d}F(\varphi)-\frac{\left(\frac{\epsilon}{\epsilon-1}\tau\right)^{1-\epsilon}}{\epsilon-1}\gamma\bar{\varphi}^{\gamma-\epsilon+1}\right) \\ &= -(\epsilon-1)-(\gamma-\epsilon+1) \\ &= -\gamma \end{aligned}$$

The response of producer surplus is

$$\begin{aligned} \frac{\mathrm{d}\ln\Pi}{\mathrm{d}\ln\tau} &= \frac{1}{\Pi}\left(\int_0^{\bar{\varphi}}\frac{\mathrm{d}\frac{\left(\frac{\epsilon}{\epsilon-1}\tau\varphi\right)^{1-\epsilon}}{\epsilon}}{\mathrm{d}\ln\tau}\mathrm{d}F(\varphi)+\frac{\mathrm{d}\int_0^{\bar{\varphi}}\frac{\left(\frac{\epsilon}{\epsilon-1}\tau\varphi\right)^{1-\epsilon}}{\epsilon}-f\,\mathrm{d}F(\varphi)}{\mathrm{d}\bar{\varphi}}\frac{\mathrm{d}\bar{\varphi}}{\mathrm{d}\ln\tau}\right) \\ &= \frac{1}{\Pi}\left((1-\epsilon)\int_0^{\bar{\varphi}}\left(\frac{\epsilon}{\epsilon-1}\tau\varphi\right)^{1-\epsilon}\mathrm{d}F(\varphi)-\left(\frac{\left(\frac{\epsilon}{\epsilon-1}\tau\bar{\varphi}\right)^{1-\epsilon}}{\epsilon}-f\right)\gamma\bar{\varphi}^{\gamma}\right) \\ &= \frac{1}{\Pi}\left((1-\epsilon)\Pi+(1-\epsilon+\gamma)f\bar{\varphi}^{\gamma}-\frac{\left(\frac{\epsilon}{\epsilon-1}\tau\right)^{1-\epsilon}}{\epsilon}\gamma\bar{\varphi}^{\gamma-\epsilon+1}\right) \\ &= -(\epsilon-1)-(\gamma-\epsilon+1) \\ &= -\gamma \end{aligned}$$

Because the revenue from each variety is proportional to the consumer surplus $u^*(\varphi)$, we know $\frac{\mathrm{d}\ln R}{\mathrm{d}\ln\tau}$ has the same decomposition as $\frac{\mathrm{d}\ln U}{\mathrm{d}\ln\tau}$.

Finally, the response of the mass of entrants $\mu \equiv F(\bar{\varphi})$ is

$$\frac{\mathrm{d}\ln\mu}{\mathrm{d}\ln\tau} = \frac{\mathrm{d}\ln\bar{\varphi}^{\gamma}}{\mathrm{d}\ln\tau}$$

$$
\begin{aligned}
&= \frac{\mathrm{d}\ln\left(\frac{\epsilon-1}{\epsilon}\left(\epsilon f\right)^{1/(1-\epsilon)}/\tau\right)^{\gamma}}{\mathrm{d}\ln\tau} \\
&= -\gamma,
\end{aligned}
$$

thereby proving Proposition 1.

Appendix B.2 Model Extensions: Endogenous Location Choice

In this appendix, we extend the baseline model model in the main text to incorporate entrepreneur's endogenous location choice. We discuss how our reduced-form evidence show this margin not to be empirically relevant, that there is little substitution of investments from control to treated locations affected by the judicial reform or from investing locally to externally.

Consider an economy with N locations. A unit mass of nonlocal entrepreneurs can choose a location to enter and serve the local consumers. The consumer in each location has separable preferences over products from nonlocal firms:

$$
U_n = \int_{\varphi\in\Phi_n} u\left(q_n\left(\varphi\right)\right) - p_n\left(\varphi\right) q_n\left(\varphi\right)\, \mathrm{d}F\left(\varphi\right), \tag{10}
$$

where Φ_n is the set of nonlocal entrepreneurs (index by φ) that serve location n, $u\left(q\right) \equiv \frac{\epsilon}{\epsilon-1} q^{\frac{\epsilon-1}{\epsilon}}$ is utility derived from each firm φ. The consumer preferences (10) imply the following demand function for each firm:

$$
q^*\left(p\right) = \arg\max_{q}\left\{u\left(q\right) - pq\right\} = p^{-\epsilon}. \tag{11}
$$

Firms make location, entry, and pricing decisions. First, each nonlocal entrepreneur decides on a target location n based on expected profitability $\bar{\pi}_n$ and idiosyncratic preferences $\left\{\xi_n\right\}_n$. The entrepreneur then draws a cost index $\varphi \leq 1$ from distrubtion $F\left(\varphi\right) = \varphi^{\gamma}$ and decides whether to pay the fixed entry cost f to produce in location n with marginal cost $c_n\left(\varphi\right)$. After entry, firms engage in monopolistic pricing, choosing prices that maximizes variable profits.

Formally, an entrepreneur with preferences $\left\{\xi_n\right\}$ first chooses the target location that delivers the highest expected profit net of entry costs:

$$
\max_n \xi_n \bar{\pi}_n, \qquad \bar{\pi}_n \equiv \int_0^1 \max\left\{\pi_n\left(\varphi\right) - f, 0\right\}\, \mathrm{d}F\left(\varphi\right), \tag{12}
$$

where the maximization inside the integral of (12) indicates entry decision after drawing the cost index φ, and $\pi_n(\varphi)$ is the variable profits:

$$\pi_n(\varphi) \equiv \max_p (p - c_n(\varphi)) q^*(p). \tag{13}$$

Equilibrium price $p_n(\varphi)$ is the maximizer of (13).

Define $\bar{\varphi}$ as the cost index for which $\pi_n(\bar{\varphi}) = f$. Because of the fixed entry cost f, only entrepreneurs with sufficiently costs ($\varphi \leq \bar{\varphi}$) will enter.

We parametrize the marginal cost as $c_n(\varphi) \equiv \tau_n \varphi$, where $\tau_n \geq 1$ is a location-specific marginal cost shifter that depends on judicial fairness; a more locally biased justice system in location n raises the cost of production through higher τ_n.

We parametrize the idiosyncratic locational preferences ξ_n of entrepreneurs as being drawn independently from the Fréchet distribution (equivalent to $\ln \xi_n$ drawn from Gumbel):

$$G_n(\xi) = e^{-z_n \xi^{-\theta}},$$

where the Fréchet scale parameter (z_n) controls the average preference for target location n, which depend for example on the physical, cultural, or political factors in n. The Fréchet shape parameter θ controls the dispersion of prospects and regulates the sensitivity of location choice to economic variables (in particular the expected profits) relative to idiosyncratic factors. Specifically, let ω_n denote the share of nonlocal entrepreneurs choosing location n. The Fréchet distribution of idiosyncratic shocks imply a constant elasticity of substitution in the location choice shares with respect to relative ex-ante net profits $\frac{\mathrm{d}\ln(\omega_n/\omega_m)}{\mathrm{d}\ln(\bar{\pi}_n/\bar{\pi}_m)} = \theta$.

Given entrepreneurial preferences $\{\xi_n\}$ and the degree of local protection $\{\tau_n\}$, an equilibrium is the collection of firms' location choices $\{\omega_n\}$, entry decisions, prices $\{p_n(\varphi)\}$, quantities $\{q_n(\varphi)\}$, and variable profits $\{\pi_n(\varphi)\}$, such that a firm chooses location n iff n is the maximizer of (12) and enters iff $\pi_n(\varphi) \geq f$, $\pi_n(\varphi)$ solves (13), prices are the maximizers of (13), and quantities are consistent with the consumer demand function $q_n(\varphi) = q^*(p_n(\varphi))$.

Relative to the model in the main text, a judicial reform that reduces local protection τ_n and can now affect consumer and producer surplus through an additional channel: a reform in location n raises

the ex-ante net profits $\bar{\pi}_n$ in that location, thereby attracting other nonlocal firms to choose location n and substitute away from other locations.

The response of consumer surplus (as in equation 10) to judicial reform (a decline in τ_n) can be decomposed as

$$\begin{aligned} -\frac{\mathrm{d}\ln U_n}{\mathrm{d}\ln \tau_n} &= \frac{-1}{\int_0^{\bar{\varphi}} u_n(\varphi) F(\varphi)} \left(\underbrace{\int_0^{\bar{\varphi}} \frac{\mathrm{d}u_n(\varphi)}{\mathrm{d}\ln \tau_n} \mathrm{d}F(\varphi)}_{\substack{\text{lower production} \\ \text{costs among entrants}}} + \underbrace{\frac{\mathrm{d}\int_0^{\bar{\varphi}} u_n(\varphi)\, \mathrm{d}F(\varphi)}{\mathrm{d}\bar{\varphi}} \frac{\mathrm{d}\bar{\varphi}}{\mathrm{d}\ln \tau_n}}_{\substack{\text{entry of marginal firms given} \\ \text{a higher cutoff cost index}}} \right) - \underbrace{\frac{\mathrm{d}\ln \omega_n}{\mathrm{d}\ln \tau_n}}_{\substack{\text{more entrepreneurs} \\ \text{choose location } n}} \quad (14) \\ &= \underbrace{(\epsilon - 1)}_{\substack{\text{lower production} \\ \text{costs among entrants}}} + \underbrace{(\gamma - \epsilon + 1)}_{\text{new entrants}} + \underbrace{\theta\gamma(1-\omega_n)}_{\substack{\text{more entrepreneurs} \\ \text{choose location } n}} \\ &= \gamma(1 + \theta(1-\omega_n)), \end{aligned}$$

where $u_n(\varphi)$ is the equilibrium consumer surplus derived from a nonlocal firm with cost index φ (the maximand of 11).

Besides the two key elasticities (ϵ and γ) in the main text, the shape parameter θ in entrepreneur's preference distribution serves as the elasticity of substitution in entrepreneurs' location choice in response to higher expected net profits $\bar{\pi}_n$ after the reform.

We make two conceptual points using this model extension. First, we can still use the empirical measure of how the number of nonlocal firms operating in location n changes after the reform to assess the impact of the judicial reform on consumer and producer surplus and overall economic activity. Formally, let $\mu_n \equiv \omega_n F(\bar{\varphi}_n)$ denote the mass of nonlocal firms that enter location n; $\Pi_n \equiv \omega_n \bar{\pi}_n$ is the total net profits in location n; $R_n \equiv \int_{\varphi \in \Phi_n} p_n(\varphi) q_n(\varphi)\, \mathrm{d}F(\varphi)$ is the total revenue in location n. It can be shown that, just as in the baseline model in the main text, $\mathrm{d}\ln \mu_n$ is a sufficient statistic for $\mathrm{d}\ln U_n$, $\mathrm{d}\ln \Pi_n$, and $\mathrm{d}\ln R_n$:

$$\frac{\mathrm{d}\ln \mu_n}{\mathrm{d}\ln \tau_n} = \frac{\mathrm{d}\ln U_n}{\mathrm{d}\ln \tau_n} = \frac{\mathrm{d}\ln \Pi_n}{\mathrm{d}\ln \tau_n} = \frac{\mathrm{d}\ln R_n}{\mathrm{d}\ln \tau_n}. \quad (15)$$

Second, we comment on interpreting the difference-in-difference estimator, which compares the before-after changes in the number of outside investors to a location n that has gone through a judicial

reform, to a location m, which did not experience a reform ($\beta^{DiD} = \mathrm{d}\ln\mu_n - \mathrm{d}\ln\mu_m$). A standard drawback of the DiD estimator is that, because the reform in location n may attract potential entrants to substitute away from m towards n ($\mathrm{d}\ln\mu_m \neq 0$), the DiD estimator does not recover $\mathrm{d}\ln\mu_n$.

Our extended model provides guidance on how to interpret β^{DiD}. Specifically, the degree of substitution across locations by potential entrepreneurs can be expressed as $\frac{\mathrm{d}\ln\mu_m}{\mathrm{d}\ln\tau_n} = \gamma\theta\omega_n$, where θ is the elasticity of substitution across locations, and ω_n is the pre-reform mass of entrepreneurs who choose location n. Hence, the bias in the DiD estimator is

$$\text{bias} \equiv \frac{\beta^{DiD} - \mathrm{d}\ln\mu_n}{\beta^{DiD}} = \frac{\theta}{1+\theta}\omega_n.$$

When $\theta = 0$, there is no substitution across locations, and the bias is zero.

The judicial reforms that we exploit are rolled-out at the prefecture level; there is no within-prefecture, cross-county variation in the roll-out. The evidence in Table A.19, columns (1)–(3) shows that relative to counties in non-treated prefectures, counties in treated prefectures experienced (1) significantly more external investments from external counties within the prefecture; (2) no more investments from external prefectures. This is evidence for $\theta \approx 0$, meaning the increase in economic integration in prefectures that has undergone the reform is mainly driven by net creation of new investments across counties within the treated prefecture, and not by the substitution of investments away from non-treated to treated prefectures.

Along the same lines, column (4) of Table A.19 shows that treated prefectures experience no statistically different number of local, within-county investments relative to non-treated prefectures. This shows that the investment response we find corresponds to net creation of new investments across counties, and there is little evidence of substitution from investing locally to nonlocally within treated prefectures.

SUPPLEMENTARY MATERIAL (NOT FOR PUBLICATION)

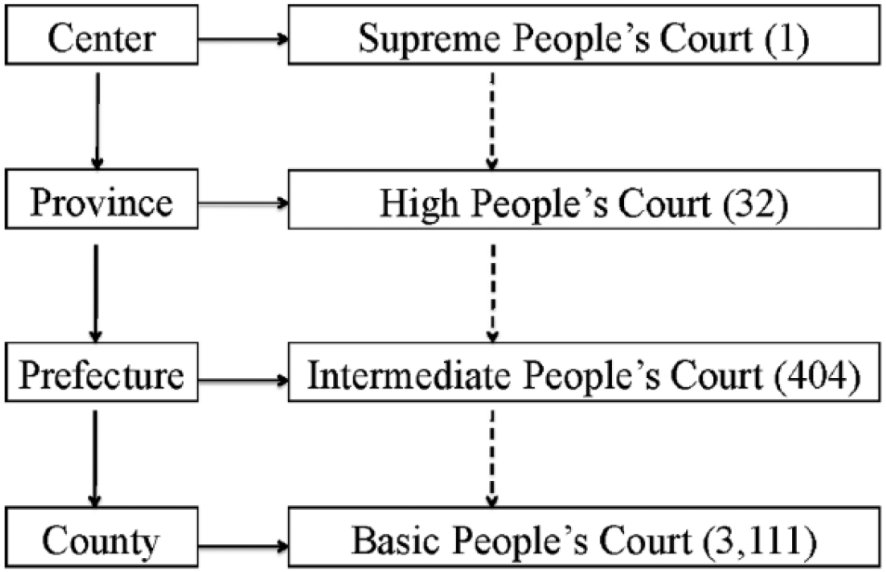
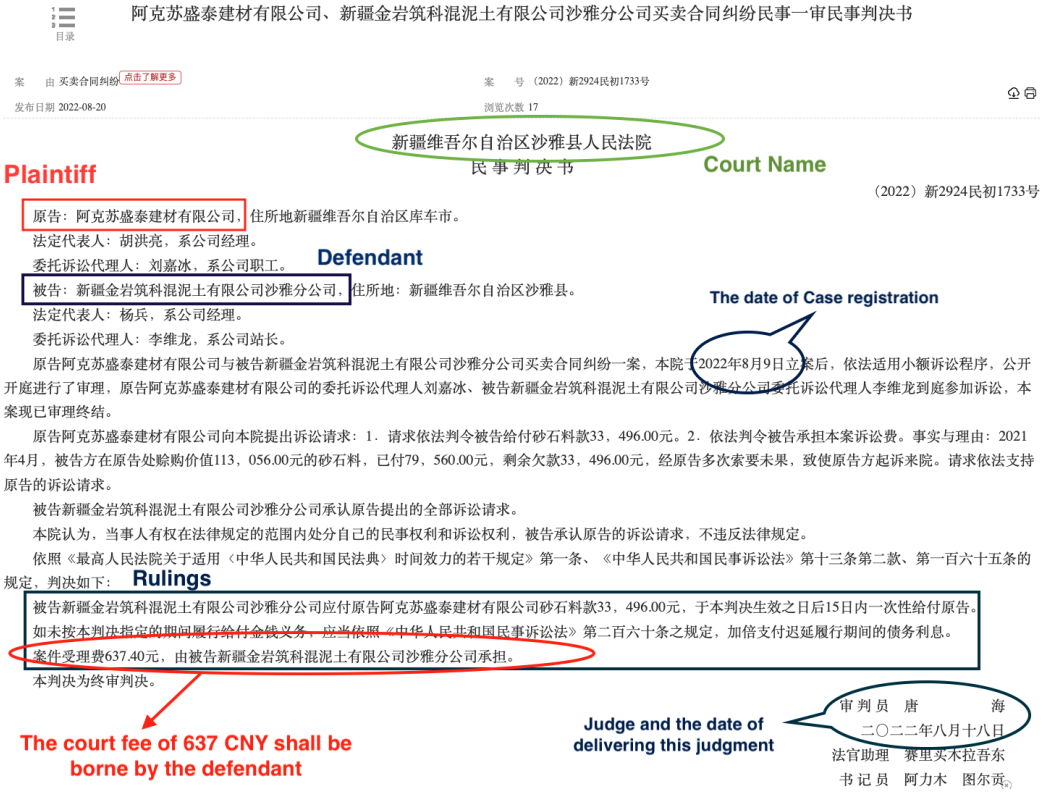


Figure S.1: Structure of China's Judicial System

Notes: Reprinted from Wang (2018).

(a) Frontpage of the China Judgements Online Website



(b) Sample Court Judgement

Figure S.2: China Judgements Online Website and An Example of Court Judgement

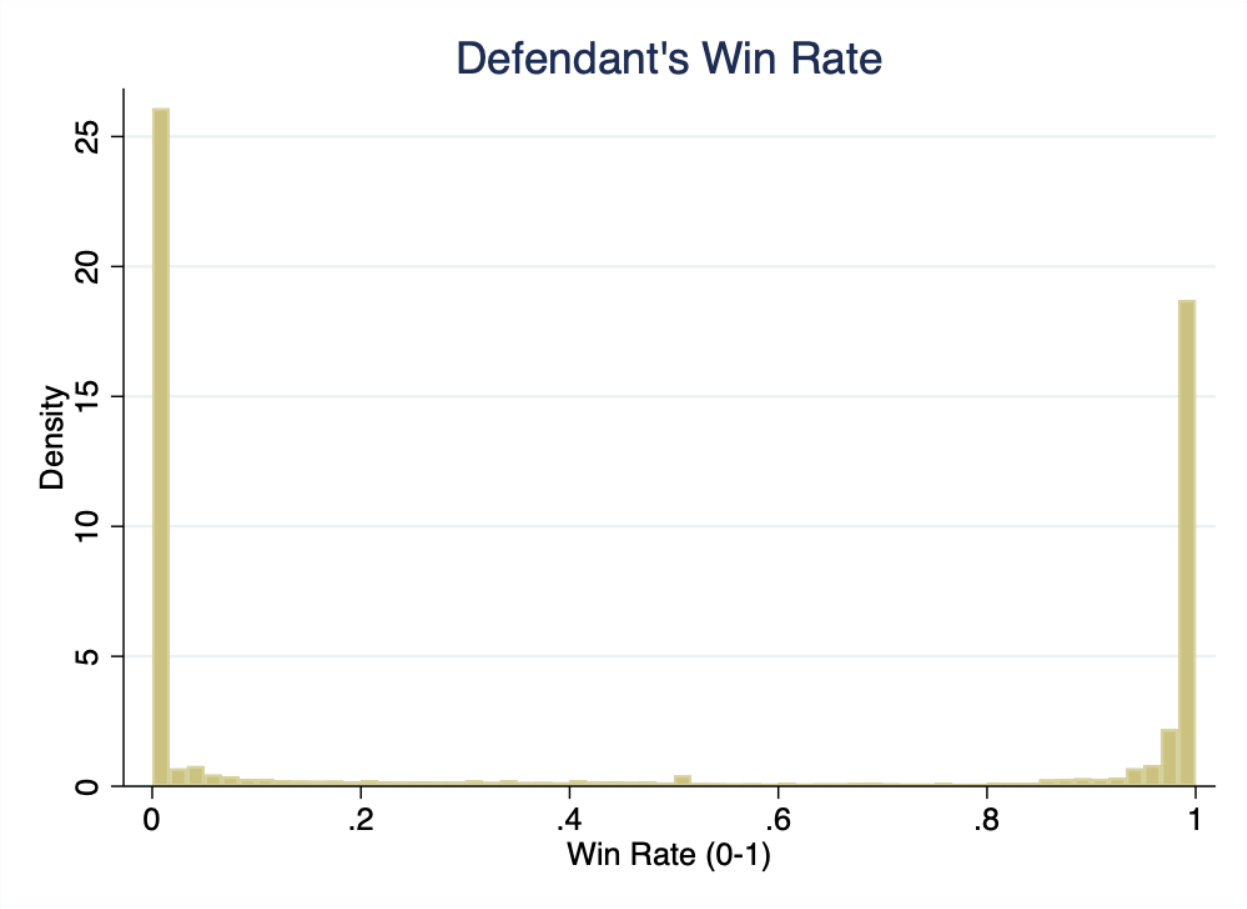


Figure S.3: Missing Rate of First Trial Court Verdicts

Notes: This figure plots the distribution of win rate for all the inter-regional commercial lawsuits in our sample using verdicts that were tried between 2014 and 2020 and released by *China Judgements Online* before August, 2022.

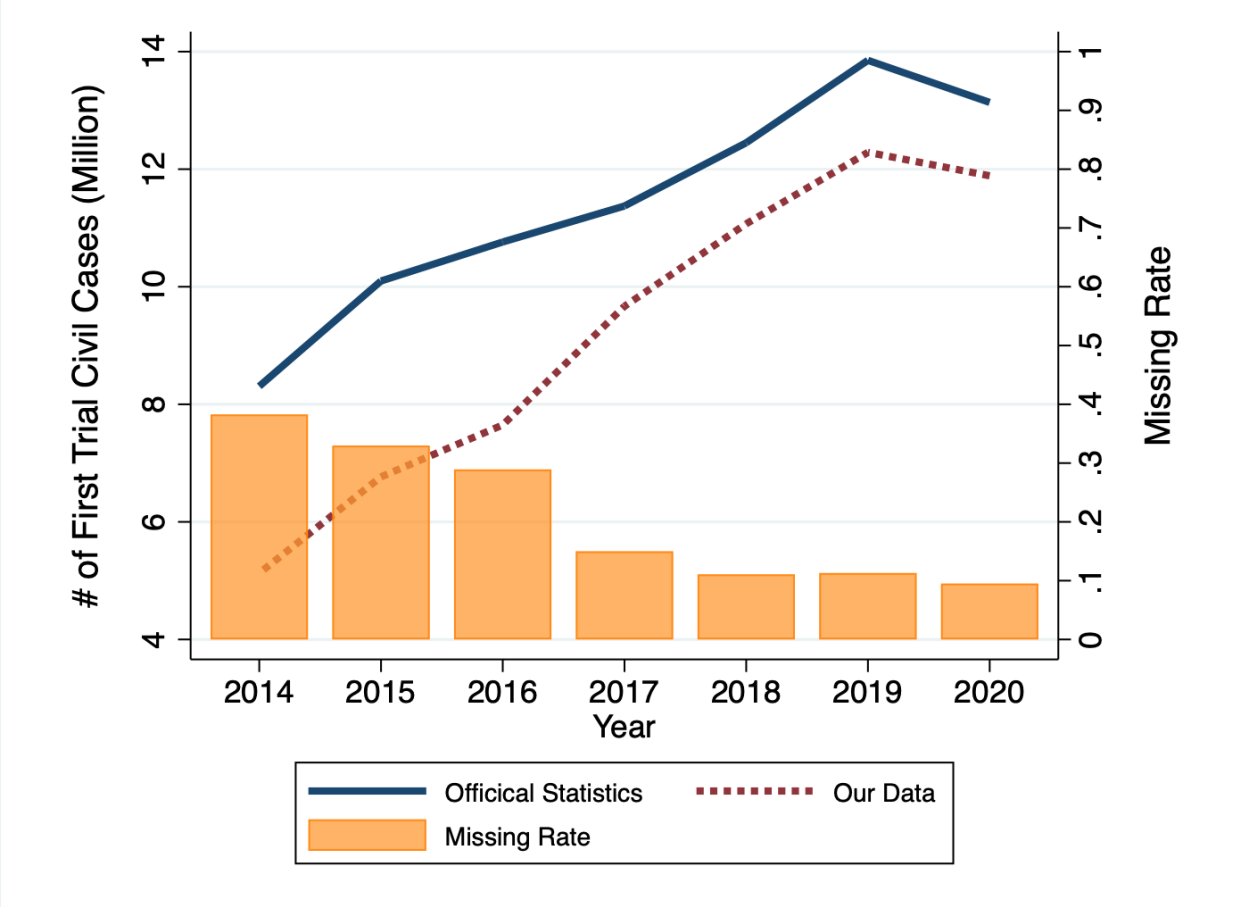


Figure S.4: Missing Rate of First Trial Court Verdicts

Notes: The official number of first trial civil cases is retrieved from *China Statistical Yearbook* published by National Bureau of Statistics between 2015 to 2021, while the number of first trial civil cases in our data is calculated using verdicts that were trialed between 2014 and 2020 and released by *China Judgements Online* before August, 2022.

Figure S.5: Frontpage of the Tianyancha.com