Judicial Independence, Local Protectionism, and Economic Integration: Evidence from China*

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Abstract

We show that judicial independence can reduce local protectionism and foster cross-regional economic integration. We exploit a judicial independence reform in China with staggered roll-out since 2014. The reform removed local governments' control over local courts' financial and personnel decisions, thereby substantially improving local courts' independence. Combining novel data on the universes of civil lawsuits and business registration records, we show that local defendants' rate of winning court cases against non-local plaintiffs declined by 7.0% after the reform. The effect is mainly driven by improvements in the quality of judicial decisions and is more salient for politically connected local defendants. Over time, the reduction in local protectionism encouraged smaller non-local firms to file lawsuits against larger local firms. Using the shareholding network extracted from business registration records, we find that the decline in local protectionism could attract 8.4% more inward investment flows into reformed localities. This has the potential to increase China's GDP by 1.9% when the judicial independence reform is implemented nationwide.

Keywords: judicial independence, local protectionism, economic integration
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1 Introduction

A just and functioning legal system that enforces contracts and property rights under the rule of law has been long perceived as a fundamental building block of economic prosperity (Smith, 1937; North, 1986; Besley and Persson, 2011; Acemoglu and Robinson, 2012). As a core principle of the separation of powers, judicial independence is necessary to prevent improper political influence over the courts, thereby enabling the legal system to play its key role in safeguarding fair trials, settling commercial disputes, and maintaining a competitive market environment (Hamilton, 1788;Hayek, 1960;Buchanan, 1974).

In reality, however, the degree of judicial independence from political influence varies substantially across the globe and is on average substantially lower in developing economies and civil law countries (Glaeser and Shleifer, 2002; La Porta et al., 2008). In such settings, political capture of judiciaries is far from uncommon, as different levels of governments frequently interfere with judicial decisions to favor local firms over external ones (Cooter, 1996; Hay and Shleifer, 1998). Such local protectionism impedes the formation of profitable business relations and deters economic integration. While a large cross-country literature has documented strong correlations between judicial independence and various politico-economic conditions (Djankov et al., 2003; La Porta et al., 2004; La Porta et al., 2008), there has been little rigorous empirical evidence on how judicial independence causally affects court decisions and economic activities.

This paper exploits the staggered roll-out of China's high-stakes legal reform to analyze the impact of judicial independence on local protectionism and economic integration. Several institutional features make China an ideal empirical setting to study the economic impact of judicial independence. First, prior to the judicial reforms studied in this paper, local governments held significant control over local courts. This, combined with the country's high levels of economic decentralization, led to ubiquitous judicial local protectionism in handling commercial lawsuits (Gong, 2004; Xu, 2011; Li, 2012; Ng and He, 2017; Wang, 2018). Even the president of China's Supreme People's Court once stated that "law

was taken by some local officials as a tool to protect parochial interests."[1] Second, since 2014, the Supreme People's Court has been gradually rolling out a high-stakes judicial reform that aims to shield the local courts from local political influence, improving judicial independence by taking away local governments' financial and personnel controls over local courts. Extensive anecdotal observations by legal scholars, judges, and lawyers testify that the reform has significantly transformed China's judicial system (Zhou, 2017; Chen, 2018; Zhang and Ginsburg, 2019; Supreme People's Court, 2019). Third, due to the sheer size and intricacy of the Chinese economy, economic integration (or the lack thereof due to local judicial protectionism) would likely have profound welfare consequences.

We compile novel administrative data on the universes of Chinese court verdicts between 2014 and 2021 (133 million cases) and business registration records between 1978 and 2021 (75 million registries). We document that the reform has significantly reduced local protectionism in judicial decisions. Following the reform, the win rate of local defendants against external plaintiffs dropped by 3.1 percentage points, which is a 7.0% reduction from the baseline average win rate of local defendants. The decline in the win rate is especially large for politically connected local defendants (11.4%), consistent with these firms receiving more local protection pre-reform.

Across various measures commonly used in the law literature, we find consistent evidence that the judicial reform has not only made court rulings less favorable toward local defendants, but also led to significant improvements in the quality of judicial decisions: (a) appeal rates decreased for external plaintiffs, while remaining unchanged for local defendants; (b) judges became more likely to approve requests for evidence examination and expert witness testimony; (c) court verdicts provided more detailed judicial reasoning in judgment files (with higher word counts); and (d) judges became less likely to cite discretionary codes in judicial reasoning. These results rule out the alternative interpretation that judicial independence did not actually lead to better judicial decisions, but instead simply enabled judges to start "selling" judicial decisions equally to both sides. We also find no evidence that the reform has affected the speed of trials or the enforce-

[1] See Zheng (1994), p.472.

ment rate of court orders, and we find no evidence of any career repercussions for the judges who reduced the degree of local protectionism after the reform.

The judicial reform affects local defendants' win rate through two channels: (a) it changes the judge's incentives in rulings (intensive margin); and (b) it changes the selection of non-local firms that litigate against local defendants (extensive margin). By focusing on the subset of lawsuits that were filed before the reform in each location and comparing rulings that were made shortly before or after the reform, we can tease out confounding changes in case composition and identify how rulings were affected by changes in judges' incentives. This exercise reveals that our baseline finding—that, on net the reform reduces the local defendants' win rates—is primarily driven by the intensive margin effects. In contrast, on the extensive margin, smaller non-local firms become more likely to sue larger local firms after the reform. Because these small plaintiffs have lower win rates against larger defendants, the changes in the composition of plaintiffs and defendants actually create a bias against our baseline estimates. This is consistent with the pattern that our intensive margin result is stronger than that in the baseline.

It is important to note that the reform does not completely decouple the judicial and executive branches or impose real checks-and-balances on the top-level political leaders; instead, the reform consolidates the personnel and financial controls over local courts to the provincial governments. The reform is thus better categorized as a move towards judicial independence in the sense of *rule by law*, rather than *rule of law*—meaning law is used as a tool of governance and does not stand above politics and the government. The reform does not fully shield judicial decisions from political influence. Even though it may be more difficult for a local firm to influence the higher-level provincial government, local protectionism may still manifest when the plaintiff is from outside the province of the defendant. One may even be concerned that the consolidation of power could exacerbate favoritism towards provincially and centrally connected firms. Empirically, we find that the reduction in judicial protectionism is stronger if the plaintiff is from within the province (but outside the county) than from outside the province; even in the latter cases, the reform has had a significantly positive impact on the win rate of the external plain-

tiffs. We find no support of heightened favoritism towards provincially and centrally connected firms, such as those that had prior procurement contracts with the higher-level governments or the provincially and centrally affiliated state-owned enterprises.

Taken together, our analysis of civil lawsuits indicates that the judicial independence reform systematically reduced local protectionism in China's judicial system, without introducing salient new biases. To understand how this affected economic integration, we construct a novel measure of cross-location entrepreneurial investment network in China by tracking the yearly changes in each firm's shareholding structure, as documented in the administrative business registration records. We find that, when a local court has undergone the judicial independence reform, firms in that jurisdiction received an additional 8.4% of annual investment from non-local business investors.

Further analysis reveals that the investment response is mainly driven by non-local firms making new entries to serve the local markets, rather than them making protection-seeking investments in existing local firms in the form of joint ventures. Consistent with Coase (1937), we also find that as the judicial reform lowers the transaction costs of dealing with local firms, inward investments become less likely to flow into local firms in complementary industries with input-output linkages and, instead, more likely to go to the same industries as the non-local investors. These findings suggest that, the judicial independence reform, by improving local business environments for non-local investors, has played important roles in facilitating economic integration in China.

To quantify the aggregate economic gains of reducing judicial local protectionism, we build a simple model of external entrepreneurial investments à la Melitz (2003). In the model, a judicial reform reduces protectionism-induced distortions by reducing a non-local firm's cost of serving the local market, and non-local entrepreneurs' pricing and entry decisions respond endogenously. We show that our reduced-form estimate on how the number of non-local entrepreneurial investors responds to the reform is a model-based sufficient statistic for the reform's overall economic impact. Our back-of-envelope calculation indicates that, when the judicial independence reform is implemented nationwide, the overall economic gains from improved cross-regional economic integration will

be as high as 1.9% of China's GDP.

This study relates to three strands of literature. First, it adds to the long-standing discussion on judicial capacity and economic development. While large bodies of theoretical work and cross-country literature have long pointed to 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 judicial independence shapes judicial outcomes and economic activities.[2] Notable exceptions are Mehmood (2022a,b), which show that a change in the selection procedure of judges in Pakistan, from presidential appointment to appointment by peer judges, reduced pro-government rulings and led to higher real estate investments. Our contribution is to exploit exogenous variation in judges' incentives (rather than their selection) and show that judicial independence from local political influence in China's civil law setting can reduce local protectionism, thereby fostering cross-regional economic integration.[3]

Our study also sheds new light on the importance of judiciaries in the politico-economic institutions of modern-day China. The conventional wisdom is that judiciaries play a limited role in the Chinese economy. Because 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 make "special deals" with important local firms to help them bypass the judicial system and obtain timely support (Bai et al., 2020a). As the authoritarian regime consolidated its political control over the past decade, it is generally believed to have turned further against the rule of law, leaving the government's political power unbound by the judicial

[2]In contrast, there exists a burgeoning empirical literature that studies the economic impacts of courts' speed in processing cases (Chemin, 2009; Visaria, 2009; Ponticelli and Alencar, 2016; Boehm and Oberfield, 2020; Rao, 2021).

[3]Our systematic analysis of the universe of Chinese civil lawsuits also adds to several smaller-scale studies on the legal impacts of the judicial independence reform in China, such as field interviews of judges (Wang, 2021), analyses of the around 1000 cases involving publicly listed firms (Zhao and Zhang, 2022; Lei and Li, 2022), or analysis of around 4000 administrative litigation cases (Zhou et al., 2021). More generally, this paper also speaks to the broader discussion on judicial biases in China (Gong, 2004; Li, 2012; Wang, 2013; Ng and He, 2017; Wang, 2018).

system (Minzner, 2011, 2015, 2018; Ringen, 2016; Zhang, 2016; Shirk, 2018). Our findings challenge such conventional wisdom. Different from the institutional forces in the decades prior, China's judicial reforms since 2014 have made local courts significantly more independent from local governments, thereby empowering local legal institutions to be increasingly important in facilitating economic development. These findings are corroborated by observations of legal scholars (Zhou, 2017; Chen, 2018; Zhang and Ginsburg, 2019; Supreme People's Court, 2019). This systematic turn towards legalism at the local level despite political centralization is an important yet under-appreciated change in China's delicate politico-economic equilibrium.

Finally, our paper also adds to the literature on local protectionism and economic integration (Baldwin and Venables, 1995; Nunn, 2007; De Loecker and Goldberg, 2014; Donaldson, 2015). As a byproduct of China's tournament-like political ladder (Zhou, 2004, 2014), local protectionism has been documented to hinder the country's economic integration (Young, 2000); distort the formation of industrial clusters (Bai et al., 2004); and generate considerable welfare losses (Barwick et al., 2021). Our contribution is to demonstrate judicial capture as an important channel through which local protectionism operates, and to show that an independent justice system could substantially reduce local protection and foster market integration.

The remainder of this paper proceeds as follows. Section 2 introduces the institutional background. Section 3 discusses our data. Section 4 presents the judicial impacts of the reform. Section 5 discusses the economic impacts of the reform and quantifies the welfare implications. Section 6 concludes.

2 Institutional Background

In this section, we introduce China's judicial system, provide background information on judicial local protectionism in China, and discuss the goal and implementation of the judicial independence reform.

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 Appendix Figure A.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. Courts at the latter three levels are collectively referred to as *local* courts.

When one firm brings a civil lawsuit against another firm, the trial is heard in the defendant's jurisdiction by default. Therefore, inter-regional commercial cases 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.[4] For a small share of civil lawsuits with exceptionally large damages involved, the first hearing happens at the level of the prefectural intermediate court or even the provincial high court. After the first verdict, one appeal can be made to the next level of the court system, up to the Supreme People's Court.[5]

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. As a result, 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 independence in China relies heavily on the incentives of the judges in local courts, and 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

[4]The percentage number is calculated using the raw data provided by China Judgments Online, see `https://wenshu.court.gov.cn/`

[5]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.

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 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.

Not surprisingly, when a judge's income and career development are both determined by the local government officials, it would be very 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 features a combination of political centralization and 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 the local firms over external ones. This could be driven by both the pursuit of career advancement and the extraction of personal rents.

An important way in which local government officials can exercise local protectionism is by influencing the local courts. In principle, courts are supposed to be independent organizations that can fairly resolve conflicts between local and external 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 require the judges to favor local defendants against external 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 internet memes ridiculing

the futility of going against these firms in court.[6]

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 local and supreme court judges, including the president of the Supreme People's Court (Zheng, 1994). Perhaps most telling is that, in a national survey of local judges about the sources of unfairness in China's judicial system conducted by the Supreme People's Court immediately before the judicial independence reform in 2014, 68% of the local judge respondents listed local protectionism as the major reason for biased rulings.[7]. These qualitative observations are also corroborated by quantitative studies documenting that connected firms tend to obtain systematically more favorable court rulings (Ang and Jia, 2014; Lu et al., 2015; Xu, 2020; Chen and Xu, 2021).

2.3 The Judicial Independence Reform

Because of the severity of judicial local protectionism, in November 2013, the Central Committee of the Communist Party of China published the "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 of the local governments, and ensure the proper enforcement of the rule of law."[8]

Following the central government's guidelines, in 2014, the Supreme People's Court formally launched the high-stakes judicial reform, which aims to systematically improve

[6]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 Nanshan, ByteDance has a 98% win rate in its home court Haidian; netizens came up with nicknames for these firms describing their home court advantages. Source: `https://bbs.mysipo.com/thread-1109742-1-1.html`

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

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

judicial independence by decoupling local courts from local governments.[9] 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 with the provincial governments. After this reform, county and prefectural governments no longer hold formal leverage over local courts, thereby significantly improving local courts' independence. The Supreme People's Court frequently refers to this reform as a milestone in China's legal development, which is echoed by extensive qualitative evidence documented by legal scholars (Zhou, 2017; Chen, 2018; Supreme People's Court, 2019). As summarized by Zhang and Ginsburg (2019), the reform has brought China's judicial independence to "an unprecedented level."

It is important to note that, by consolidating judicial controls to the provincial governments, this reform does not completely decouple the judicial and executive branches or impose real checks-and-balances on the top-level political leaders. The reform is thus better categorized as a move towards judicial independence in the sense of *rule by law*, rather than *rule of law*—meaning law is used as a tool of governance and does not stand above politics and the government. The provincial and central governments could remain influential in the judicial process. Because of these features, there could be two limitations that undermine the reform's role in eliminating local protectionism and political influence in judicial decisions. First, even though provincial governments are more detached from local adjudication than lower-level governments, local courts could still be pressured to exercise protectionism when the plaintiff is from outside the province. Second, as provincial governments are more sensitive to pressure from the center, the degree of favoritism towards provincially and centrally connected firms could remain substantial or even be

[9] An interesting question is why the reform was implemented in 2014, but not earlier, given that the widespread local protectionism and its hindrance to economic integration has been saliently recognized since the 1990s. As explained in Zhang and Ginsburg (2019), the reform's timing relates to two political trends in China. First, in the first three decades of China's economic reform, the country largely followed a decentralized economic development model, and a judicial independence reform that effectively weaken the local governments could encounter strong local oppositions. Since Xi took office in 2013, however, political power has been greatly centralized, thereby reducing the scope of local oppositions and also creating an incentive for the central government to combat local abuse of political power. Second, Xi's first major policy agenda was the anti-corruption campaign, which has earned him widespread support within China. As a result, positioning the Party leadership as a champion of legality against bureaucratic corruption became an effective way to strengthen political legitimacy.

heightened.[10] We will investigate these hypotheses in Section 4.

Figure 2a illustrates the staggered roll-out schedule of the judicial independence 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 Supreme People's Court 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.

The staggered roll-out of the judicial independence reform was designed deliberately as a gradually experimentation that covers a representative sample of economic areas.[11] Such policy experimentation is a common feature of China's high-stakes policy reforms, as it can reduce policy uncertainty and inform the policymakers on the optimal protocols for policy implementation (Wang and Yang, 2021). Even though other politico-economic factors (such as local fiscal conditions and central-local patronage relationships) may affect the sequence of reform roll-outs, there is no *a priori* reason that such factors should be systematically correlated with the underlying trends of local judicial biases and inter-regional investments, which is the assumption needed for our research design. In subsequent analyses, we will formally examine the potential selection issues in roll-out by testing for pre-reform trends in all judicial and economic outcomes using event study models.

[10] It is substantially less costly for the central government to ensure compliance of the 31 provincial governments than the over 2000 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)).

[11] For instance, see the press conference by the reform committee: http://www.gov.cn/xinwen/2014-06/15/content$_2$701248.*htm*

3 Data

In this paper, we compile, to the best of our knowledge, the most comprehensive dataset on commercial lawsuits in China. We combine it 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.

3.1 Universe of Court Verdicts

In 2013, as part of its efforts to increase 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 Appendix Figure A.2.

We collected the universe of court verdicts in China between 2014 and 2021 from CJO. This included 133 million judgment files, from which we identified more than 6 million civil lawsuits between firms.[12] These firm-to-firm civil lawsuits are the focus of this paper. For each judgment file, we extract 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, plaintiff claims supported by the court, obligations to pay court fees, and judicial reasoning provided by the judge.

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

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 this lawsuit, from the perspective of the court.[13] Therefore, we follow the law literature to extract detailed information on each party's obligation to pay court fees, and we measure the win rate of each side using the share of the opposing side's obligation to pay court fees: $WinRate_j = \frac{CourtFee_i}{CourtFee_i + CourtFee_j}$.[14] As reported in Table 1, the average win rate of local defendants in cases filed by external plaintiffs is 0.45, with a standard deviation of 0.47.

Besides win rates, we also follow the law literature and measure the "quality" of judicial decisions in four different ways: (a) the appeal rate after the first verdict, for both plaintiff and defendant;[15] (b) the court approval rate of requests to examine evidence or invite an expert witness, for both plaintiff and defendant;[16] (c) the richness of judicial reasoning in the judgment file (measured by word count);[17] and (d) the frequency of citing discretionary codes in the judgment file.[18]

The judgment files in the CJO data are generally reliable, as any manipulation of such publicized administrative records would require a three-way collusion among the plaintiff, the defendant, and the local court. The firms involved in commercial lawsuits, in particular, have strong incentives to ensure the accuracy of these judgment files since such information is widely used in the credit rating process by major commercial banks. Furthermore, in recent years China has adopted an open justice system whereby civil cases are live-streamed when requested by plaintiffs. As millions of trial recording videos are

[13] 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%.

[14] 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.

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

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

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

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

publicly accessible on the internet, it has become even more unlikely that the court judgment files can 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 cases from the website.[19]

However, neither issue is likely to substantially affect our analysis, for several reasons. First, the bulk of the missing cases documented in the literature were simply backlogs due to local courts' capacity constraints in the early years, and these files were added to the CJO later on.[20] Second, we have been scraping the CJO website daily since 2018 for any updates, and any cases that were deleted after posting—including the batch deleted in 2021—would have been captured by our data. Third, we cross-validate our data with the national-level official statistics in China Statistical Yearbooks from 2015 to 2021, which were published by National Bureau of Statistics. As shown in Appendix Figure A.3, we find that our data has an average missing rate of 21% in first trial civil lawsuits, and the missing rate fell below 10% in more recent years as local courts gained more technological capacity to digitize cases.[21] A substantial share of these missing files belong to those 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 judicial independence reform was correlated with file missing rates using official statistics from provincial-level statistical yearbook, and

[19] 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`

[20] 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 data.

[21] We can only calculate the missing rates for first trial civil lawsuits, as the China Statistical Yearbook only reports the number of first trial of civil cases. Nevertheless, it's unlikely that omitting appeal cases would substantially affect the missing rate since first trial case accounts for over 90% of all the verdicts.

find no such evidence (Appendix Table A.1).

3.2 Universe of Business Registrations

Our firm-level data is from *Tianyancha*, a company that offers paid access to the universe of China's business registration records. These records are licensed by the National Enterprise Credit Information Publicity System, maintained by the State Administration for Industry and Commerce (SAIC). Appendix Figure A.4 shows a screenshot of the *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 civil 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 to 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 we assign each individual investor the location that accounts for the most shareholdings. We then use the location information to label individual investors as "local" or "non-local." Our procedure enables us to construct a county-to-county 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 over the latter. Even though registered capital should in principle capture the total capital injected by the investors at the time of registering a firm, during our sample period firms in China were not legally required to provide proof for the full amount of registered capital at the time of registration. Firms have incentives not to completely misrepresent their registered capital;[22] nevertheless, this variable may contain measurement error, as there are low legal and financial stakes for accurate accounting (Shi et al., 2021).

We use the number of external investments as the main outcome variable, because of its accuracy, for measuring cross-region integration. We continue to use the total investment amount as a secondary outcome because there is no obvious reason that the measurement error would be systematically correlated with the roll-out schedule of the judicial independence reform. Importantly, as explained in Section 5.4, 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 the 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 independence 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

[22] There is limited incentive to under-report a firm's registered capital, as it may be used by potential partners as a reference to the firm's overall economic capacity; in some industries there are also minimum requirements on the 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.

of reform roll-out. The roll-out information was further corroborated using information from local courts' websites and local governments' fiscal expenditure records.

Second, to identify local defendants that are connected to local governments, 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 governments between 2013 and 2021. We define firms with government contracts as "connected," and those without contracts as "unconnected."

Third, to verify whether the judicial reform's impact on court rulings resulted in 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 eventually not fully executed.

3.4 Descriptive Statistics

Table 1 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.[23]

As a motivation for the subsequent econometric analyses, Figure 2b plots the time-series patterns of the judicial independence reform roll-out and judicial local protectionism aggregated to the national level. Specifically, the orange bars represent the number of prefectures that have adopted the reform, and the black line shows the difference in local defendants' average win rates between courts that were eventually treated and non-

[23]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 the local firms unless they have a very strong case. A related interpretation is that, many *ex ante* strong cases (taking the local courts' biases into account) would be settled outside of the court, causing the win rates to converge to 50%-50% in equilibrium (Priest and Klein, 1984).

treated (as of 2021). As can be seen, as the reform rolled out to an increasing number of courts after 2014, local defendants' average win rates in the eventually reformed courts dropped sharply and steadily, relative to local defendants' average win rate in the eventually non-reformed courts.

In Figure 2c, we repeat the same exercise for investment flows into eventually reformed counties, relative to eventually non-reformed counties (as of 2021). As we can see, as local courts experienced the reform in more counties, the gap in investment inflows between eventually reformed counties and eventually non-reformed counties kept enlarging.

Figures 2b and 2c suggest that, at the aggregated level, the reform roll-out is strongly correlated with both court rulings and investment flows. In Sections 4 and 5, we will try to establish the causal impacts of the judicial independence reform on the two outcome variables using a difference-in-differences approach.

4 Judicial Impacts

In this section, we investigate the impacts of the judicial independence reform on various judicial outcomes.

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:

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

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 has already gone through the reform at time t, and zero otherwise; and ϕ_i and λ_t stand for court fixed effects and semi-year fixed effects, respectively. The standard errors are clustered at the local court level. For robustness, in Section 4.1, we also present alternative DiD results based on disaggregated case level data.

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

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

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. The patterns are essentially the same for conventional unadjusted event study estimates and for estimates based on other methods proposed in the recent literature (i.e., Borusyak et al., 2021 and Callaway and Sant'Anna, 2021). We present different versions of event studies when discussing robustness in Section 4.1.

4.1 Win Rates of Local Defendants

Table 2 Column (1) presents our baseline DiD result, obtained from estimating Equation 1 using court-semiyearly level data. The results show that, after the reform, local courts rule significantly less favorably toward local defendants; their average win rate (against external plaintiffs) falls by 3.1 percentage points, representing a 7.0% reduction from their baseline average win rate.[24]

Because the judicial independence reform consolidates the control over local courts to the provincial level—as discussed in Section 2—the reform 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 (2) and (3) of Table 2: inter-county cases within the same province tried in the reformed local courts saw a reduction in the defendants' win rate by 4.4 percentage points, while inter-provincial cases only saw a reduction by

[24] In Appendix Table A.2, 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. All our results in Table 2 remain significant and are even larger in magnitude under this alternative, case-level specification. The larger magnitudes also indicate that the reform's impact is larger for local courts that have higher case loads.

2.3 percentage points.[25] In particular, the smaller but still significant effect in Column (3) suggests that, even though in principle the reform leaves open the scope for provincial governments to exercise local favoritism, in practice the degree of favoritism experienced a significant decline for inter-provincial cases as well. This could be due to two potential reasons: (a) it might be more difficult for firms to influence provincial governments, relative to influencing lower level (county and prefectural) governments; and (b) the benefit relative to the cost of protecting a single firm may be lower for provincial governments than for county and prefectural governments.

It is important to note that the judicial reform does not fully shield judicial decisions from political influence; in particular, the favoritism towards firms that are connected to the provincial or central governments can remain substantial or even be heightened. A natural question is that, through consolidating even more political and judicial power to the higher-level governments, would the reform alleviate or exacerbate this type of favoritism? To answer this question, in Columns (4) to (6) of Table 2, 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 governments 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 provincial or central governments if it has won procurement contracts from these higher-level units. Our results show that the judicial reform: (a) reduces unconnected local defendants' win rates by 7.0%; (b) reduces locally connected local defendants' win rates by 15.2%; and (c) has no detectable impact on the win rates of provincially or centrally connected local defendants.[26] These findings suggest that the reform's impact is most salient on the locally connected firms, who 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 (or the alleviation) of judicial favoritism towards provincially and centrally connected firms.

In a similar spirit, we examine whether the reform has differential effects on the locally owned SOEs versus the provincially and centrally owned SOEs. As shown in Appendix

[25]The two coefficients are statistically different at the 1% level.
[26]These coefficients are pair-wise significantly different at the 1% level.

Table A.5, 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 the win rates following the reform. These results are consistent with the findings in Table 2, 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.

As a placebo test, Appendix Table A.4 replicates Table 2 with a different outcome variable: the average win rate of local defendants against *local* plaintiffs.[27] 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 generally affect all plaintiffs/defendants in different ways.[28]

Figure 3 shows the dynamics of the treatment effect, by plotting the event study coefficients obtained from estimating Equation 2. For the baseline specification, we observe a flat pre-trend before the reform, and a salient reduction in local defendants' win rate immediately after the reform, which is persistent in the subsequent periods. For robustness, Appendix Figure A.5 presents results from alternative event study specifications, and our results remain. In Appendix Figure A.6, we also observe consistent patterns for the two sets of heterogeneity results: flat pre-trends in all subsamples, but more salient trend breaks after the reform for intra-provincial cases and cases with locally connected defendants. These findings provide additional confidence in the validity of our research design.

We also examine where in the distribution of local defendants' win rates the baseline DiD results come from. Specifically, using case-level data, we assign each ruling into one of four bins based on the local defendants' win rate: 0-25%, 25-50%, 50-75%, and 75-100%. We then separately fit Equation 1 on indicators for whether the local defendants'

[27] In Appendix Figure A.8, we present the corresponding event studies.

[28] Not surprisingly, as shown in Column (2), local defendants with political connections have reduced win rates after the courts gained independence from political influence. Importantly, however, the magnitude of this reduction is less than one-third of what was documented in Table 2 Column (2); as shown in Column (3), non-connected local defendants do not see any significant changes in win rates at all. These results are consistent with the judicial independence reform removing both local protectionism and political favoritism at the same time.

win rate belongs to each bin. The DiD coefficients from these regressions are plotted in Figure 3b. 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 suggests that, before the judicial independence 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 inframarginal judicial local protectionism could be more costly than a scenario where the rulings are slightly and equally tilted in favor of all local firms.

4.2 Quality of Judicial Decisions

We interpret the results documented in Section 4.1 as evidence for the court becoming less biased after the judicial independence reform. However, an alternative interpretation is that, after the reform, as local courts are no longer forced by local government to favor local firms, they may simply make careless decisions, or even start "selling" judicial decisions to whichever party is willing to pay a higher bribe. In these scenarios, the court simply replaces 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 directly investigate how the reform has affected the *quality* of judicial decisions. Following the law literature, we measure the quality of judicial decisions in four different ways. First, a lower appeal rate is generally used as a proxy for more fair 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 solid if there is a higher word count in the judgment file that explains the judicial reasoning behind the verdict (Liu, 2018). Fourth, judicial rea-

soning that frequently cites “discretionary codes" are typically deemed less legally sound (Liu and Li, 2019).[29]

Table 3 shows that, across all four quality measures, there are significant improvements in judicial quality after a court goes through the judicial independence reform: (a) appeal rates drop by 13% for external plaintiffs while remaining unchanged for local defendants;[30] (b) judges become 28% more likely to approve external plaintiffs' requests for evidence examination and expert witness testimony, while not changing approval rates for local defendants; (c) judges provide more detailed legal reasoning for the rulings in publicized judgment files (as reflected by a 7% increase in the word count for legal reasoning); and (d) judges become 15% less likely to cite discretionary codes when conducting legal reasoning.

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

To understand the dynamics of these results, in Figure 4, we plot the event study coefficients that correspond to the DiD results presented in Table 3. 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 increas-

[29] “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.

[30] In Appendix Table A.6, 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: (1) external plaintiffs appeal less after the reform even if they lose, and (2) local defendants are no more likely to appeal after the reform even if they lose. Fact (1) is especially suggestive of the reform's role in improving the quality and fairness of judicial decisions. Fact (2) 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 to become even more protective of local firms after the reform (and are thus less likely to reverse the initial verdict). However, this interpretation is inconsistent with fact (2) that, after the reform, the local defendants are no more likely to appeal even when losing.

ing over time, which helps explain why the baseline results on win rates appear to be persistent.

4.3 Changes in Rulings vs. Composition of Cases

Our baseline results on local defendants' win rates could come from two sources. On the intensive margin, the judicial independence reform changed the incentives of the judges, so that the same case would be adjudicated differently before and after the reform. On the extensive margin, after observing the intensive margin effects, external firms could adjust their lawsuit 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 margins and show that our baseline effects are primarily driven by the former, intensive margin. If anything, the changes in case composition tends 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 intensive margin effect, we focus on the subset of lawsuits that were filed within six months before the local court implemented the judicial independence reform, and compare the rulings that were made before and after the reform. As shown in Appendix Table A.7, 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 extensive margin effect and single out the intensive margin effect. As shown in Table 4, for the same court, rulings made right before the reform appeared to be significantly more favorable toward local defendants, compared to rulings made shortly after the reform. This result is robust to controlling for a demanding set of judge fixed effects, and the effect size is significantly larger than the baseline DiD estimates. This confirms that the change in judges' incentives is the main driving force behind our baseline findings.[31]

Second, we investigate the reform's extensive-margin impact on the composition of

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

cases that get filed. As shown in Appendix Figure A.9, 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 5 Column (1) quantifies this effect: lawsuits between external plaintiffs and local defendants increased by 8.2% 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 Table 5, Columns (2) to (7), after the reform, the plaintiffs on average had 10.2% less registered capital and 19.5% fewer employees, and were 10.2% younger in terms of firm age. In contrast, the local defendants were on average 11.0% larger as measured by total capital, 16.3% larger as measured by total employees, and 5.6% 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 extensive margin effect creates more cases with weak external plaintiffs and strong local defendants, to the extent that such cases have a lower win rate for the plaintiffs (which explains why the 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 intensive margin effect, which is free from this change in case composition, appears to be substantially larger than the baseline effect (which is a combination of intensive and extensive margin effects).

4.4 Other Potential Effects of the Reform

Our evidence thus far suggests that the judicial reform has reduced the local courts' favoritism towards local defendants in cases involving external plaintiffs. In this section, we investigate other potential effects and concerns that may compromise our interpretation.

One potential concern is that, even as the judicial quality improves and local favoritism declines after the reform, there may have been a reduction in judicial compliance, as the

rulings against local defendants may not have been strictly enforced. If this is the case, then the nominal reduction in local protectionism may not be actually consequential. Empirically, we find no evidence that supports this hypothesis: the reform's impact on rulings has indeed translated into enforcement. 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 data 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.8, 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 the same. These patterns provide evidence against the "reduced compliance" hypothesis.

There is also the possibility that, despite improving the 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) changes in response to the reform. As Appendix Figure A.11 shows, there is no significant changes in the duration of trials, indicating that the local judges improved the quality of judicial decisions without sacrificing the quantity.

Another possible side effect of the reform is the potential career repercussions for the local judges, who, as a result of the reform, increasingly rule against the interests of the local governments. To examine this possibility, we identify the judges in our data that have stopped appearing in court verdicts for more than six months. Such disappearance could potentially reflect a judge being fired or being assigned fewer cases as a punishment (which is associated with lower performance pay and promotion likelihood).[32] In Appendix Figure A.10, we show that, after the reform, despite ruling significantly less favorably towards local defendants, local judges did not become more likely to disappear from our data. This lack of career repercussions is consistent with our interpretation that

[32]Naturally, a judge could also stop appearing in verdicts since he has reached the official retirement age. While we cannot directly observe judge age, there is no obvious reason for this variable to be systematically correlated with the judicial reform.

the local governments no longer hold strong leverage over the local judges as a result of the reform.

5 Economic Impacts

Since commercial cases are tried in defendant's jurisdiction by default, all else equal, non-local firms should be discouraged from conducting business or investing in regions where local protectionism is practiced by local courts. A politically captured judicial system therefore hinders regional economic integration by deterring non-local firms from operating in the local market. A prime example is China's Northeast region, which has long been criticized for its poor judicial environment and prevalent local protectionism, and is well-known within China as a region for non-local investors to avoid.[33]

In this section, we examine this hypothesis formally. In Section 5.1, we leverage novel panel data on the universe of inter-regional investment network in China, and document the reduced-form relationship between the judicial independence reform and inter-regional investment flows. In Section 5.2, we document that the reform encourages new entries to serve the local market, while discouraging protection-seeking investments into politically connected local firms. In Section 5.3, we investigate the spillover effects between treatment and control regions. In Section 5.4, we construct a simple model of cross-location business investments and derive model-based sufficient statistics to map reduced-form estimates into the overall economic gains from reducing judicial local protectionism.

5.1 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

[33]China's outgoing premier, Keqiang Li, also publicly acknowledged this portrayal of the Northeast region in a speech in 2016; see https://zh.m.wikipedia.org/zh-hans/%E6%8A%95%E8%B5%84%E4%B8%8D%E8%BF%87%E5%B1%B1%E6%B5%B7%E5%85%B3.

county-year level, for each county, we obtain a measure of the yearly total investments received by all local firms from outside investors.[34]

To investigate how the judicial independence reform affects external investments into each local county, we estimate Equation 1, using "outside investment" as the outcome variable. Columns (1) and (2) of Table 6 show that, when a prefecture undergoes the reform, the number of outside investments received by firms in that prefecture goes up by 8.4%, and the total value of outside investments increases by 8.0%. The investment responses to the reform indicate that improved judicial environments indeed attract more outside investments, likely by making credible commitments to protect property rights and enforce contracts (Anderson and Parker, 2008). These findings also echo the rich cross-country evidence documented in the FDI literature.[35]

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.

Figure 5 shows the dynamics of the investment effects. There are no pre-trends in investments prior to the reform; one year after adopting the reform, treated regions start to experience steady growths in both the number and the total value of yearly investments from outside investors. The magnitudes of the investment effects appear to stabilize after four years, suggesting that the long-run impact of the reform on investments is likely larger than our DiD estimates.[36]

A potential reason for the relatively quick investment response to the reform could be found in the reform's decade-long roll-out schedule: when regions adopt the reform

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

[35]For example, see Globerman and Shapiro (2002); Li and Resnick (2003); Li (2021).

[36]In Appendix Figure A.12, we present results from alternative event study specifications, and the results remain the same.

in later years, outside investors may have long witnessed the changes that have taken place in those early-reform regions and may thus be more confident in investing in the newly reformed regions. As shown in Columns (1) and (2) of Table A.9, the investment response was slower in the early years of reform and faster in the later years, consistent with outside investors gradually updating their expectations about reform effectiveness as the reform expands nationwide.[37]

5.2 Mechanisms for the Response of Inter-Regional Investments

If the observed increase in inter-regional investments is indeed driven by reduced judicial local protectionism, one might expect heterogeneities with respect to different types of investments.

One important dimension of heterogeneity concerns whether non-local firms serve the local market through setting up local branches (new entry) or investing in existing local firms (joint venture). In the new entry scenario, the non-local firm likely has to engage in extensive dealings with other local firms and local workers, thereby generating the scope for transaction costs and contractual disputes. The judicial reform, by reducing judicial local protectionism, could potentially lower these transaction costs and therefore encourage more entry. By contrast, the reform's impact on joint ventures is more ambiguous. On the one hand, investing in politically connected local partners is a substitute for direct entry that can potentially shield non-local investors from local judicial discrimination. By way of making direct entry more viable, the judicial reform may reduce the attractiveness of joint ventures. On the other hand, non-local investors in joint ventures may also benefit directly from the reform due to better investor protection.

In Columns (3) and (4) of Table A.9, we test these predictions by separately investigating the reform's impacts on inter-regional new entry and joint venture formation.[38] Results show that the reform leads to a large and significant increase in new entries, and a relatively modest change in joint ventures. These patterns are consistent with hypoth-

[37]The event studies corresponding to these results are presented in Appendix Figure A.13
[38]The event studies corresponding to these results are shown in Appendix Figure A.14.

esis that the reform lowers transaction costs for non-local firms and raises the viability of serving the local market via direct entry, without having to seek legal protection by integrating with powerful local partners.[39]

Another interesting and related dimension of heterogeneity concerns the types of local industries receiving inward investments. While lower transaction costs should unambiguously encourage 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 linkages 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 (5) and (6) of Table A.9, our baseline investment results are indeed entirely driven by inter-regional investments within the same industry of the external entrepreneurs.[40] 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 decreased significantly following the reform. These findings are highly consistent with the judicial independence reform reducing non-local firms' transaction costs of dealing with local business partners, which reduces non-local firms' incentives to integrate with complementary local firms and promotes direct entry.

[39] As shown in Columns (7) and (8) of Table A.9, the baseline investment increase is mainly driven by investment in non-tradable sectors, rather than tradable ones. This is also consistent with the interpretation that the new entries are non-local firms trying to serve the local markets. The event studies corresponding to these results are shown in Appendix Figure A.17.

[40] The event studies corresponding to these results are presented in Appendix Figure A.15

5.3 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 misattribute the distributional effect due to cross-location substitution of economic activities as the aggregate gains experienced by the treated locations.

We show this issue is unlikely to be empirically relevant: the treatment effect we find is indeed due to better regional economic integration rather than due to substitution of economic activities. Specifically, we exploit the fact that the reform's roll-out varies at the prefecture level, meaning all counties within the same prefecture have the same treatment status over time. Column (1) of Table 7 estimates the reform's impact on investment flows from outside counties within the same prefecture.[41] Results show that the baseline investment response is predominantly driven by these intra-prefectural investment flows; the number (and total amount) of investment from other counties in the same prefecture increased by 11.6% (17.4%) following the reform. In contrast, Columns (2) and (3) show that investment flows from external prefectures or provinces did 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 towards investing non-locally into other counties, or because external investments crowd out (or crowd in) local investments. In Column (4) of Table 7, we repeat

[41] The corresponding event studies are presented in Appendix Figure A.16

the DiD exercise using intra-county investments as the outcome variable; the result shows that the judicial independence reform does not affect local entrepreneurship differentially between treated and control prefectures. In Appendix Table A.10, we further examine the response of different types of local investments, including the entry by new local firms (column 1); local investments by existing local firms in total (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 independence 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.

5.4 Welfare Implications

To conduct a back-of-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.[42]

[42] Admittedly, the modeled impact of the reform is simplistic and misses other potential politico-economic implications. Nevertheless, as discussed in Section 4 (and especially Section 4.4), 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 first order when compared to the judicial benefits.

We show the investment response to the reform, as estimated in Section 5.1, 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.

5.4.1 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 modeling choice 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. 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 draw-

ing the cost index φ, and $\pi(\varphi)$ is the variable profits:

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

The equilibrium price $p(\varphi)$ is the maximizer of (6). Define $\bar{\varphi}$ as the cost index for which $\pi(\bar{\varphi}) = f$. 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 produce with marginal cost φ, 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 $\tau\varphi$ with a 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 $\{p(\varphi)\}$, quantities $\{q(\varphi)\}$, and variable profits $\{\pi(\varphi)\}$, such that a firm enters iff $\pi(\varphi) \geq f$, $\pi(\varphi)$ solves (6), prices are the maximizers of (6), and quantities are consistent with the consumer demand function $q(\varphi) = q^*(p(\varphi))$.

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 modeling choices are motivated by our reduced-form findings in Table 7, 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 excluding 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.

5.4.2 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 surpluses 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

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

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. 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 8.4% 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 experience the same proportional gains from the reform.

The judicial independence 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 independence reform could lead to over a $20 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-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 23.1% of total firm count (and 19.3% by registered capital), Proposition 1 implies the judicial reform, by facilitating more inter-regional investment flows, can potentially expand GDP by 1.9% if adopted throughout China.

6 Conclusion

China's high-states judicial independence reform decoupled local courts from the influence of local governments. This paper shows that the reform has systematically reduced judicial local protectionism and facilitated economic integration in the country.

To reach this conclusion, we compile novel administrative data sets covering millions of commercial lawsuit verdicts and business registration records, and we exploit the staggered roll-out of the reform between 2014 and 2021. We find that the reform has signif-

icantly reduced judicial local protectionism in China, as reflected by a 7.0% reduction in local defendants' average win rate against external plaintiffs. This effect is particularly salient for firms connected to the local governments, but is muted for firms connected to the provincial and central governments. The baseline effect is also stronger for inter-county and inter-prefectural lawsuits within the same province than for inter-provincial lawsuits. These patterns are consistent with the fact that the reform only decouples judicial and executive branches at the local levels, but does not impose real checks and balances on the provincial and central governments.

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 towards local firms but also improved the quality of judicial decisions: (a) external plaintiffs became less likely to appeal the verdict (no change for local defendants); (b) courts became more likely to allow external plaintiffs to examine evidence or invite an expert witness (no change for local defendants); (c) judges provided more detailed judicial reasoning in the judgment files; and (d) judges were less likely to cite discretionary codes in judicial reasoning. Taken together, these results consistently suggest that the reform has led the local courts to reduce their favoritism towards local defendants and improve the quality and fairness of judicial decisions.

Further analysis allows us to decompose the reform impacts into intensive and extensive margins. On the intensive margin, we document that, for similar cases, the same judge would rule significantly differently before and after the reform. On the extensive margin, we find that reform led an increasing number of small, young, external firms to sue their large, old, local counterparts, thereby changing the composition of civil lawsuits in China in the longer run.

We exploit administrative data on the universe of inter-regional investment networks in China and find that external individual and business investors are 8.4% more likely to invest 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-envelope calculation suggests that, by improving cross-regional economic integration, a nation-wide roll-out of the judicial independence reform can potentially lead to overall economic gains by as much as 1.9% of China's GDP.

Our findings have important implications for the understanding of the contemporary Chinese political economy. In traditional conceptualizations of the Chinese economy, judiciaries play a limited role since 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 rule of law, leaving the government's political power unbound by the judicial system. Our findings challenge such conventional wisdom. Different from the institutional forces in the decades prior, China's judicial reforms since 2014 have made local courts significantly more independent from local governments, thereby empowering local legal institutions to be increasingly important in facilitating economic development. These findings are corroborated by observations of legal scholars. This systematic turn towards legalism at the local level despite political centralization is an important yet under-appreciated change in China's delicate politico-economic equilibrium. As documented in our paper, increased judicial independence has indeed significantly reshaped the inter-business as well as state-business relationships in China in the past decade.

Importantly, by consolidating judicial powers to the provincial governments, China's judicial independence reform does not completely decouple the judicial and executive branches or impose real checks-and-balances on the top-level political leaders. Nevertheless, the reform embodies a historical move towards legalism in China. Going forward, important questions remain on the sustainability of the co-existence between a centralized authoritarian government and independent local judiciaries, whether the movement towards legalism can progress further, and how the turn towards legalism will affect the fate of an authoritarian regime and the welfare of its citizens in the long run. Answering such questions will help us better understand China, as well as other authoritarian regimes

worldwide, such as Pakistan, Russia, and Turkey, which have recently gone through similar judicial independence reforms.

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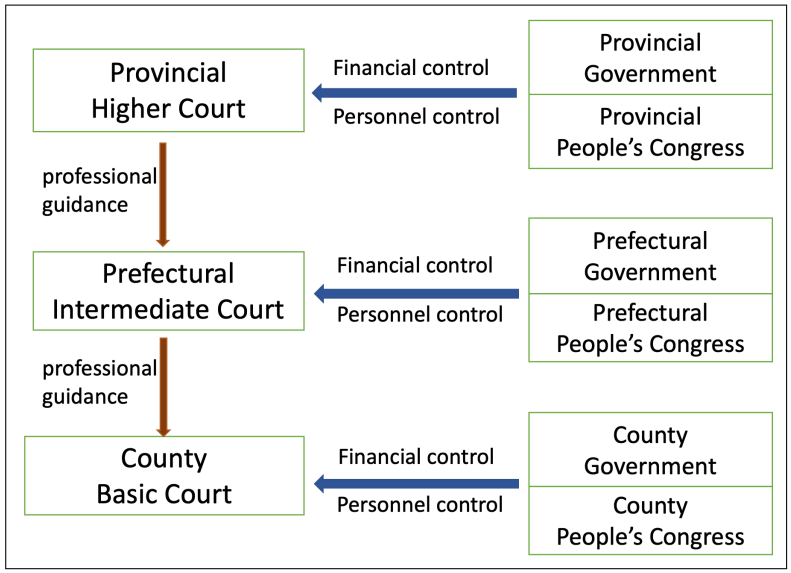
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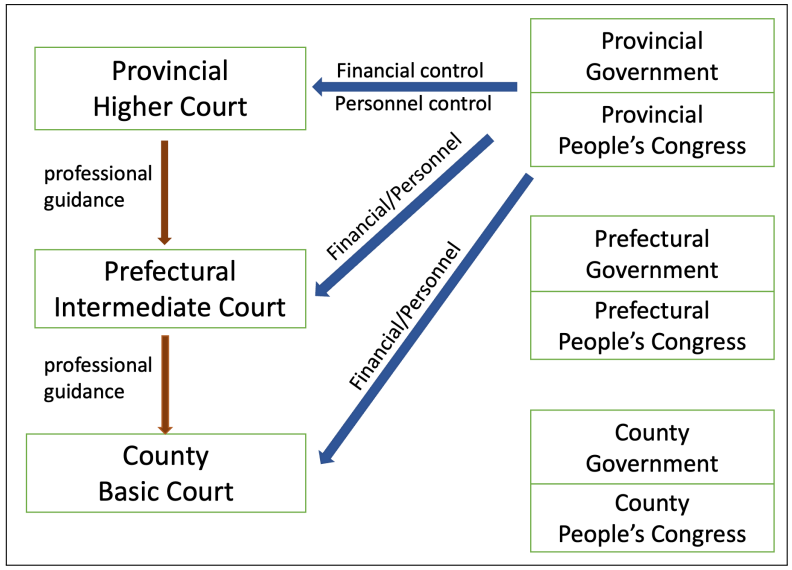
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Figures and Tables



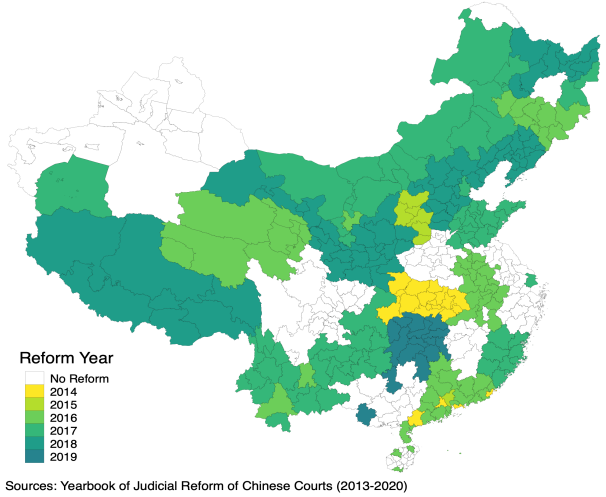
(a) Before



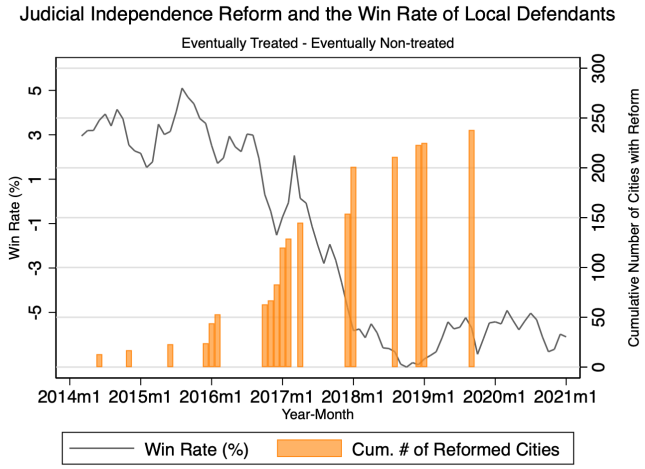
(b) After

Figure 1: Judicial Independence Reform and Incentive Structure Changes

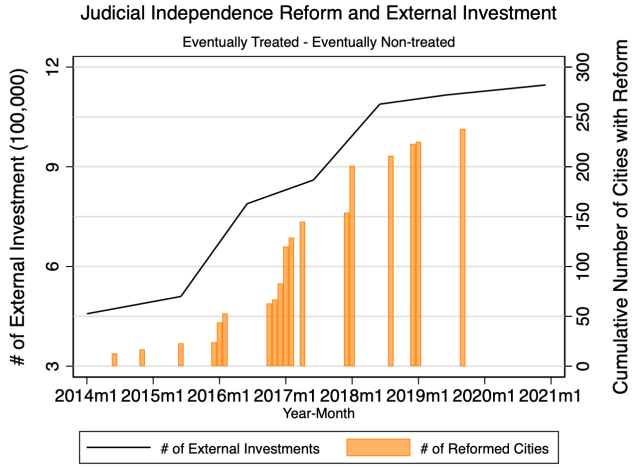
Notes: Panel (a) demonstrates the administrative structure of China's local judiciaries *before* the judicial independence reform. Panel (b) demonstrates the administrative structure of China's local judiciaries *after* the judicial independence reform.



(a) Geographical Expansion



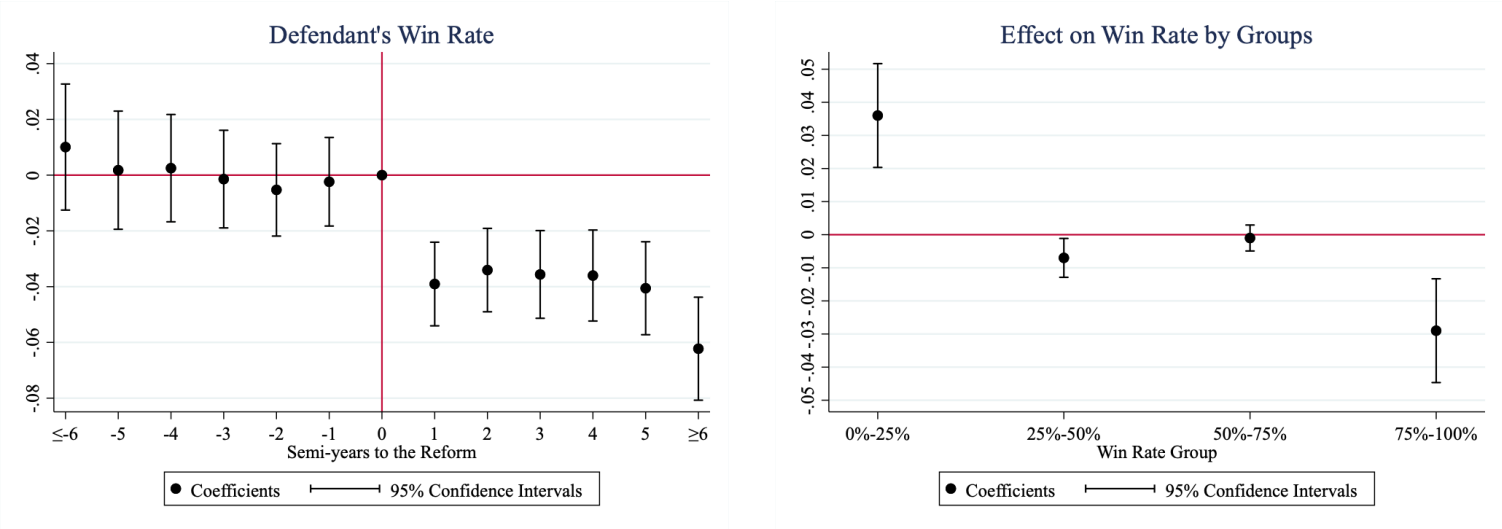
(b) Reform Expansion & Local Defendants' Win Rate



(c) Reform Expansion & External Investment

Figure 2: Reform Expansion

Notes: Panel (a) illustrates the roll-out schedule of the judicial independence reform across the country. In Panel (b), the orange bars represent the number of prefectures that have already adopted the judicial independence 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). In Panel (c), the orange bars represent the number of prefectures that have already adopted the judicial independence reform in a given year, and the black line plots the difference in inward investment flows between eventually treated counties and eventually non-treated counties (as of 2021).

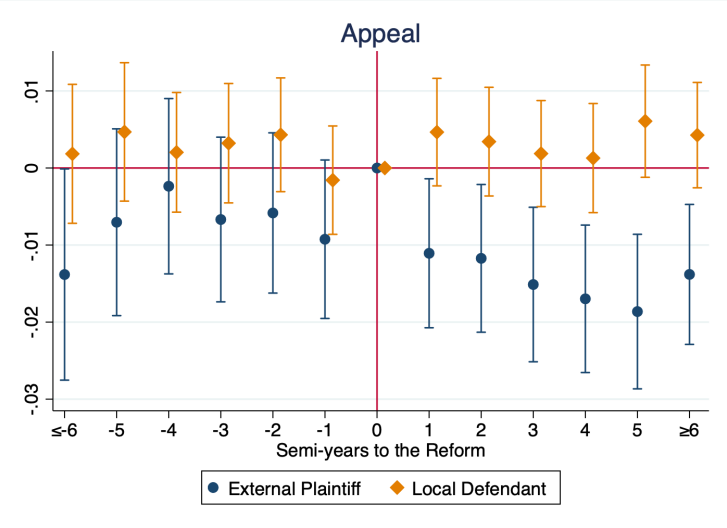


(a) Changes in Local Defendants' Win Rate

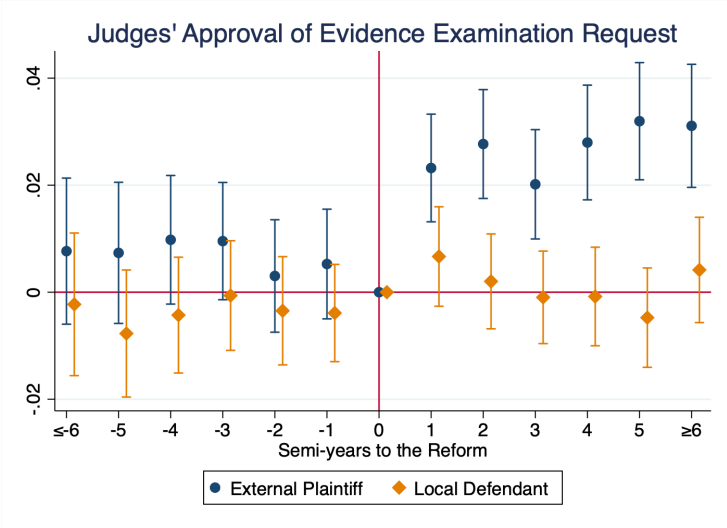
(b) Effect across Groups

Figure 3: Judicial Independence Reform and Judicial Impacts

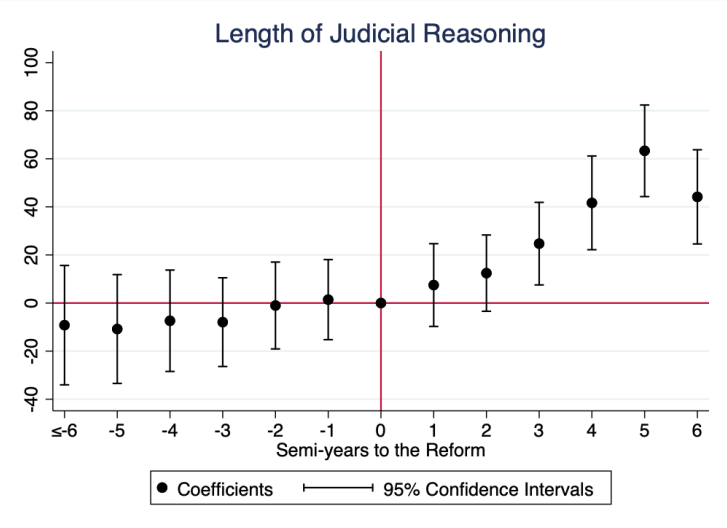
Notes: In Panel (a), we plot the event study coefficients (and the corresponding 95% confidence intervals) for the baseline results, following the approach suggested by Sun and Abraham (2021). In Panel (b), we estimate how the reform affected the likelihood of local defendants' win rates falling into different quartiles; the estimates plotted (as well as their 95% confidence intervals) come from four separate DiD regressions.



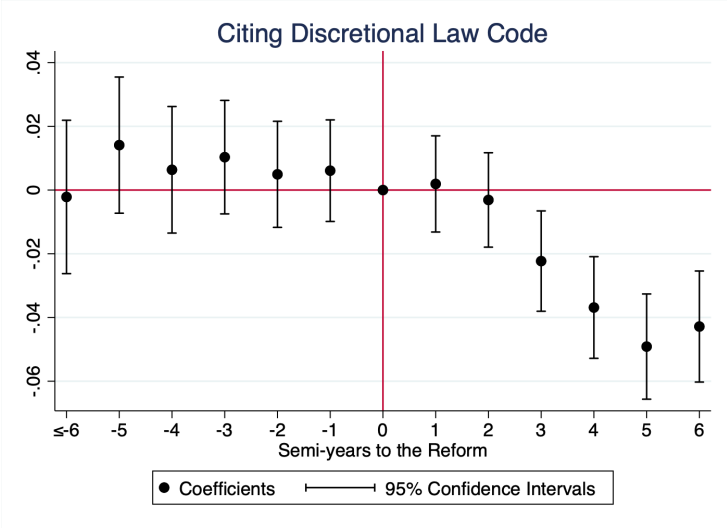
(a) Appeal



(b) Judges' Approval of Evidence Examination Request



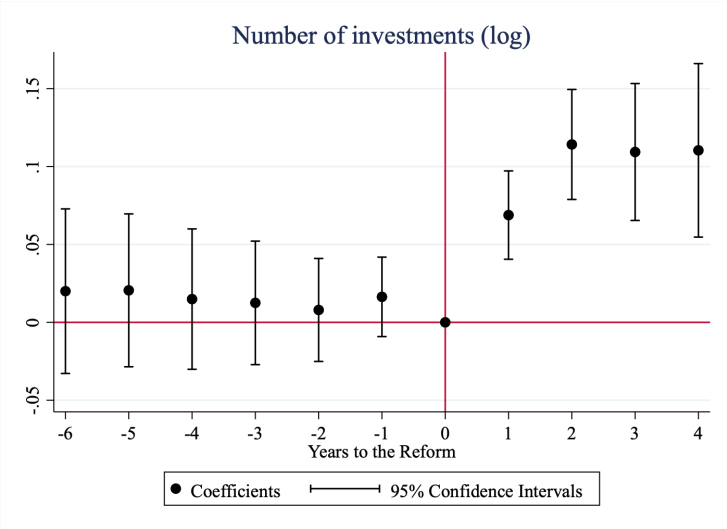
(c) Length of Judicial Reasoning



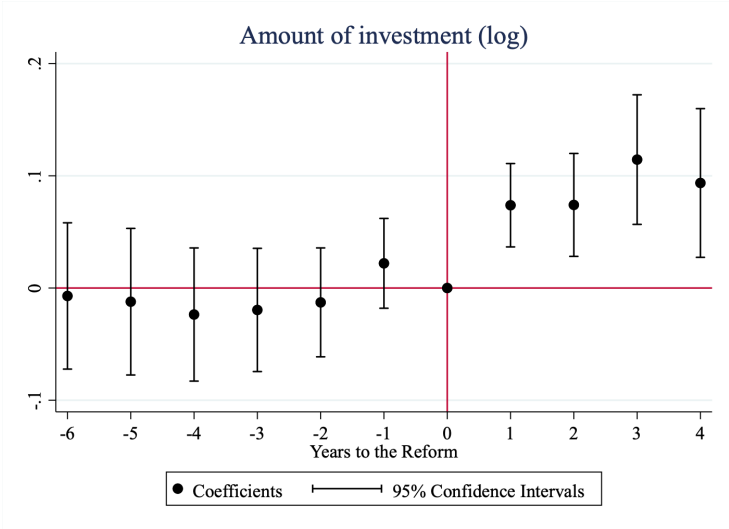
(d) Judge Citing Discretional Law Code

Figure 4: Judicial Independence Reform and Judicial Quality

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) Number of Investments



(b) Amount of Investment

Figure 5: Judicial Independence Reform and External Investment

Notes: Panel (a) plots the event study coefficients (as well as 95% confidence intervals) for the number of inward investment flows. Panel (b) plots the event study coefficients (as well as 95% confidence intervals) for the total amount of inward investment flows. All event studies are estimated following the approach suggested by Sun and Abraham (2021).

Table 1: 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	1,243,114	0.45	0.47	0.00	1.00
Politicially Connected Defendant (yes=1)	1,243,114	0.08	0.28	0.00	1.00
Plaintiff Appeal (yes=1)	1,243,114	0.06	0.24	0.00	1.00
Defendant Appeal (yes=1)	1,243,114	0.04	0.19	0.00	0.00
Appr. of Pltf's Evidence Exam. Req. (yes=1)	1,243,114	0.09	0.29	0.00	1.00
Appr. of Dfdt's Evidence Exam. Req. (yes=1)	1,243,114	0.08	0.24	0.00	1.00
Length of Judicial Reasoning	1,243,114	482.67	526.99	50.00	1,480.00
Citing Discretionary Law Code (yes=1)	1,243,114	0.25	0.43	0.00	1.00
Defaulter (yes=1)	1,243,114	0.02	0.15	0.00	0.00
Panel B. Firm litigant Characteristics					
Registered Capital (Million CNY)	2,308,787	82.70	144.77	1.00	360.00
Firm Age	2,791,424	9.70	6.32	2.00	22.31
Number of Employees	1,361,887	544.75	2,840.84	2.00	1,997.00
Panel C. Inter-county Investment Flow Data					
Number of External Investment	43,733	602.54	1,372.68	20.00	2,055.00
Amount of External Investment (100 Mill. CNY)	43,733	20.85	77.36	0.20	84.28

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. 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 2: Judicial Independence Reform and Local Defendants' Win Rate

	All Cases	Intra-Province	Inter-Province	Non-connected	Connected to Local	Connected to Provincial/Central
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reform	-0.031***	-0.044***	-0.023***	-0.030***	-0.073***	-0.005
	(0.005)	(0.006)	(0.006)	(0.005)	(0.012)	(0.022)
Mean of Outcome	0.44	0.40	0.47	0.43	0.48	0.56
Court FE	Y	Y	Y	Y	Y	Y
Seimi-year FE	Y	Y	Y	Y	Y	Y
Coeff. Equality Test P-Value		0.000		0.000	0.000	
Observations	46,907	43,472	38,408	46,615	17,576	8,036
R-Squared	0.212	0.185	0.198	0.211	0.264	0.347

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 all local defendants in all inter-regional commercial lawsuits. Columns (2) and (3) investigate the local defendants' average win rates in intra- and inter-provincial lawsuits, respectively. Columns (4), (5), and (6) investigate the average win rates of local defendants that are not politically connected, politically connected to county/prefectural governments, and provincial/central governments respectively. Number of observations change across columns since there are singletons for certain court-semiyear observations (e.g., some local courts do not have any connected local defendants in some semiyears). Case-level analysis yield similar outcomes. Standard errors clustered at the court level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table 3: Judicial Independence Reform and Judges' Decision Quality

	Appeal Rate		Evidence Examination		Judicial Reasoning	Discretionary Codes
	Plaintiff	Defendant	Plaintiff	Defendant		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reform	-0.010***	0.003	0.025***	0.003	34.518***	-0.035***
	(0.003)	(0.002)	(0.003)	(0.002)	(5.541)	(0.005)
Mean of Outcome	0.077	0.044	0.090	0.062	482.671	0.230
Court FE	Y	Y	Y	Y	Y	Y
Semi-year FE	Y	Y	Y	Y	Y	Y
Observations	46,907	46,907	46,907	46,907	46,905	46,907
R-Squared	0.158	0.136	0.147	0.152	0.291	0.263

Notes: This table reports the impacts of the judicial independence 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 request by 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. Standard errors clustered at the court level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table 4: Intensive Margin - Conditional on Case Composition

	Local Defendant's Win Rate	
	(1)	(2)
Post Reform	-0.071***	-0.080***
	(0.017)	(0.021)
Mean of Outcome	0.45	0.45
Court FE	Y	N
Semi-year FE	Y	Y
Judge FE	N	Y
Observations	38,875	32,244
R-Squared	0.209	0.495

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

Table 5: Extensive Margin - Changes in Case Compositions After Reform

	Share of External Plaintiff Cases	Regis. Capital (Million)		# of Employees		Age	
		Plaintiff	Defendant	Plaintiff	Defendant	Plaintiff	Defendant
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post Reform	0.027***	-8.786***	8.796***	-115.759**	82.697***	-0.954***	0.555***
	(0.004)	(2.200)	(1.600)	(56.649)	(28.131)	(0.085)	(0.077)
Mean of Outcome	0.33	86.09	79.80	590.95	508.33	9.36	9.97
Court FE	Y	Y	Y	Y	Y	Y	Y
Seimi-year FE	Y	Y	Y	Y	Y	Y	Y
Observations	46,907	1,064,215	1,242,824	602,175	758,559	1,255,078	1,534,289
R-Squared	0.615	0.047	0.043	0.066	0.043	0.067	0.068

Notes: This table reports the impacts of the judicial independence reform on the composition of commercial lawsuits. Column (1) presents the DiD estimate for the number 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 court level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table 6: Judicial Independence Reform and External Investment

	Number of Investment (log)	Amount of Investment (log)
	(1)	(3)
Post Reform	0.084***	0.080***
	(0.017)	(0.019)
Mean of Outcome	5.504	10.718
County FE	Y	Y
Year FE	Y	Y
Observations	42,437	42,437
R-Squared	0.860	0.799

Notes: This table reports the impacts of the judicial independence reform on inward investments at the county level. Column (1) reports the DiD estimate for the number of inward investments. Column (2) reports the DiD estimate for the total amount of inward investments. Standard errors clustered at the county level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table 7: Judicial Independence Reform and External Investment - Cross-location Heterogeneity

	(1)	(2)	(3)	(4)
Panel A				
	Number of Investment (log)			
	Same Prefecture	Outside Prefecture	Outside Province	Same County
Post Reform	0.116***	0.018	0.006	-0.015
	(0.017)	(0.016)	(0.016)	(0.017)
Mean of Outcome	5.103	3.581	4.016	6.709
Observations	42,291	41,690	41,921	42,611
R-Squared	0.853	0.840	0.852	0.882
Panel B				
	Amount of Investment (log)			
	Same Prefecture	Outside Prefecture	Outside Province	Same County
Post Reform	0.174***	0.035	-0.016	-0.018
	(0.019)	(0.025)	(0.024)	(0.017)
Mean of Outcome	9.798	8.687	9.418	12.204
Observations	42,291	41,690	41,921	42,611
R-Squared	0.820	0.658	0.718	0.846
County FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Notes: This table reports the impacts of the reform on different types of investments. Panel A shows results for the number of investments, Panel B shows results for the total amount 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 county level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Appendix A APPENDIX

ONLINE APPENDIX

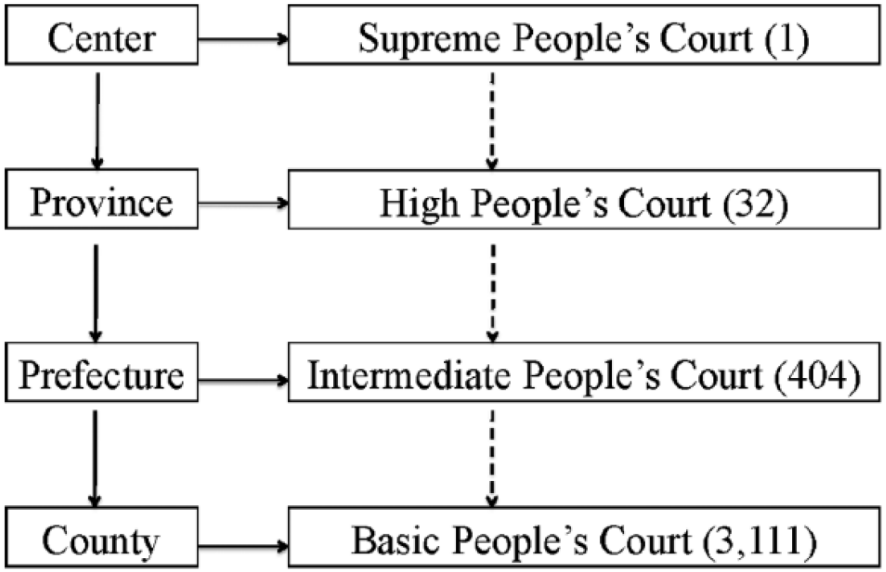
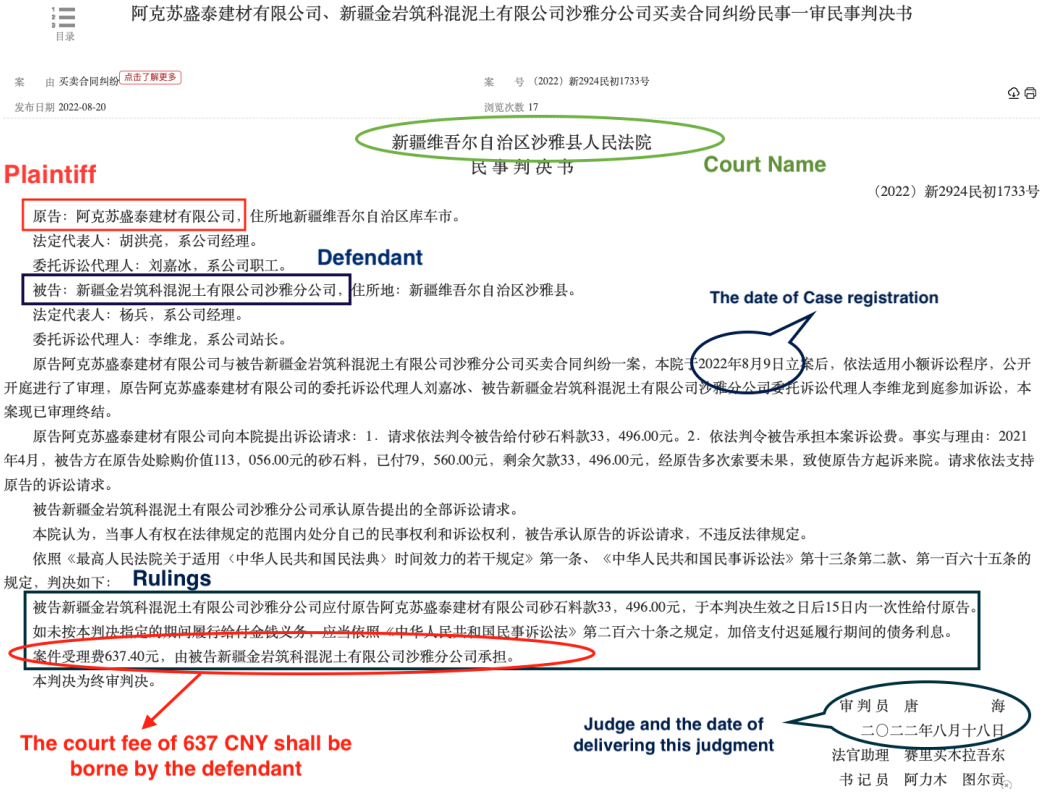


Figure A.1: Structure of China's Judicial System

Notes: Reprinted from Wang (2018).



(a) Frontpage of the China Judgements Online Website



(b) Sample Court Judgement

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

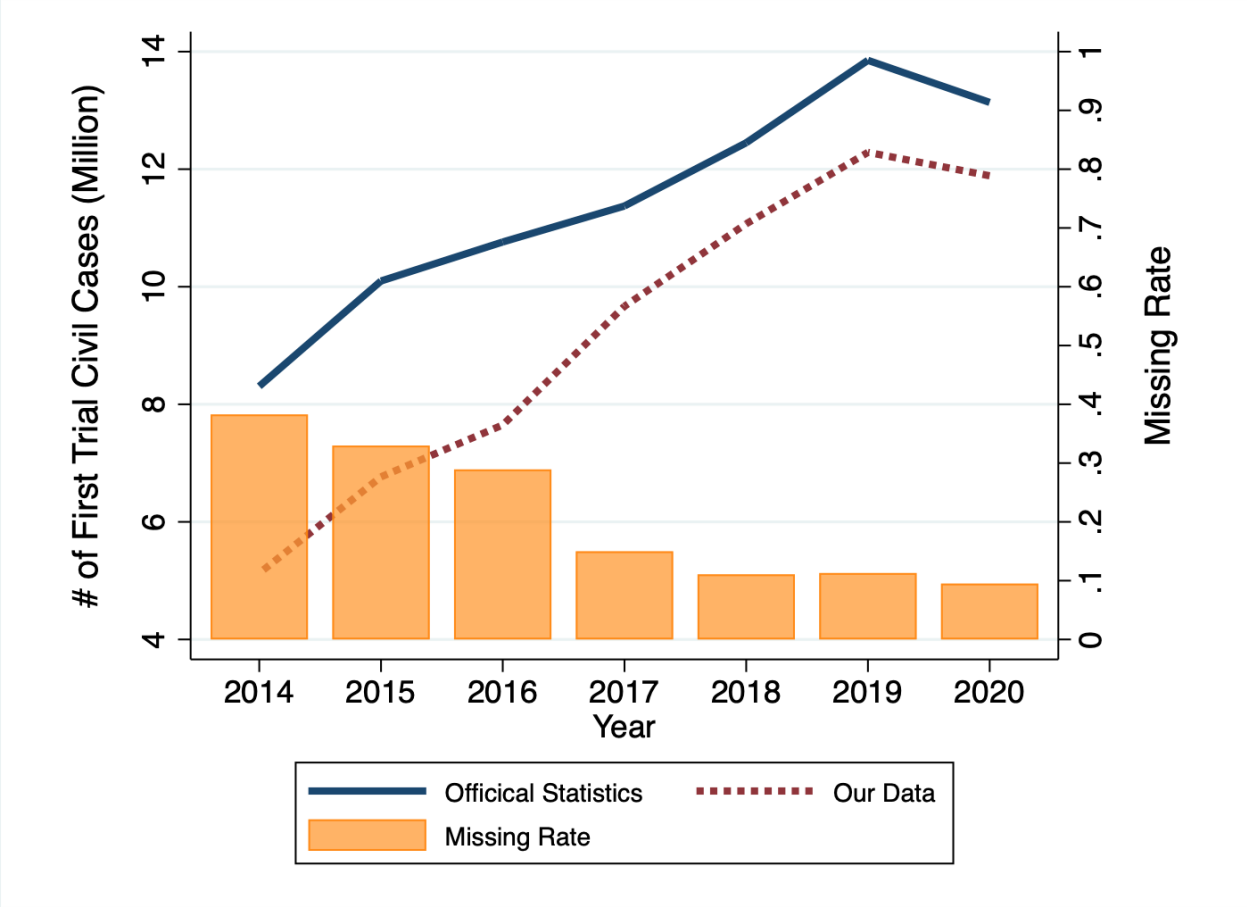
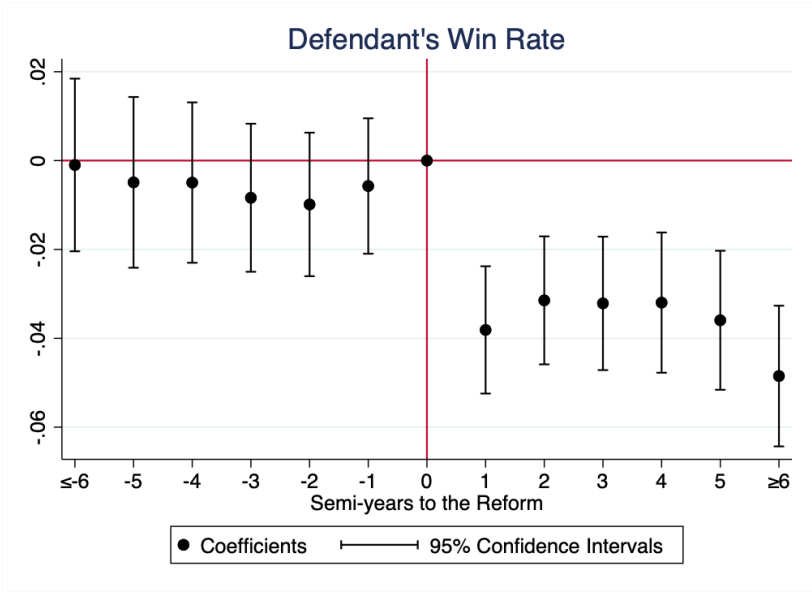


Figure A.3: Missing Rate of First Trial Court Verdicts

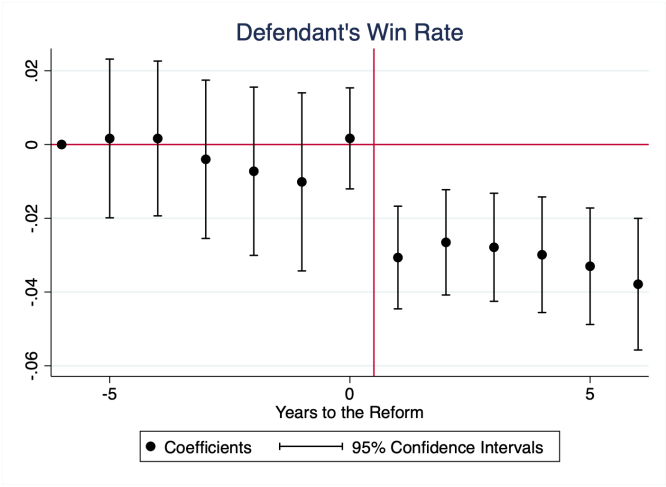
Notes: The official number of first trial civil cases is retrieved from *China Statistical Yearbook* published by National Bureau of Statistics between 2015 to 2021, while the number of first trial civil cases in our data is calculated using verdicts that were trialed between 2014 and 2020 and released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



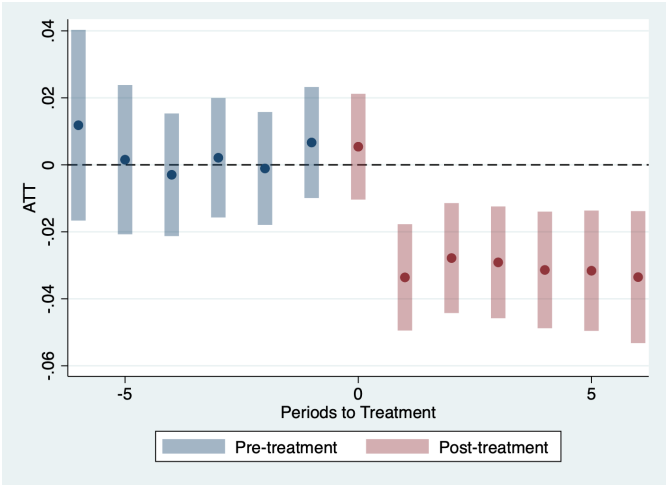
Figure A.4: Frontpage of the Tianyancha.com



(a) Unadjusted Event Study



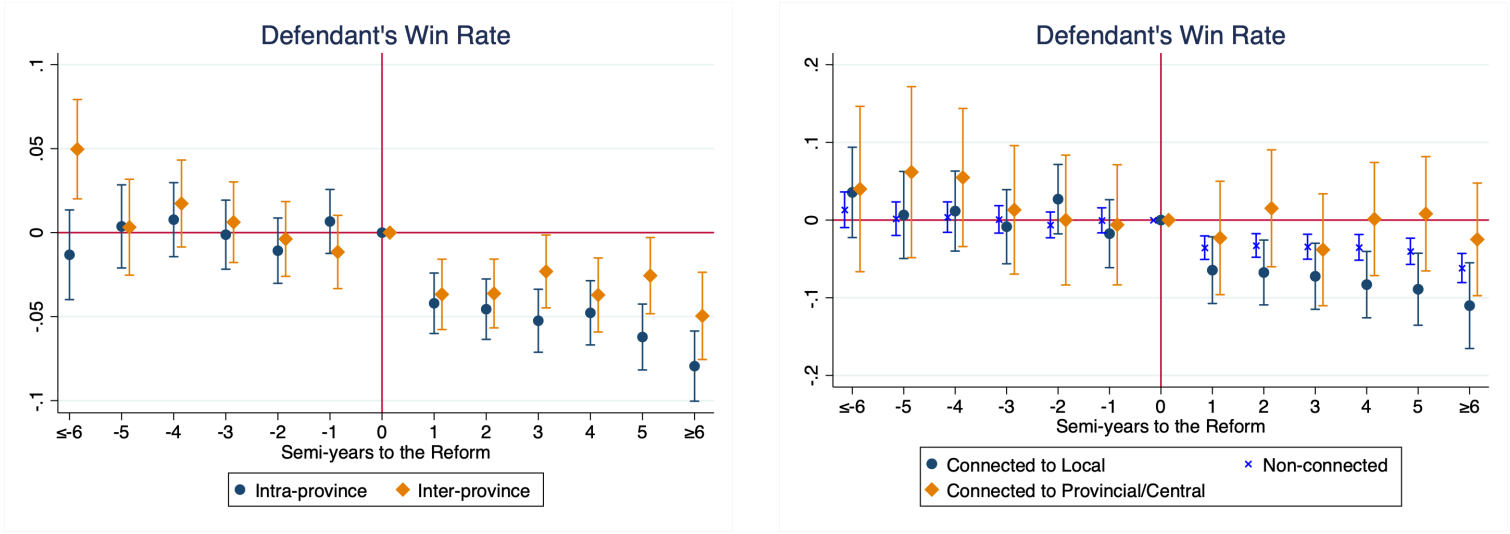
(b) Adjusted following Borusyak et al., 2021



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

Figure A.5: Judicial Independence Reform and Judicial Impacts - Alternative Estimators

Notes: Panel (a) plots the baseline event study coefficients (as well as 95% confidence intervals), with no additional adjustments. Panel (a) plots the baseline event study coefficients (as well as 95% confidence intervals), following the approach suggested by Borusyak et al. (2021). Panel (c) plots the baseline event study coefficients (as well as 95% confidence intervals), following the approach suggested by Callaway and Sant'Anna (2021).



(a) Intra-province v.s Inter-province

(b) Connected v.s.Non-Connected Local Defendant

Figure A.6: Judicial Independence Reform and Judicial Impacts - Heterogeneity

Notes: Panel (a) plots the event study estimates corresponding to Columns (2) and (3) of Table 2, Panel (b) plots the event study estimates corresponding to Columns (4), (5) and (6) of Table 2. All event studies are estimated following the approach suggested by Sun and Abraham (2021).

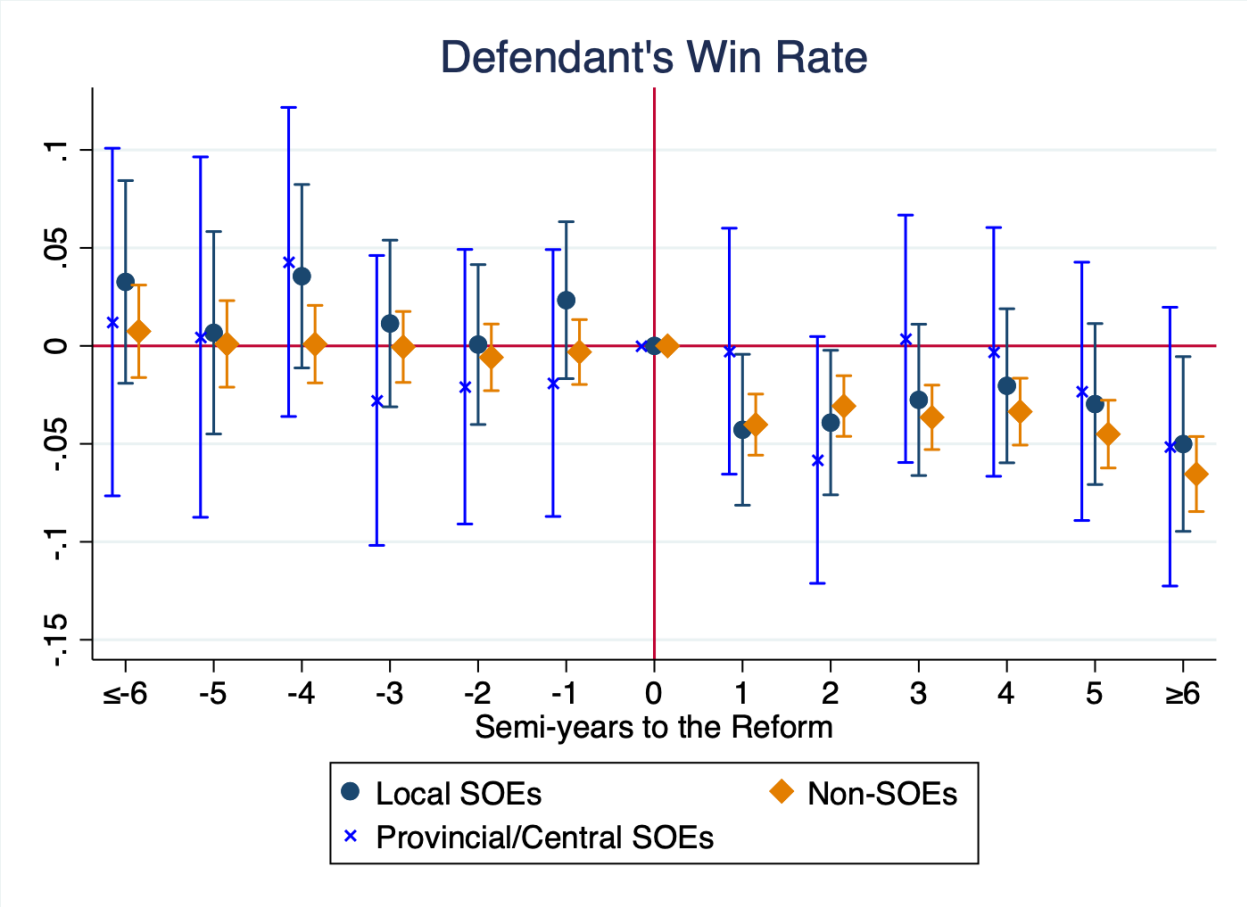


Figure A.7: Judicial Independence Reform and Judicial Impacts - Heterogeneity

Notes: This figure plots the event study coefficients (as well as 95% confidence intervals) corresponding to Columns (1), (2) and (3) of Table A.5 following Sun and Abraham (2021).

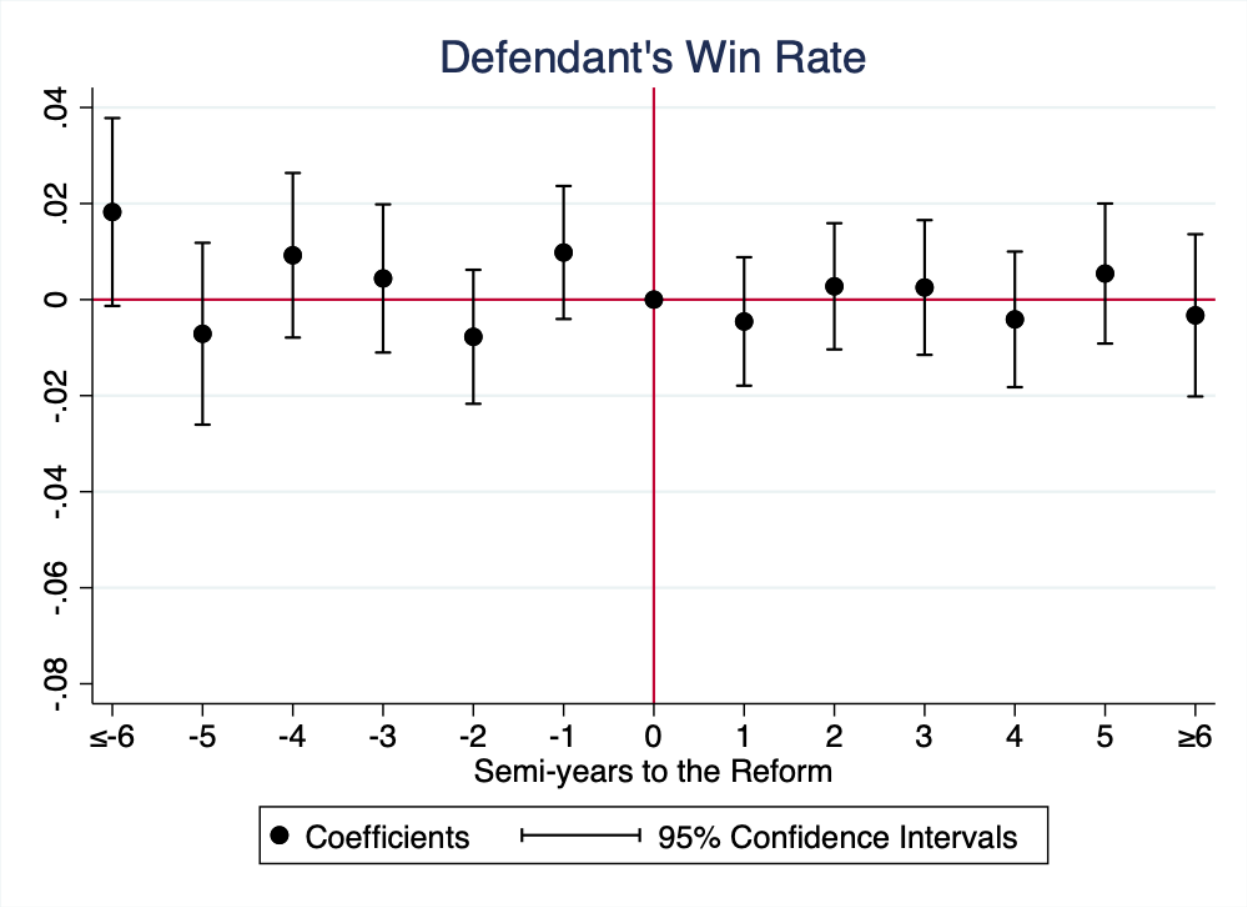


Figure A.8: Placebo Test Using Civil Lawsuits Between Local Firms

Notes: This figure plots the event study coefficients (as well as 95% confidence intervals) corresponding to Column (1) of Table A.4 following Sun and Abraham (2021).

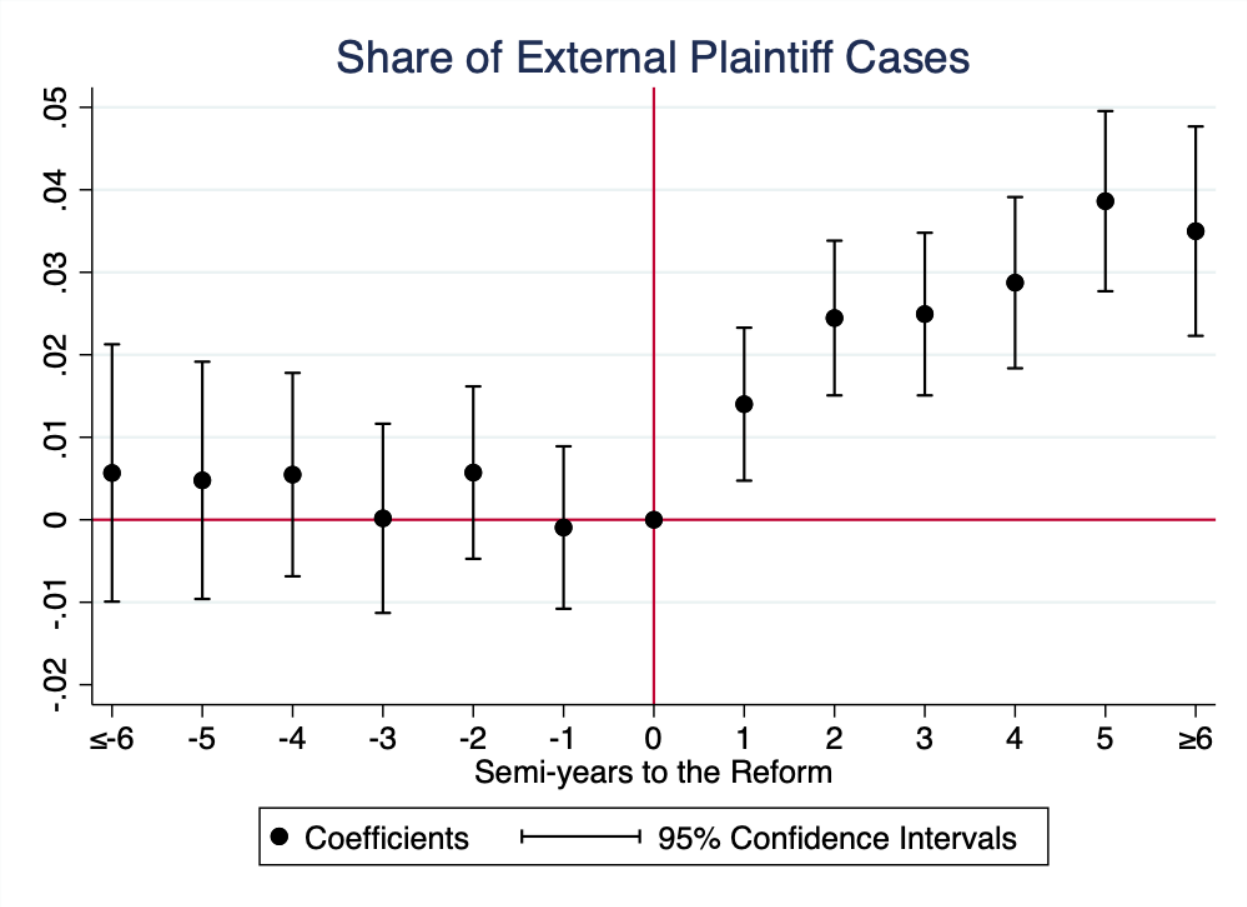


Figure A.9: Judicial Independence Reform and Share of External Plaintiff v.s. Local Defendant Cases

Notes: This figure 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 following Sun and Abraham (2021).

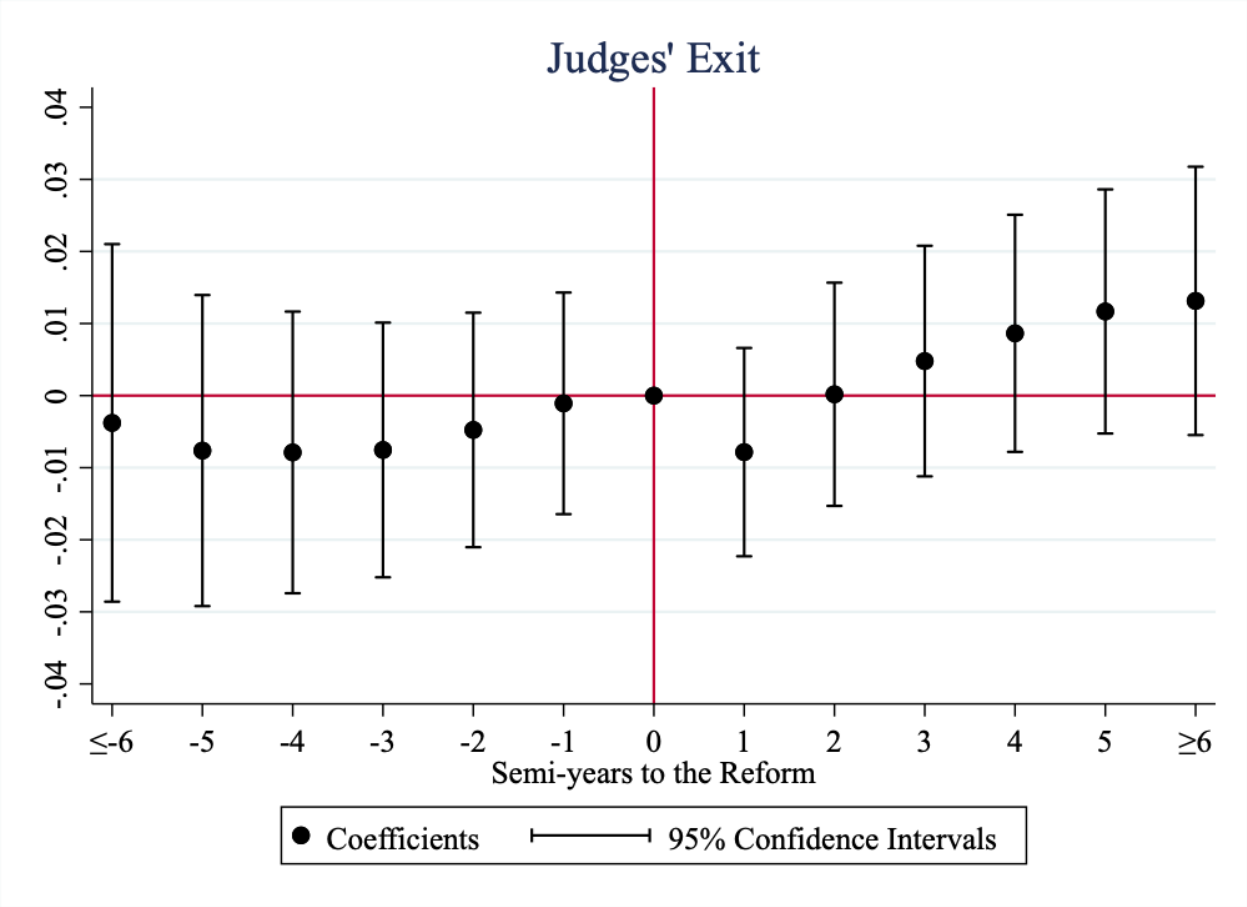


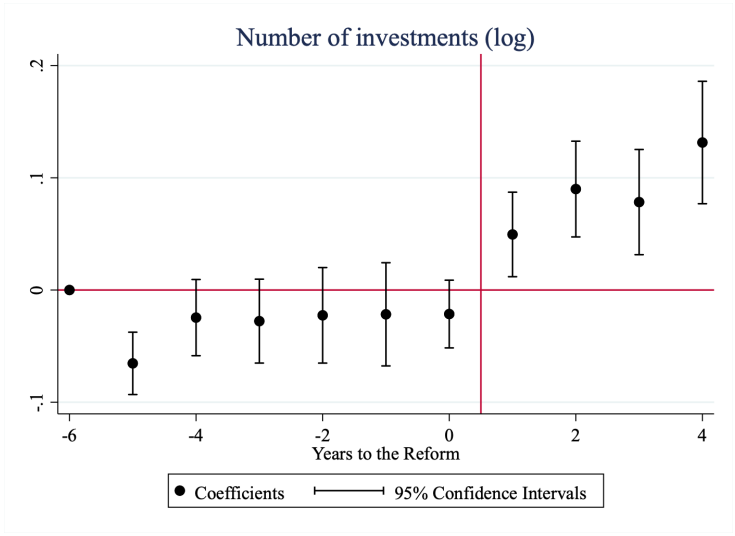
Figure A.10: Judicial Independence Reform and Judges' Exit

Notes: This figure plots the event study coefficients (as well as 95% confidence intervals) using the probability of judges' exit as outcome variable following Sun and Abraham (2021).

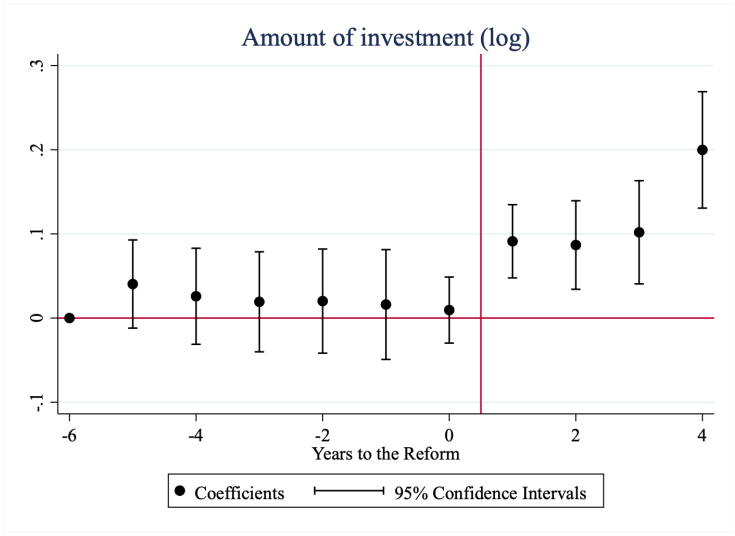


Figure A.11: Judicial Independence Reform and Trial Speed

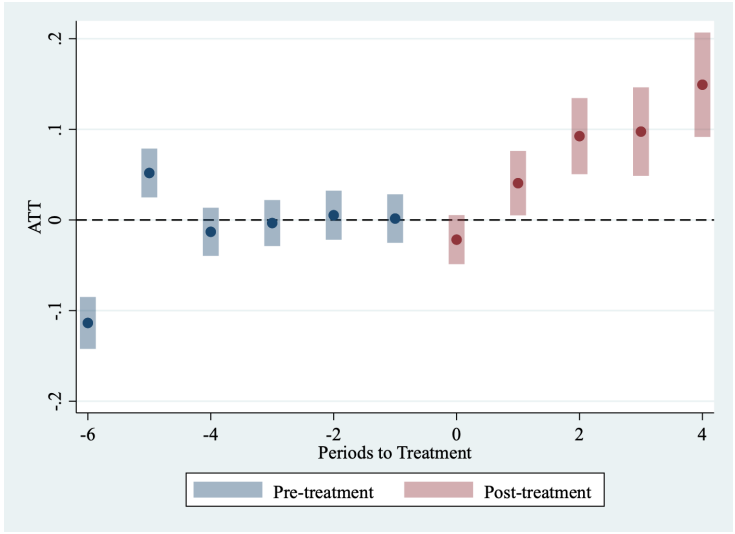
Notes: This figure plots the event study coefficients (as well as 95% confidence intervals) using the duration of a case (number of days from filing to verdict) as outcome variable following Sun and Abraham (2021).



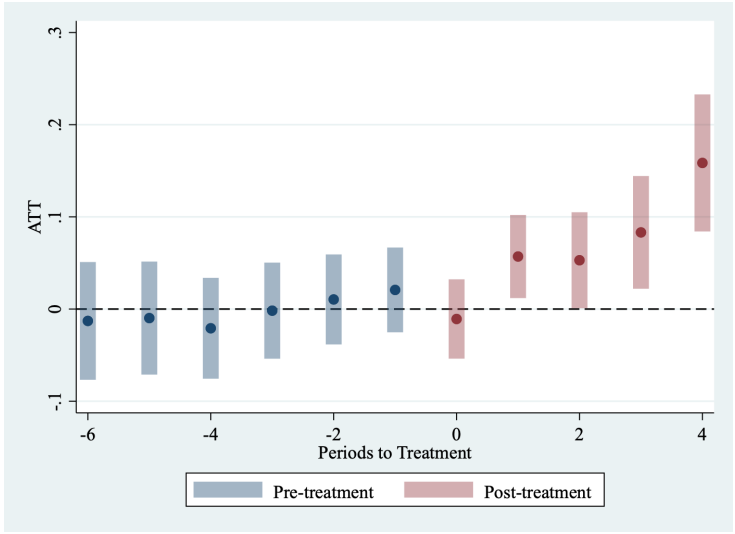
(a) Number of External Investments



(b) Amount of External Investment



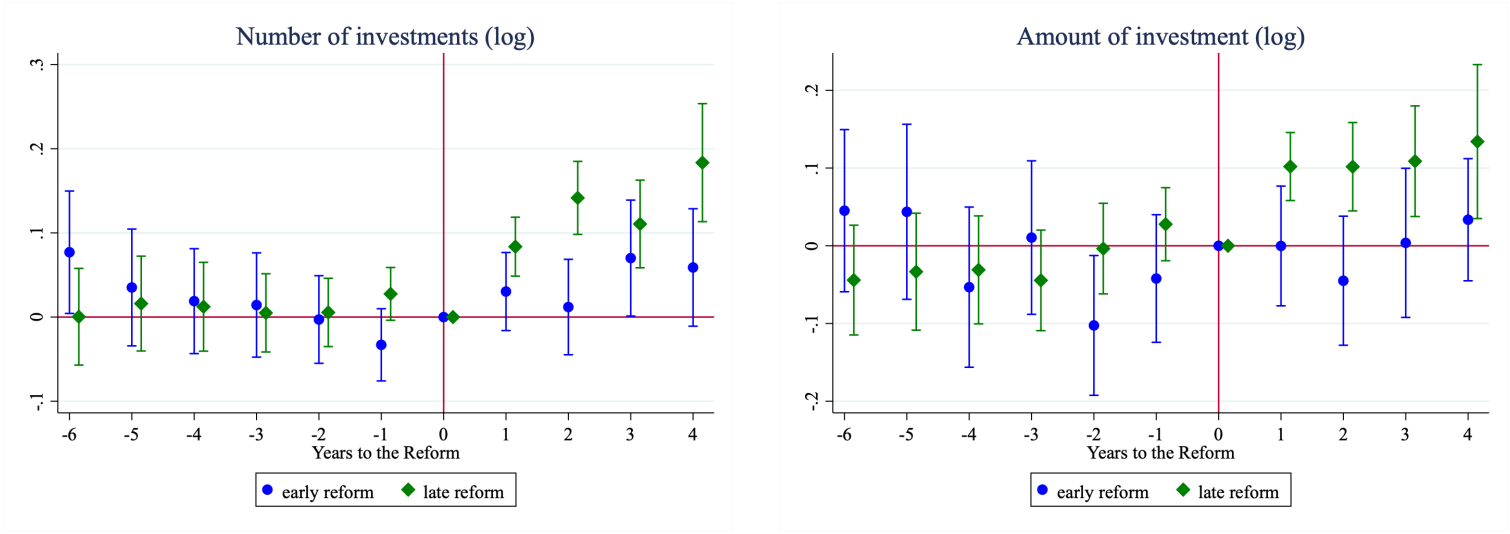
(c) Number of External Investments



(d) Amount of External Investment

Figure A.12: Judicial Independence Reform and External Investment

Notes: Panels (a) and (b) plot the event study estimates corresponding to Table 6, following the approach suggested by Borusyak et al. (2021). Panels (c) and (d) plot the event study estimates corresponding to Table 6, following the approach suggested by Callaway and Sant'Anna (2021).

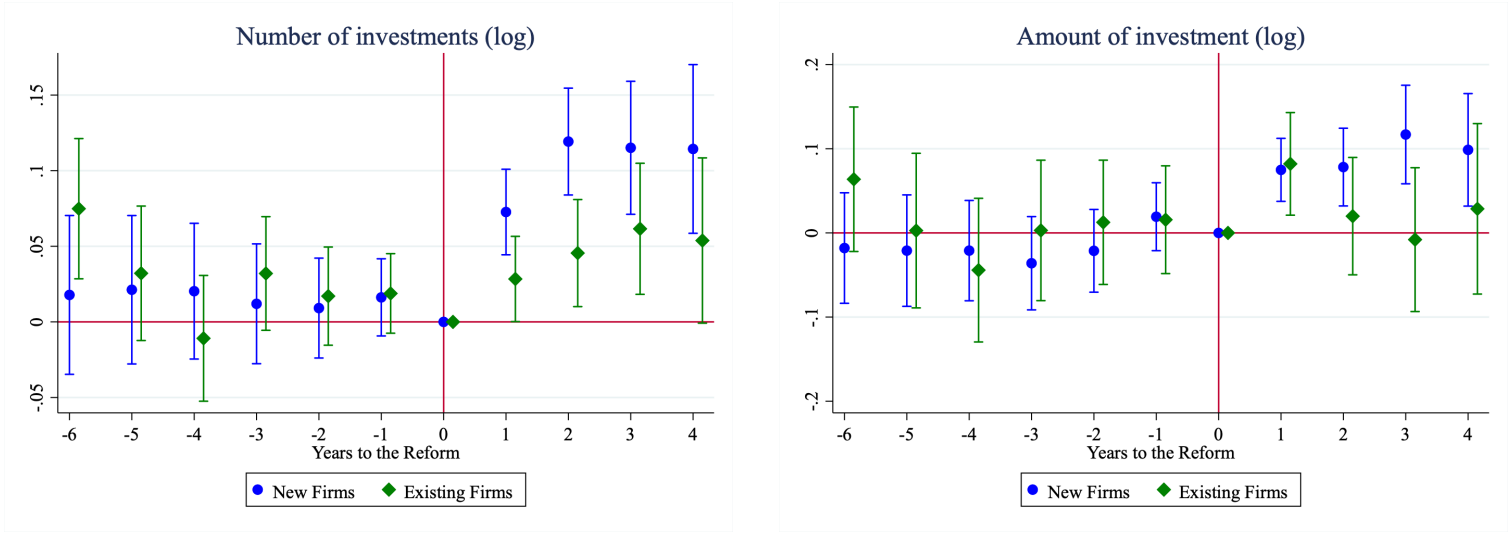


(a) Number of External Investments

(b) Amount of External Investment

Figure A.13: Early Reform Regions vs. Late Reform Regions

Notes: Panels (a) and (b) plot the event study estimates corresponding to Columns (1) and (2) of Table A.9. Panel (a) presents the number of inward investments separately for early reform regions (reformed before 2016) and late reform regions (reformed after 2016), following the approach suggested by Sun and Abraham (2021). Panel (b) presents the total amount of inward investments separately for early reform regions (reformed before 2016) and late reform regions (reformed after 2016), following the approach suggested by Sun and Abraham (2021).

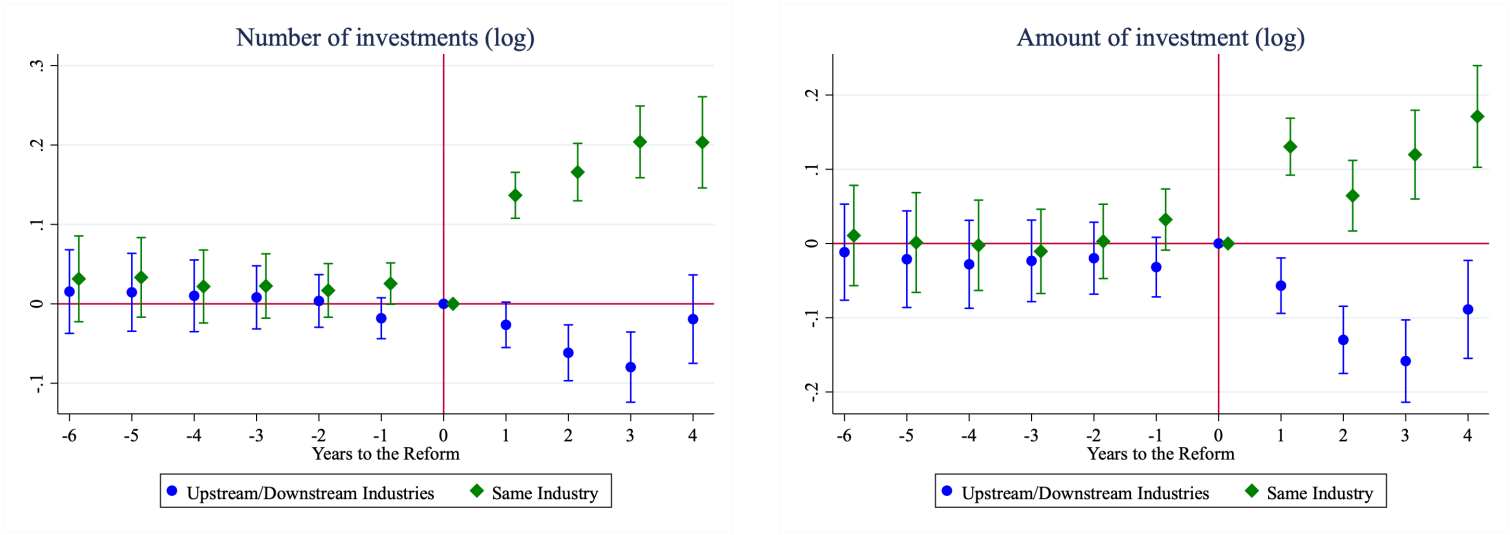


(a) Number of External Investments

(b) Amount of External Investment

Figure A.14: New Entry vs. Joint Venture Formation

Notes: Corresponding to Columns (3) and (4) of Table A.9, these two figures plot the event study estimates for non-local firms investing in new local branches (new entry) vs. investing in existing local firms (joint venture), following the approach suggested by Sun and Abraham (2021). Panel (a) focuses on the number of investments, Panel (b) focuses on the total amount of investments.

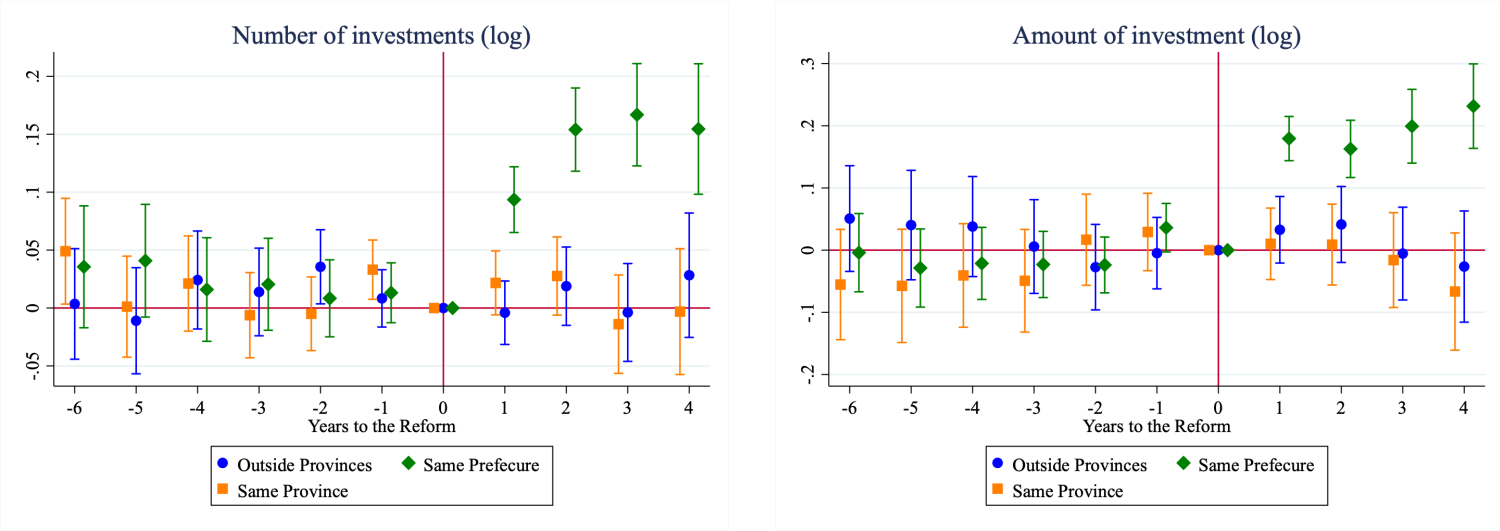


(a) Number of External Investments

(b) Amount of External Investment

Figure A.15: Investing in Own Industry vs. Investing in Complementary Industries

Notes: Corresponding to Columns (5) and (6) of Table A.9, 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, following the approach suggested by Sun and Abraham (2021). Panel (a) focuses on the number of investments, Panel (b) focuses on the total amount of investments.

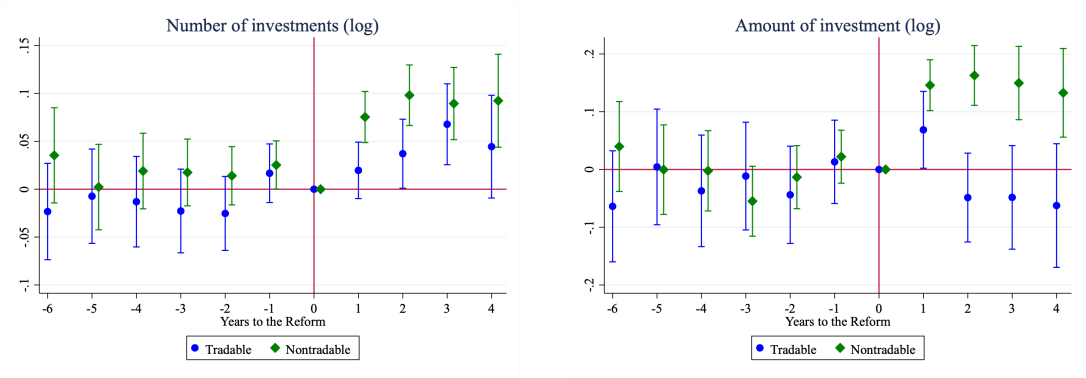


(a) Number of External Investments

(b) Amount of External Investment

Figure A.16: Judicial Independence Reform and External Investment

Notes: Panel (a) plots the event study estimates corresponding to Columns (1) to (3) in Panel A of Table 7, following the approach suggested by Sun and Abraham (2021). Panel (b) plots the event study estimates corresponding to Columns (1) to (3) in Panel B of Table 7 , following the approach suggested by Sun and Abraham (2021).



(a) Tradable v.s. Non-tradable **(b)** Tradable v.s. Non-tradable

Figure A.17: Judicial Independence Reform and External Investment - Heterogeneity

Notes: Panels (a) and (b) plot the event study estimates corresponding to Columns (7) and (8) of Table A.9, following the approach suggested by Sun and Abraham (2021). Panels (c) and (d) plot the event study estimates corresponding to Columns (7) and (8) of Table A.9, following the approach suggested by Sun and Abraham (2021).

Table A.1: Judicial Independence 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 independence 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.2: Judicial Independence Reform and Local Defendants Win Rate (Case-level Analysis)

	All Cases	Intra-Province	Inter-Province	Non-connected	Connected to Local	Connected to Provincial/Central
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Post Reform	-0.040***	-0.058***	-0.013	-0.039***	-0.067***	-0.008
	(0.008)	(0.010)	(0.011)	(0.008)	(0.014)	(0.019)
Court FE	Y	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y	Y
Observations	1,191,854	596,261	595,498	1,089,773	82,306	18,762
R-Squared	0.080	0.069	0.114	0.083	0.117	0.185
Panel B						
Post Reform	-0.031***	-0.049***	-0.004	-0.030***	-0.051***	0.014
	(0.006)	(0.008)	(0.010)	(0.006)	(0.014)	(0.029)
Judge FE	Y	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y	Y
Observations	1,166,520	571,043	572,419	1,064,423	68,410	11,631
R-Squared	0.258	0.268	0.321	0.267	0.403	0.494
Mean of Outcome	0.44	0.40	0.47	0.43	0.48	0.56

Notes: This table replicates Table 2 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 court level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.3: Judicial Independence Reform and Local Defendants Win Rate (Semi-parametric DiD estimators)

	All Cases	Intra-Province	Inter-Province	Non-connected	Connected to Local	Connected to Provincial/Central
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reform	-0.030***	-0.049***	-0.015*	-0.030***	-0.049**	-0.077
	(0.007)	(0.008)	(0.009)	(0.007)	(0.021)	(0.165)
Mean of Outcome	0.44	0.40	0.47	0.43	0.48	0.56
Court FE	Y	Y	Y	Y	Y	Y
Seimi-year FE	Y	Y	Y	Y	Y	Y

Notes: This table replicates Table 2 by estimating the two-way fixed effect model (Equation 1) following the approach suggested by Callaway and Sant'Anna, 2021. Standard errors clustered at the court level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.4: Placebo Test Using Civil Lawsuits Between Local Firms

	All Cases	Connected	Non-connected
	(1)	(2)	(3)
Post Reform	0.007	-0.018**	0.007
	(0.005)	(0.009)	(0.005)
Mean of Outcome	0.39	0.45	0.38
Court FE	Y	Y	Y
Seimi-year FE	Y	Y	Y
Observations	51,393	25,396	51,076
R-Squared	0.243	0.250	0.242

Notes: This table replicates Table 2 using the civil lawsuits between local firms. Standard errors clustered at the court level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.5: Judicial Independence Reform and Local Defendants Win Rate- Heterogeneity

	Non-SOEs	Local SOEs	Provincial/Central SOEs
	(1)	(2)	(3)
Post Reform	-0.030***	-0.031***	0.003
	(0.005)	(0.011)	(0.020)
Mean of Outcome	0.43	0.46	0.54
Court FE	Y	Y	Y
Seimi-year FE	Y	Y	Y
Observations	46,192	20,602	9,904
R-Squared	0.209	0.250	0.338

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. Columns (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. Columns (3) investigate the average win rates of SOE defendants that are owned by provincial/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 court level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.6: Judicial Independence Reform and Appeal Rate

	Appeal Rate When Losing	
	Plaintiff	Defendant
	(1)	(2)
Post Reform	-0.006***	0.002
	(0.002)	(0.002)
Mean of Outcome	0.038	0.030
Court FE	Y	Y
Seimi-year FE	Y	Y
Observations	46,907	46,907
R-Squared	0.141	0.127

Notes: This table reporths the baseline DiD estimatees 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) investigate 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 court level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.7: 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	-5.355	-34.526	75.438	-233.305	-0.398	0.295
	(26.597)	(21.765)	(84.187)	(327.923)	(0.331)	(0.282)
Mean of Outcome	246.17	241.14	501.27	677.71	10.74	10.63
Court FE	Y	Y	Y	Y	Y	Y
Seimi-year FE	Y	Y	Y	Y	Y	Y
Observations	24,935	34,163	21,004	22,770	31,981	43,373
R-Squared	0.149	0.126	0.369	0.185	0.140	0.120

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

Table A.8: Judicial Independence Reform and Ruling Enforcement

	Non-compliance Rate	Complete Non-compliance	Partial Non-compliance
	(1)	(2)	(3)
Post Reform	0.001	0.001	-0.000
	(0.001)	(0.001)	(0.001)
Mean of Outcome	0.024	0.020	0.006
Court FE	Y	Y	Y
Seimi-year FE	Y	Y	Y
Observations	46,907	46,893	46,830
R-Squared	0.178	0.158	0.129

Notes: This table reports the impacts of the judicial independence 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 court level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.9: Judicial Independence Reform and External Investment - Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
	Number of Investment (log)							
	Early-reform	Late-reform	New Firms	Existing Firms	Complementary Industries	Same Industry	Tradable	Non-tradable
Post Reform	0.035	0.107***	0.089***	0.038**	-0.045***	0.149***	0.047***	0.068***
	(0.032)	(0.019)	(0.017)	(0.016)	(0.017)	(0.017)	(0.017)	(0.016)
Observations	19,995	34,740	42,417	41,265	42,421	42,396	40,067	41,069
R-Squared	0.876	0.862	0.860	0.824	0.857	0.851	0.821	0.887
Coeff. Equality Test P-Value	0.010		0.000		0.000		0.010	
Panel B								
	Amount of Investment (log)							
	Early-reform	Late-reform	New Firms	Existing Firms	Complementary Industries	Same Industry	Tradable	Non-tradable
Post Reform	0.013	0.123***	0.087***	0.037	-0.081***	0.096***	0.026	0.110***
	(0.031)	(0.023)	(0.019)	(0.025)	(0.019)	(0.020)	(0.027)	(0.023)
Observations	19,995	34,740	42,417	41,265	42,421	42,396	40,067	41,069
R-Squared	0.817	0.798	0.795	0.686	0.796	0.788	0.615	0.760
Coeff. Equality Test P-Value	0.000		0.000		0.000		0.000	
County FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	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. Columns (1) and (2) investigate investments in early-reform regions and late-reform regions, respetively. Columns (3) and (4) investigate investments in new entries and joint ventures, respectively. Column (5) 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 (6) shows the results for investments in same industry. Columns (7) and (8) investigate investments in tradable and non-tradable sectors, respectively. Standard errors clustered at the county level are reported below the coefficients. * significant at 10% ** significant at 5% *** significant at 1%.

Table A.10: Judicial Independence Reform and Local Investment - Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
	Number of Investment (log)					
	New Firms	Existing Firms	Complementary Industries	Same Industry	Tradable	Non-tradable
Post Reform	-0.014	-0.026	-0.004	-0.007	-0.010	0.019
	(0.017)	(0.016)	(0.016)	(0.017)	(0.017)	(0.016)
Observations	42,597	41,963	41,791	42,569	41,391	41,755
R-Squared	0.882	0.866	0.879	0.887	0.851	0.909
Panel B						
	Amount of Investment (log)					
	New Firms	Existing Firms	Complementary Industries	Same Industry	Tradable	Non-tradable
Post Reform	-0.015	-0.016	0.004	0.001	0.008	-0.014
	(0.017)	(0.021)	(0.023)	(0.017)	(0.024)	(0.020)
Observations	42,597	41,963	41,791	42,569	41,391	41,755
R-Squared	0.844	0.762	0.741	0.852	0.713	0.828
County FE	Y	Y	Y	Y	Y	Y
Year FE	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 county 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

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

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

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

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 $\{\xi_n\}_n$. 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 $\{\xi_n\}$ 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 $\pi_n\left(\varphi\right)$ is the variable profits:

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

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 7, 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 7 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.