

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 2.3% 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, China did not have independent judiciaries. 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 (SPC) once stated that "law

was taken by some local officials as a tool to protect parochial interests."¹ Second, since 2014, the SPC 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% 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.

The judicial reform affects local defendants' win rate through two channels: (a) it

¹See Zheng (1994), p.472.

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. If anything, 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.

Taken together, our analysis of civil lawsuits indicates that the judicial independence reform systematically reduced local protectionism in China's judicial system. 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 region received an additional 8.4% of annual investment from non-local investors (including both firms and individual investors from other regions), suggesting that the judicial independence reform has played important roles in facilitating economic integration in China.

We build a simple model of external entrepreneurial investments à la Melitz (2003). In the model, a judicial reform reduces protectionism-induced distortions and reduces 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 estimates indicate that, when the judicial independence reform is implemented nationwide, the overall eco-

conomic gains from improved cross-regional economic integration will be as high as 2.3% 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 get timely support (Bai et al., 2020a). As the authoritarian regime has consolidated its political control over the past decade, it is generally believed to have turned further

²In 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).

³Our 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).

against the rule of law, leaving the government’s political power unbound by the judicial 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.⁴ 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 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

⁴Indeed, our findings suggest that greater judicial independence since the reform has significantly reshaped the inter-business and state-business relationships in China.

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 (SPC); 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.⁵ 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 SPC.⁶

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

⁵The percentage number is calculated using the raw data provided by China Judgments Online, see <https://wenshu.court.gov.cn/>

⁶For 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.

rules, which are suggestive and non-binding for the local courts. In contrast, de facto control over the local courts is held by the corresponding levels of local governments. Specifically, for each local court, its budget, which includes salaries and bonuses for the judges and court clerks and covers other court operational costs, needs to be approved by the corresponding local government. Similarly, the local government (the 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.⁷

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 SPC (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 SPC immediately before the judicial independence reform in 2014, 68% of the local judge respondents listed local protectionism as the major reason for biased rulings.⁸ 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."⁹

Following the central government's guidelines, in 2014, the SPC formally launched the high-stakes judicial reform, which aims to systematically improve judicial independence by decoupling local courts from local governments. As shown in Figure 1b, the reform deprives the prefectural and county governments of their financial and personnel controls over the corresponding local courts; instead, all such controls are consolidated

⁷For 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>

⁸See: <https://www.cecc.gov/judicial-independence-in-the-prc>

⁹Source: http://www.gov.cn/jrzq/2013-11/15/content_2528179.htm

with the provincial governments. After this reform, prefectural and county governments no longer hold much leverage over local courts, thereby significantly improving local courts' independence. The SPC 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 worth noting that, due to administrative feasibility considerations, the judicial independence reform did not consolidate personnel and financial controls all the way to the central government, but instead stopped at the provincial level. This implies that one would expect heterogeneity in the reform's impact on local protectionism: if two firms involved in a lawsuit are from different prefectures within the same province, the provincial government should regard both firms as "local" and thus have no systematic incentives to protect one over the other. However, if a plaintiff outside the province sues a local firm, to the extent that the provincial government favors the local firm, it may still pressure the local courts for protectionist rulings even after the reform.¹⁰ We will test for this heterogeneity in Section 4.

The judicial independence reform had a staggered roll-out, as shown in Figure 2a. One hundred fifty-two local courts were selected as pilot sites in 2014, before the reform was gradually implemented in the rest of the country. Policy experimentation via staggered roll-out is a common feature in China's high-stakes policy reforms, in order to reduce policy uncertainty and explore optimal protocols for implementation (Wang and Yang, 2021). By the end of 2021, more than 70% of China's 3500 local courts had undergone the reform. 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.

¹⁰To the extent that it is more difficult for a local firm to capture the provincial government compared to prefectural/county governments, we would expect this to affect inter-provincial lawsuits, albeit to a lesser extent than inter-prefectural/county ones.

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 SPC 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.¹¹ 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.

¹¹We kept all court verdicts for cases that were tried between 2014 and 2021 and released by the *China Judgements 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.¹² 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: $WinRate_j = \frac{CourtFee_i}{CourtFee_i + CourtFee_j}$.¹³ 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;¹⁴ (b) the court approval rate of requests to examine evidence or invite an expert witness, for both plaintiff and defendant;¹⁵ (c) the richness of judicial reasoning in the judgment file (measured by word count);¹⁶ and (d) the frequency of citing discretionary codes in the judgment file.¹⁷

A potential caveat of the CJO data is incomplete disclosure by local courts, which could occur 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.¹⁸

¹²For 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%.

¹³Another 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.

¹⁴A lower appeal rate is commonly used as a proxy for higher judicial quality (Baye and Wright, 2011).

¹⁵Allowing forensic evidence examination is associated with more fair trials (Edmond and Roberts, 2011).

¹⁶Longer judicial reasoning has been documented to correlate with decision quality (Liu, 2018).

¹⁷A verdict is potentially more distorted if the judge imposes excessive discretion in his judicial reasoning (Liu and Li, 2019).

¹⁸As 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>

However, neither of these issues 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.¹⁹ 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 technical capacity.²⁰ A substantial share of these missing files belong to those exempted cases. Fourth, 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 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

¹⁹For 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.

²⁰We 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.

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;²¹ nevertheless, this variable may contain measurement error, as there are low legal and financial stakes for accurate accounting (Shi et al., 2021).

²¹There 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.

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 5.2, 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 SPC every year, summarizing the design, implementation, and effectiveness of judicial reforms in China. Each yearbook contains a chapter for each province, which provides a detailed timetable of reform roll-out. The roll-out information was further corroborated using information from local courts' websites and local governments' fiscal expenditure records.

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.

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-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:

$$Y_{it} = \alpha \cdot Reform_{it} + \phi_i + \lambda_t + \epsilon_{it} \quad (1)$$

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:

$$Y_{it} = \sum_T \beta_{1T} T_{it} + \phi_i + \lambda_t + \epsilon_{it} \quad (2)$$

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.²²

In Columns (2) and (3), we investigate heterogeneity in the baseline result with respect to the local defendant's political connections. We define a firm as politically connected if it has won at least one government procurement contract since 2014. Our results show that the judicial reform reduces the win rate for both politically connected and unconnected local defendants, but the effect is almost doubled in magnitude for connected ones.²³ This is consistent with the hypothesis that connected firms tend to receive more protection from politically captured local courts, and that these firms are affected more as the reform removes local protection.²⁴

In Columns (4) and (5), we compare the reform's impact on cases involving firms from different prefectures within the same province to cases involving firms from different provinces. As explained in Section 2, the judicial independence reform consolidates the control over local courts at the provincial level; hence, the reform should mitigate local protectionism to a greater extent for intra-provincial, inter-prefectural cases than for those across provinces. This is confirmed by the results in Columns (4) and (5): inter-

²²In 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.

²³We can reject the null hypothesis that the two coefficients are the same at the 1% level.

²⁴Interestingly, as shown in Appendix Table A.5, when we split the sample by whether the local defendants are state-owned enterprises (SOEs) or private firms, we find that the reform has comparable impacts on private firms and SOEs. In Columns (3) to (5), we further decompose the SOEs into three types: centrally-controlled SOEs, big locally-controlled SOEs, and small locally-controlled SOEs. As we can see, the central SOEs have very high baseline win rates as local defendants (57%), and are completely unaffected by the reform. In contrast, locally-owned SOEs, regardless of their sizes, have significantly lower win rates as local defendants after the reform. These findings are consistent with the interpretation that the reform made the judiciaries more independent from the *local* governments, while the central government still maintains substantial influence.

prefectural cases within the same province saw a reduction in the defendants' win rate by 4.4 percentage points, while inter-provincial cases only saw a reduction by 2.3 percentage points.²⁵ In particular, the smaller but still significant effect on the latter cases suggests that, even though in principle the reform leaves open the scope for provincial governments to exercise local protectionism, in practice the degree of protectionism 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 (prefectural and county) governments; and (b) the benefit relative to the cost of protecting a single firm may be lower for provincial governments than for prefectural and county governments.

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.²⁶ 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.²⁷

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 cases with politically connected defendants and intra-provincial cases. These findings provide additional confidence in the validity of our research design.

²⁵The two coefficients are statistically different at the 1% level.

²⁶In Appendix Figure A.8, we present the corresponding event studies.

²⁷Not 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.

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' 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 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 reasoning that frequently cites “discretionary codes” are typically deemed less legally sound (Liu and Li, 2019).²⁸

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 18% for external plaintiffs while remaining unchanged for local defendants; (b) judges become 29% 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 14% 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 increasing over time, which helps explain why the baseline results on win rates appear to be

²⁸“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.

persistent.

A related concern is that the courts may have improved the quality of judicial decisions and ruled more favorably toward the external plaintiffs after the reform, but did not actually enforce these rulings strictly. If that is the case, then the nominal reduction in local protectionism might not translate into consequential outcomes. We show evidence against this “reduced compliance” hypothesis, and show that the reform’s impact on rulings has 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.6, 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.

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.²⁹

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

²⁹The exploitation of within-judge behavioral changes is similar in spirit to Ash et al. (2022).

tiffs (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).

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.

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 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 county-year level, for each county, we obtain a measure of the yearly total investments received by all local firms from outside investors.³⁰

To investigate how the judicial independence reform affects external investments into each local county, we estimate Equation 1, using “outside investment” as the outcome

³⁰Note that here we can no longer disaggregate the time dimension to every six months, because many firms only update their shareholding information annually.

variable. Table 6 shows 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 6.6%. These results indicate that improved judicial environments indeed attract more outside investments, echoing the rich cross-country evidence documented in the FDI literature.³¹

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; post-reform, regions experience steady growth in both the number and the total value of investments from outside investors. The magnitude 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.³²

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 due to entrepreneurs substituting from the control to 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.

³¹For example, see Gliberman and Shapiro (2002); Li and Resnick (2003); Li (2021).

³²In Appendix Figure A.10, we present results from alternative event study specifications, and the results remain the same.

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.³³ Results show that the baseline investment response is predominantly driven by intra-prefectural investment flows; the number (and total amount) of investment from other counties in the same prefecture increased by 17.3% (19.7%) following the reform. In contrast, Columns (2) and (3) show that investment flows from external prefecture or province did not experience significant increases. These patterns provide direct evidence for the limited degree of substitution in non-local entrepreneurs' destination choices.

Analogously, another potential bias for our interpretation is that entrepreneurs substitute from investing locally towards investing non-locally in other counties. This bias would also misattribute distributional effect as aggregate gains. In Column (4) of Table 7, we repeat the DiD exercise using local investments as the outcome variable; the result shows that the judicial independence reform does not affect local entrepreneurship differentially between treated and control prefectures, thereby ruling out the empirical relevance of this concern.

To unpack mechanisms, in Appendix Table A.8, we examine two additional sources of heterogeneity of the investment response.³⁴ First, we split the sample based on whether an investment in local firm was "controlling" or "non-controlling," as measured by whether the external investor's share exceeded 50% of that local firm after the investment. As shown in Columns (1) and (2), both types of investments increased following the reform, with the "non-controlling" type being more salient. This is consistent with the fact that a fairer local court enables non-controlling external investors to participate in the local economy without having to worry about being taken advantage of by local partners. Second, Columns (3) and (4) show that the baseline investment increase was mainly driven

³³The corresponding event studies are presented in Appendix Figure A.11

³⁴The corresponding event studies are presented in Appendix Figure A.12

by investment in non-tradable sectors, rather than tradable ones. Intuitively, one possible reason is that non-tradable sectors require a bigger local presence (management and personnel) in order to serve the market, and thus are more sensitive to local judicial environments.

5.2 Welfare Implications

In this section, we present a simple model of inter-jurisdiction investments with endogenous entry, along the lines of Melitz (2003). We show that the investment response, as estimated in 4, is a model-based sufficient statistic for the economic benefits of the judicial reforms and the associated elimination of protectionism-induced distortions. We use the sufficient statistic to quantify the reform's welfare impact.

5.2.1 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:

$$U = \int_{\varphi \in \Phi} u(q(\varphi)) - p(\varphi)q(\varphi) dF(\varphi), \quad (3)$$

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

$$q^*(p) = \arg \max_q \{u(q) - pq\} = p^{-\epsilon}; \quad (4)$$

the parameter ϵ captures demand elasticity. We focus on products by non-local firms and omit local products from preferences in (3).

When firms make entry and pricing decisions, each entrepreneur draws a cost index $\varphi \leq 1$ from distribution $F(\varphi) = \varphi^\gamma$ 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:

$$\Pi \equiv \int_0^1 \max \{ \pi(\varphi) - f, 0 \} dF(\varphi), \quad (5)$$

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

$$\pi(\varphi) \equiv \max_p (p - \tau\varphi) q^*(p). \quad (6)$$

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 and choosing between investing locally and non-locally. This modeling choice is motivated by our reduced-form findings in Table 7, where there

seems to be little investment response from non-treated to treated prefectures or from investing locally to non-locally. We generalize the model in Appendix B.2 to incorporate these margins. Below, we formalize our arguments for excluding them when conducting welfare calculations.

5.2.2 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

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

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 $R \equiv \int_0^{\bar{\varphi}} p(\varphi) q(\varphi) dF(\varphi)$. 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. $\frac{d \ln \mu}{d \ln \tau} = \frac{d \ln U}{d \ln \tau} = \frac{d \ln \Pi}{d \ln \tau} = \frac{d \ln R}{d \ln \tau}$.

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 \$22 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. Assuming the judicial reform affects cross-county economic linkages equally at all of a firm's life cycle stages, we can extrapolate our estimates proportionally to the entire economy: non-local firms from outside counties account for 27.8% of total firm count (and 22.8% by registered capital), Proposition 1 implies the judicial reform can expand GDP by 2.3% if adopted throughout China.

6 Conclusion

In this paper, we study how China's high-stakes judicial independence reform, which decoupled the local courts from the local governments, affected judicial local protectionism and economic integration in the country.

Compiling novel administrative datasets covering millions of commercial lawsuit verdicts and business registration records, and exploiting the staggered roll-out of the reform between 2014 and 2021, we first document that the reform significantly reduced judicial local protectionism in China, as reflected by a 7.0% reduction in local defendants' average win rate against external plaintiffs following the reform. This effect is particularly salient for politically connected local defendants, which is consistent with the fact that these firms likely received the most protection prior to the reform. The effect is stronger for inter-county/prefectural lawsuits within the same province than for inter-provincial lawsuits, which is consistent with the fact that the reform did not fully remove the provincial governments' leverage over the county/prefectural courts.

Across a series of measures borrowed from the law literature, we find clear evidence that the reform not only made the courts rule less favorably toward local firms, but also improved the quality of judicial decisions. Specifically, we find that after the reform: (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 highly discretionary codes in judicial reasoning. Taken together, these results consistently suggest that the reform led the local courts to reduce their favoritism towards local defendants and improved 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.

Leveraging data on the universe of inter-regional investment networks in China, we find that, when a local court is subject to the reform, external individual and business investors are 8.4% more likely to invest in local firms in that region, suggesting that the

reduction of judicial local protectionism led to improved economic integration in China. Through the lens of a simple model of external investment 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 estimate suggests that a nationwide roll-out of the judicial independence reform could improve cross-regional economic integration and lead to overall economic gains by as much as 2.3% of China's GDP.

In addition to shedding new light on the general relationships between judicial independence, local protectionism, and economic integration, we believe that this paper also has profound implications for the understanding of the contemporary Chinese political economy. Specifically, in traditional conceptualizations of the Chinese economy, judiciaries have been largely ignored since they were viewed as subordinates of the party-state. Many observers of China believe that, as the Chinese government has consolidated power to an almost unprecedented extent in the past decade, it is turning further against the rule of law to avoid tying its own hands. Our findings cast doubt on this popular opinion by documenting China's systematic empowerment of its local judiciaries and its salient turning towards legalism. This echoes the qualitative observations of numerous Chinese legal scholars.

This systematic turn towards legalism is a first-order change in China's model of governance. As documented in our paper, increased judicial independence indeed significantly reshaped the inter-business as well as state-business relationships in China in the past decade. Moving forward, important questions for future research include how sustainable such co-existence of authoritarian central government and independent local judiciary will be, and how this 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, that have recently gone through similar judicial independence reforms.

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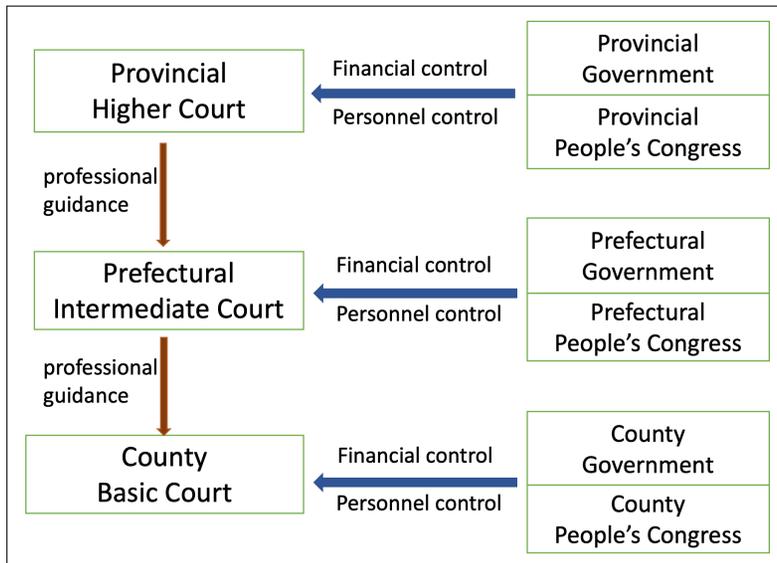
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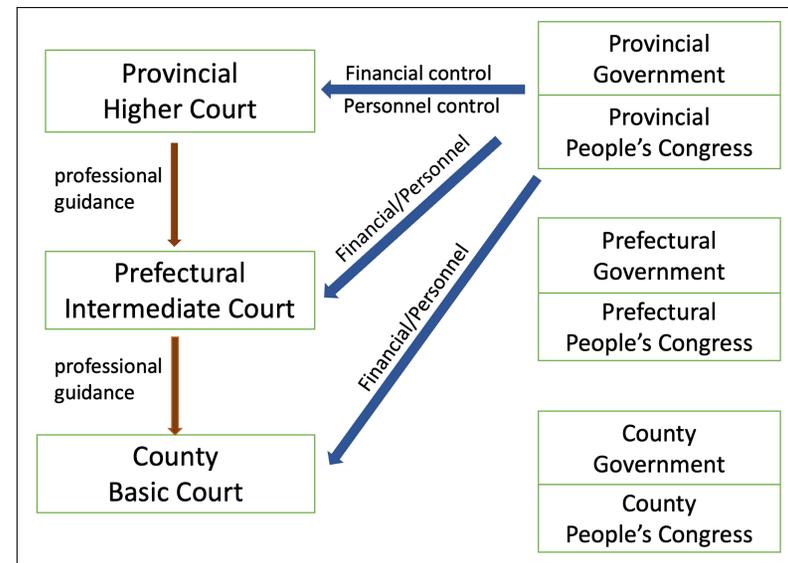
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Figures and Tables



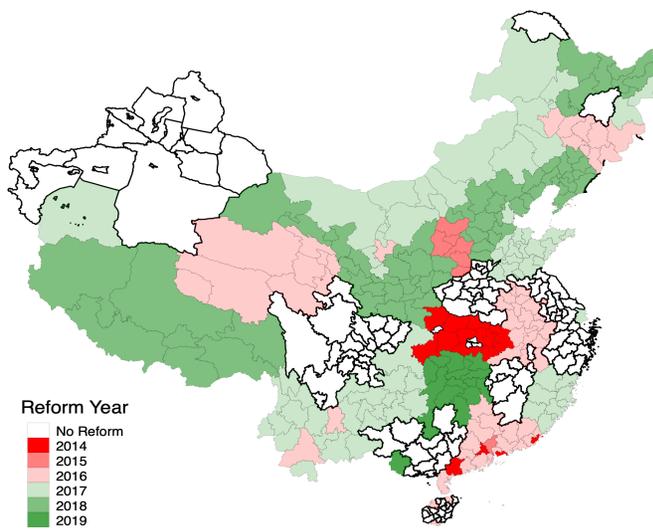
(a) Before



(b) After

Figure 1: Judicial Independence Reform and Incentive Structure Changes

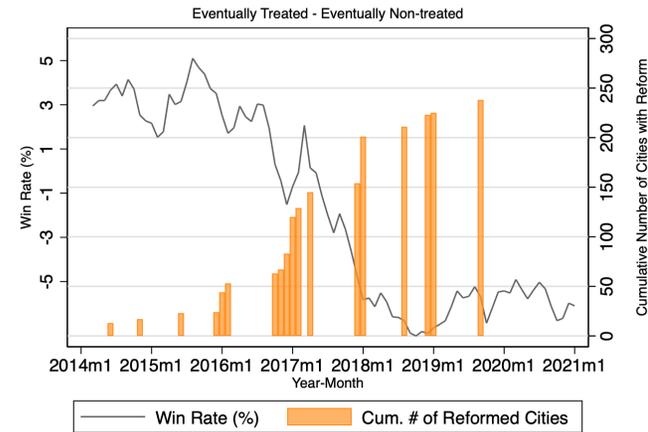
Notes: Panel (a) demonstrates the administrative structure of China's local judiciaries *before* the judicial independence reform. Panel (b) demonstrates the administrative structure of China's local judiciaries *after* the judicial independence reform.



Sources: Yearbook of Judicial Reform of Chinese Courts (2013-2020)

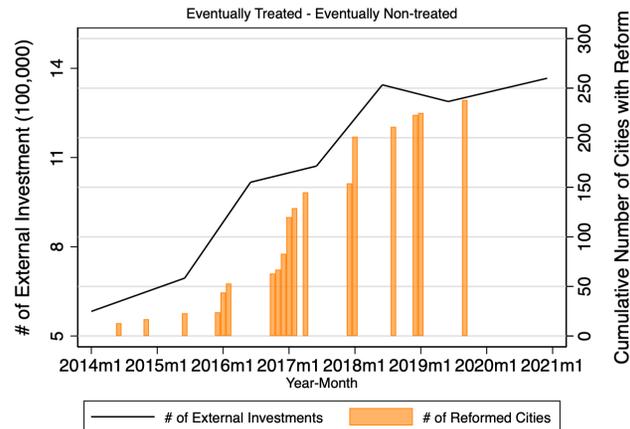
(a) Geographical Expansion

Judicial Independence Reform and the Win Rate of Local Defendants



(b) Reform Expansion & Local Defendants' Win Rate

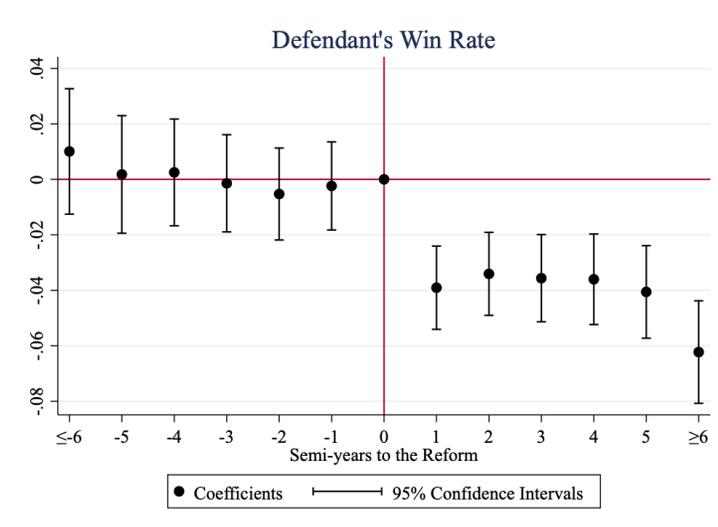
Judicial Independence Reform and External Investment



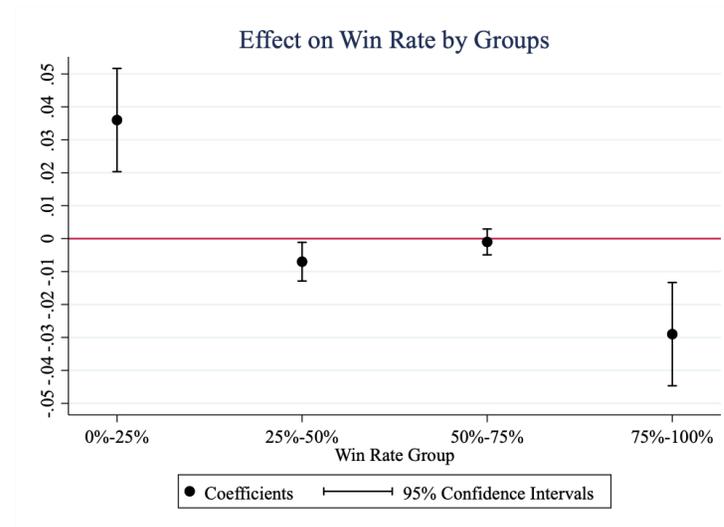
(c) Reform Expansion & External Investment

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).



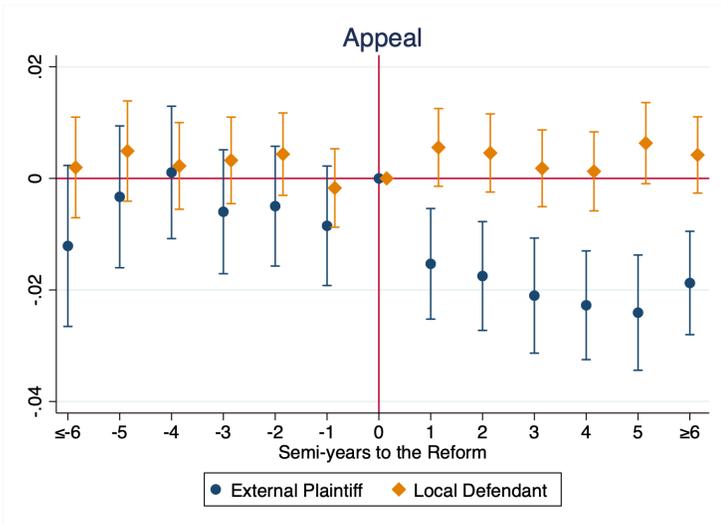
(a) Changes in Local Defendants' Win Rate



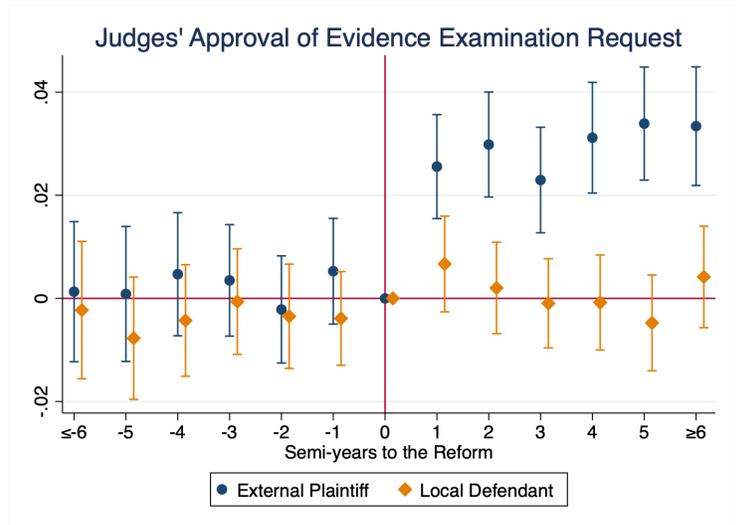
(b) Effect across Groups

Figure 3: Judicial Independence Reform and Judicial Impacts

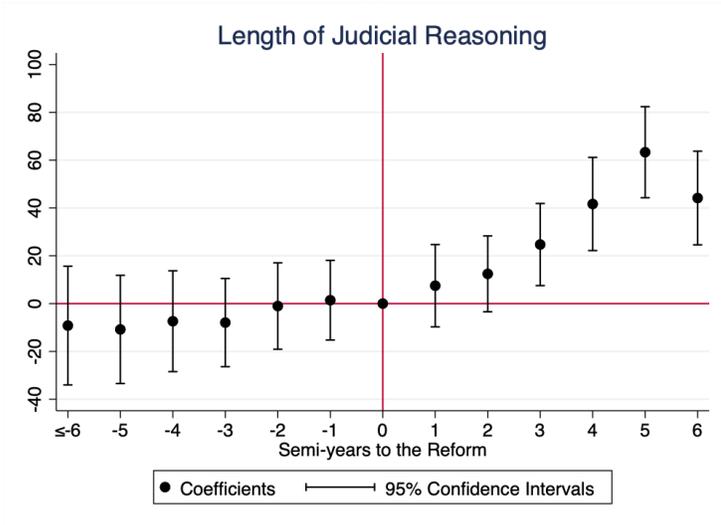
Notes: In Panel (a), we plot the event study coefficients (and the corresponding 95% confidence intervals) for the baseline results, following the approach suggested by Sun and Abraham (2021). In Panel (b), we estimate how the reform affected the likelihood of local defendants' win rates falling into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



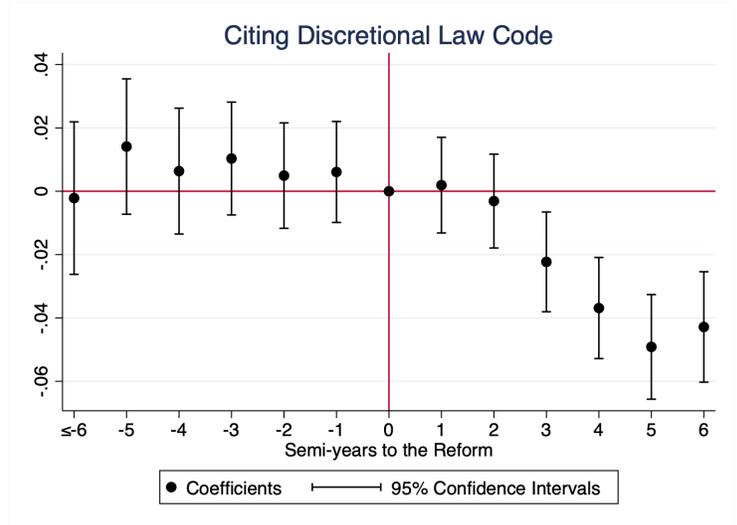
(a) Appeal



(b) Judges' Approval of Evidence Examination Request



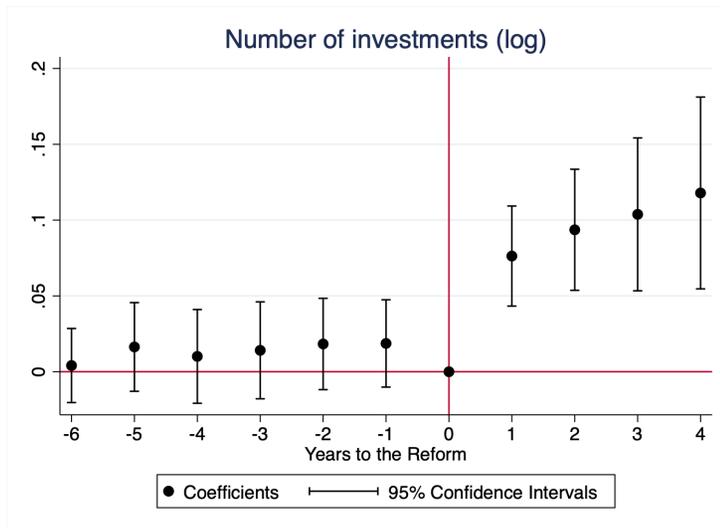
(c) Length of Judicial Reasoning



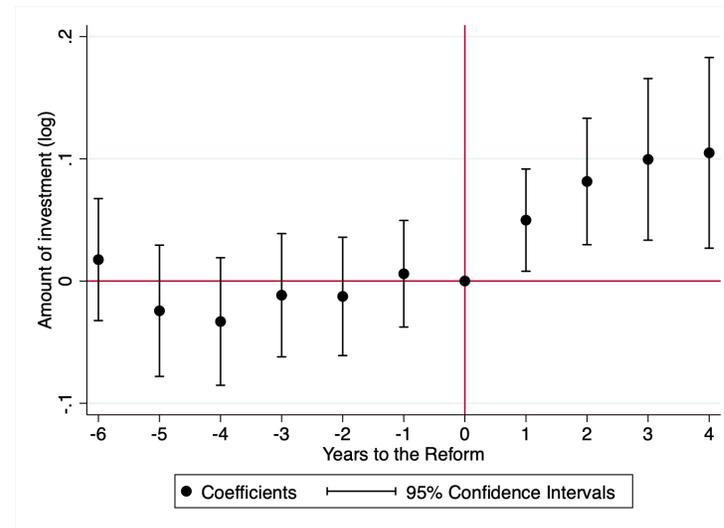
(d) Judge Citing Discretionary Law Code

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
<i>Panel A. Civil Lawsuits Between External Firms and Local Firms</i>					
Local Defendant's Win Rate	1,243,114	0.45	0.47	0.00	1.00
Politically Connected Defendant (yes=1)	1,243,114	0.08	0.28	0.00	1.00
Plaintiff Appeal (yes=1)	1,243,114	0.09	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	0.10	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
<i>Panel B. Firm litigant Characteristics</i>					
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
<i>Panel C. Inter-county Investment Flow Data</i>					
Number of External Investment	44,027	568.50	2,003.63	12.00	1,816.00
Amount of External Investment (100 Mill. CNY)	44,027	20.72	114.02	0.15	75.13

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 since 2007. To construct these two variables, we follow each firm's initial shareholding structure and its subsequent 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	Connected	Non-connected	Intra-Province	Inter-Province
	(1)	(2)	(3)	(4)	(5)
Post Reform	-0.031*** (0.005)	-0.056*** (0.012)	-0.030*** (0.005)	-0.044*** (0.006)	-0.023*** (0.006)
Mean of Outcome	0.44	0.49	0.43	0.40	0.47
Court FE	Y	Y	Y	Y	Y
Semi-year FE	Y	Y	Y	Y	Y
Observations	46,907	19,149	46,615	43,472	38,408
R-Squared	0.212	0.262	0.211	0.185	0.198

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 average win rates of politically connected and non-connected local defendants, respectively. Columns (4) and (5) investigate the local defendants' average win rates in intra- and inter-provincial lawsuits, respectively. Number of observations change across columns since there are singletons for certain court-semiyear observations (e.g., some local courts do not have any connected local 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 3: Judicial Independence Reform and Judges' Decision Quality

	Appeal Rate		Evidence Examination		Judicial Reasoning	Discretionary Codes
	Plaintiff	Defendant	Plaintiff	Defendant		
	(1)	(2)	(3)	(4)		
Post Reform	-0.016*** (0.003)	0.003 (0.002)	0.030*** (0.003)	0.003 (0.002)	34.518*** (5.541)	-0.035*** (0.005)
Mean of Outcome	0.087	0.044	0.104	0.077	482.671	0.254
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	0.160	0.136	0.150	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*** (0.017)	-0.080*** (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*** (0.004)	-8.786*** (2.200)	8.796*** (1.600)	-115.759** (56.649)	82.697*** (28.131)	-0.954*** (0.085)	0.555*** (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 Investments (log)	Amount of Investment (log)
	(1)	(3)
Post Reform	0.084*** (0.018)	0.066*** (0.020)
Mean of Outcome	5.325	10.584
County FE	Y	Y
Year FE	Y	Y
Observations	44,022	44,022
R-Squared	0.870	0.795

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)
<i>Panel A</i>				
	Number of Investments (log)			
	Same Prefecture	Outside Prefecture	Outside Province	Same County
Post Reform	0.173*** (0.018)	0.026 (0.016)	0.024 (0.017)	-0.014 (0.012)
Mean of Outcome	4.468	4.0761	4.268	5.981
Observations	43,521	43,375	43,365	44,708
R-Squared	0.864	0.860	0.861	0.895
<i>Panel B</i>				
	Amount of Investment (log)			
	Same Prefecture	Outside Prefecture	Outside Province	Same County
Post Reform	0.197*** (0.021)	0.035 (0.024)	0.028 (0.022)	0.029 (0.021)
Mean of Outcome	9.321	9.405	9.172	12.550
Observations	43,521	43,375	43,365	44,708
R-Squared	0.790	0.706	0.757	0.857
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

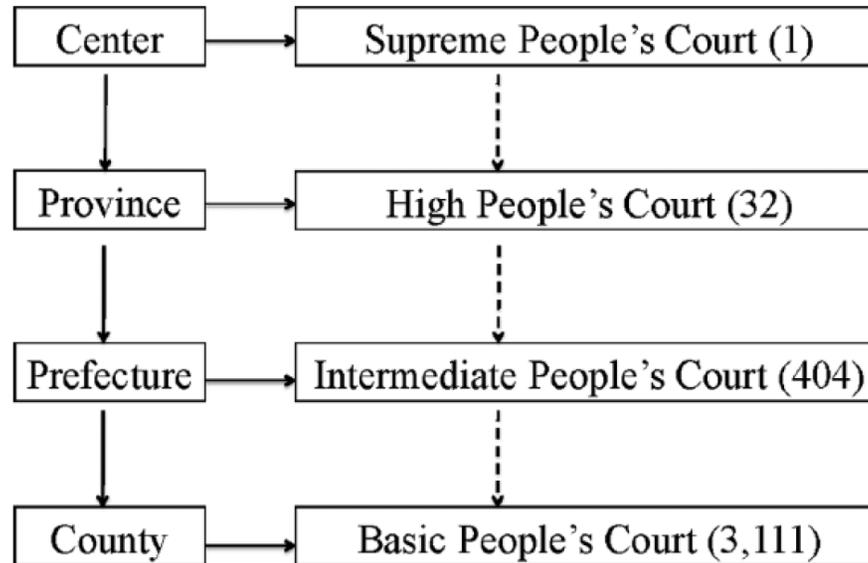


Figure A.1: Structure of China's Judicial System

Notes: Reprinted from Wang (2018).



(a) Frontpage of the China Judgements Online Website

阿克苏盛泰建材有限公司、新疆金岩筑科混凝土有限公司沙雅分公司买卖合同纠纷民事一审民事判决书

案 由 买卖合同纠纷 [点击了解更多](#) 案 号 (2022)新2924民初1733号 发布日期 2022-08-20 浏览次数 17

Plaintiff **Court Name** **The date of Case registration**

新疆维吾尔自治区沙雅县人民法院
民事判决书 (2022)新2924民初1733号

原告: 阿克苏盛泰建材有限公司, 住所地新疆维吾尔自治区库车市。
法定代表人: 胡洪亮, 系公司经理。
委托诉讼代理人: 刘嘉冰, 系公司员工。

Defendant

被告: 新疆金岩筑科混凝土有限公司沙雅分公司, 住所地: 新疆维吾尔自治区沙雅县。
法定代表人: 杨兵, 系公司经理。
委托诉讼代理人: 李维龙, 系公司站长。

原告阿克苏盛泰建材有限公司与被告新疆金岩筑科混凝土有限公司沙雅分公司买卖合同纠纷一案, 本院于2022年8月9日立案后, 依法适用小额诉讼程序, 公开开庭进行了审理。原告阿克苏盛泰建材有限公司的委托诉讼代理人刘嘉冰、被告新疆金岩筑科混凝土有限公司沙雅分公司委托诉讼代理人李维龙到庭参加诉讼, 本案现已审理终结。

原告阿克苏盛泰建材有限公司向本院提出诉讼请求: 1. 请求依法判令被告给付砂石料款33,496.00元。2. 依法判令被告承担本案诉讼费。事实与理由: 2021年4月, 被告方在原告处赊购价值113,056.00元的砂石料, 已付79,560.00元, 剩余欠款33,496.00元, 经原告多次索要未果, 致使原告方起诉来院。请求依法支持原告的诉讼请求。

被告新疆金岩筑科混凝土有限公司沙雅分公司承认原告提出的全部诉讼请求。

本院认为, 当事人有权在法律规定的范围内处分自己的民事权利和诉讼权利, 被告承认原告的诉讼请求, 不违反法律规定。依照《最高人民法院关于适用〈中华人民共和国民法典〉时间效力的若干规定》第一条、《中华人民共和国民事诉讼法》第十三条第二款、第一百六十五条的规定, 判决如下: **Rulings**

被告新疆金岩筑科混凝土有限公司沙雅分公司应付原告阿克苏盛泰建材有限公司砂石料款33,496.00元, 于本判决生效之日起15日内一次性给付原告。如未按本判决指定的期间履行给付金钱义务, 应当依照《中华人民共和国民事诉讼法》第二百六十条之规定, 加倍支付迟延履行期间的债务利息。

案件受理费637.40元, 由被告新疆金岩筑科混凝土有限公司沙雅分公司承担。

本判决为终审判决。

The court fee of 637 CNY shall be borne by the defendant

Judge and the date of delivering this judgment

审判员 唐海
二〇二二年八月十八日
法官助理 赛里买不拉吾东
书记员 阿力木 图尔贡

(b) Sample Court Judgement

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

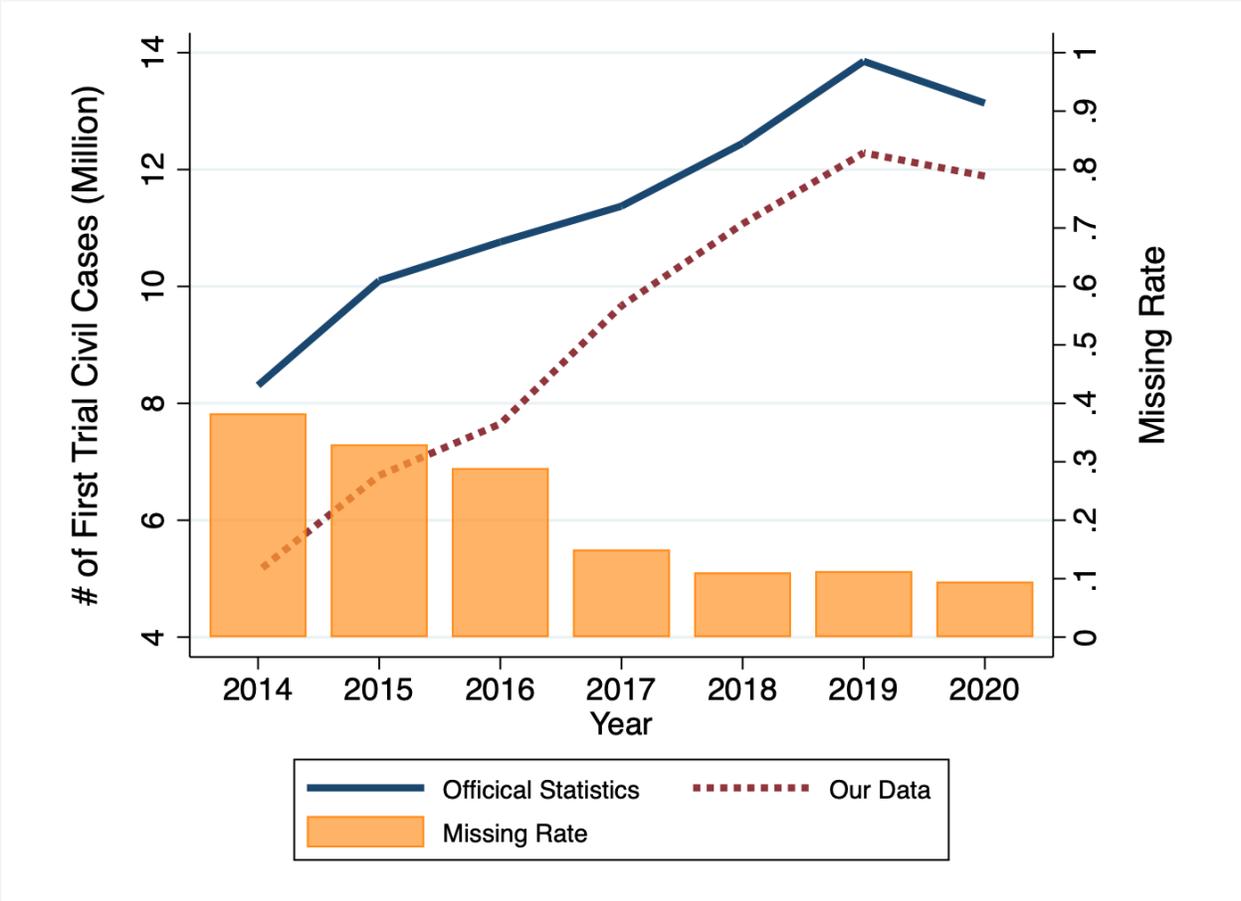
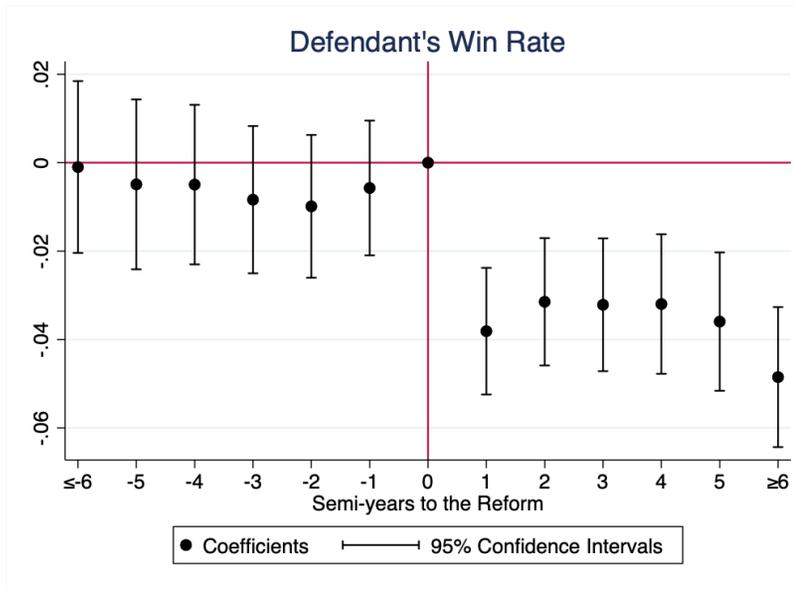


Figure A.3: Missing Rate of First Trial Court Verdicts

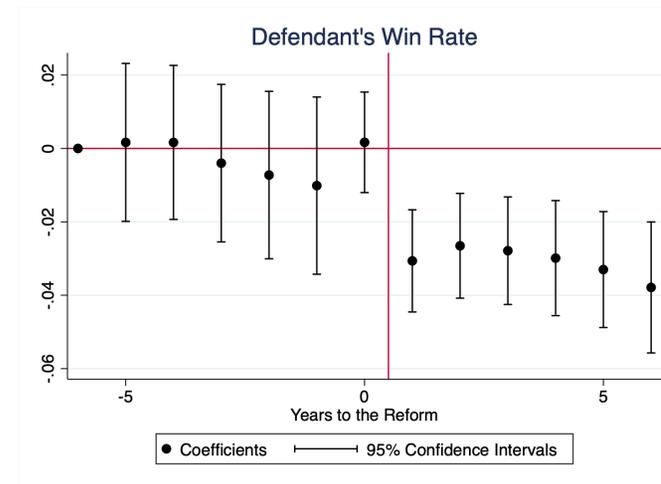
Notes: The official number of first trial civil cases is retrieved from *China Statistical Yearbook* published by National Bureau of Statistics between 2015 to 2021, while the number of first trial civil cases in our data is calculated using verdicts that were trialed between 2014 and 2020 and released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



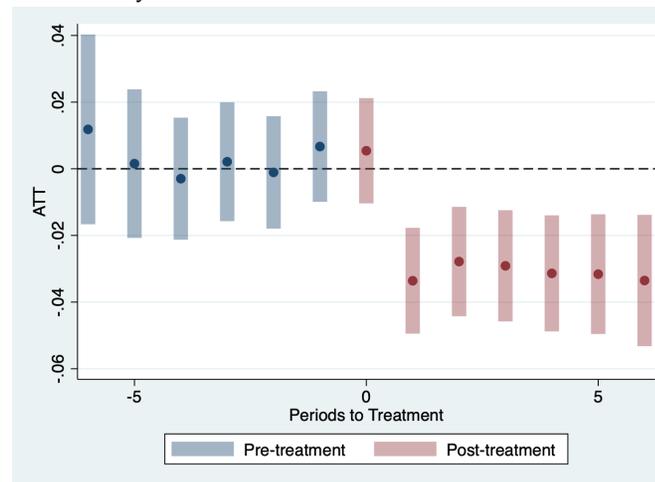
Figure A.4: Frontpage of the Tianyancha.com



(a) Unadjusted Event Study



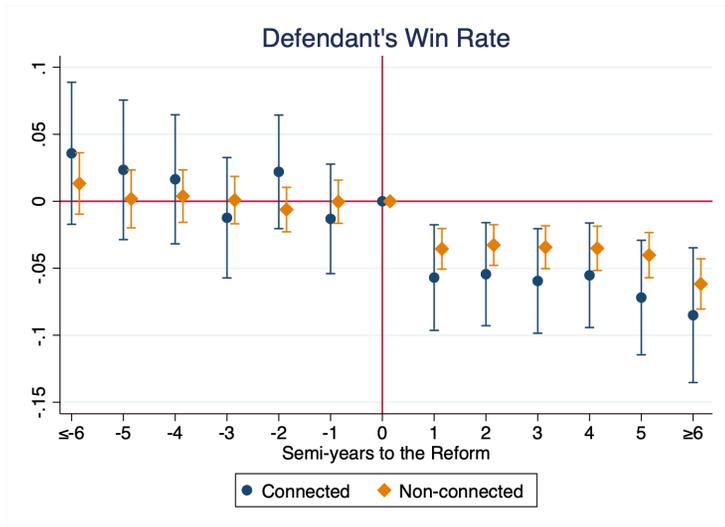
(b) Adjusted following Borusyak et al., 2021



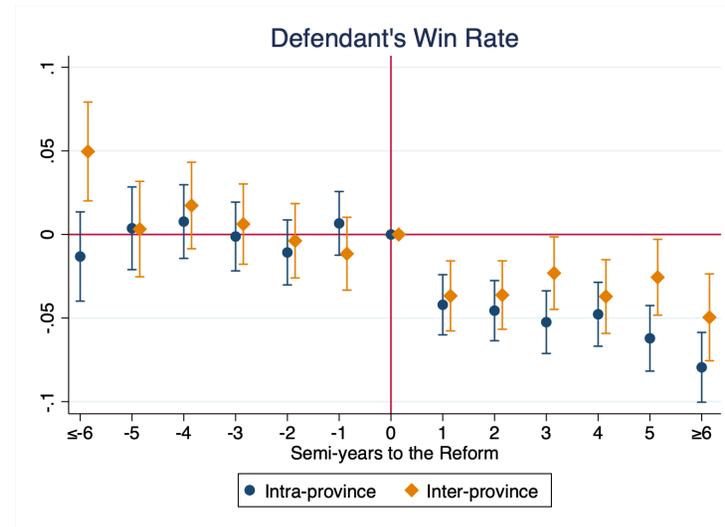
(c) Adjusted following Callaway and Sant'Anna, 2021

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



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

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) and (5) of Table 2. All event studies are estimated following the approach suggested by Sun and Abraham (2021).

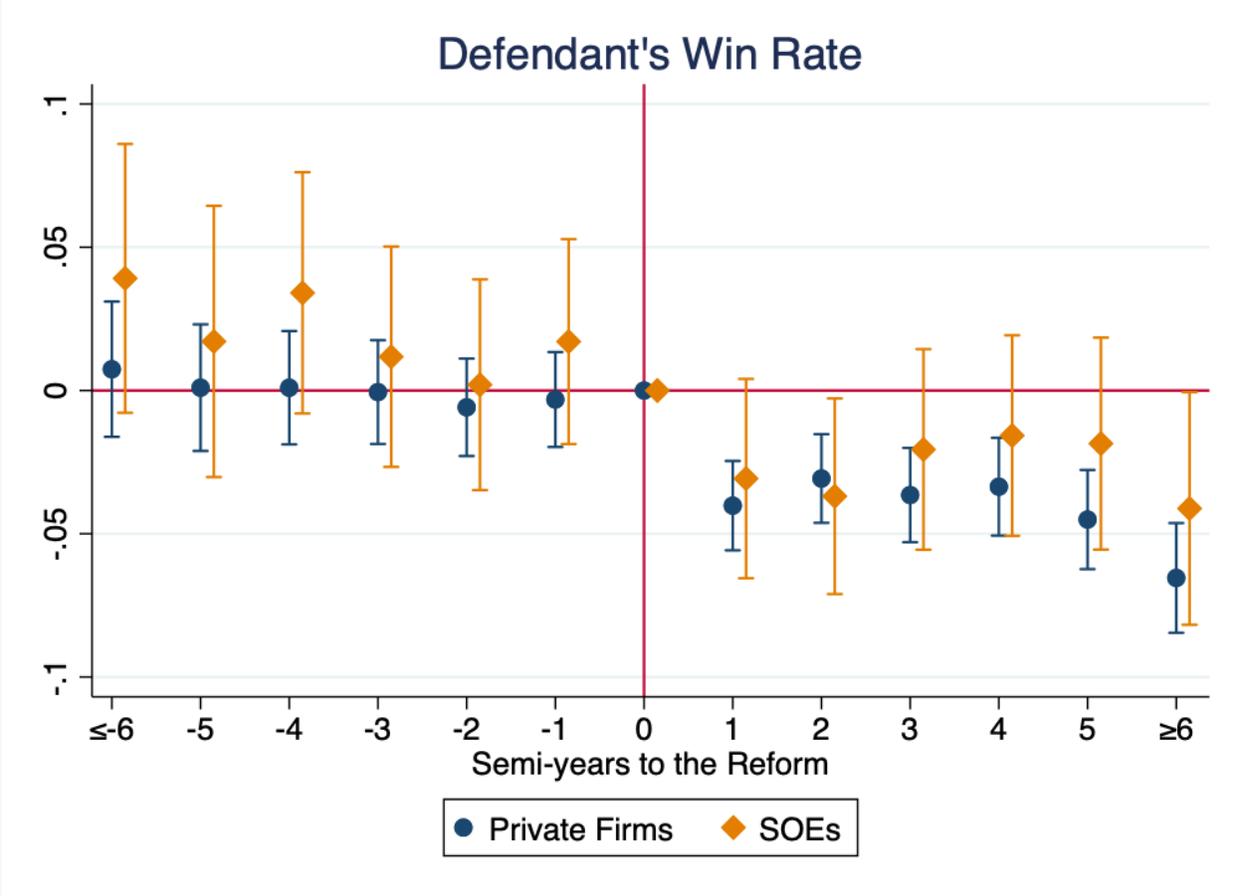


Figure A.7: Judicial Independence Reform and Judicial Impacts - Heterogeneity

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

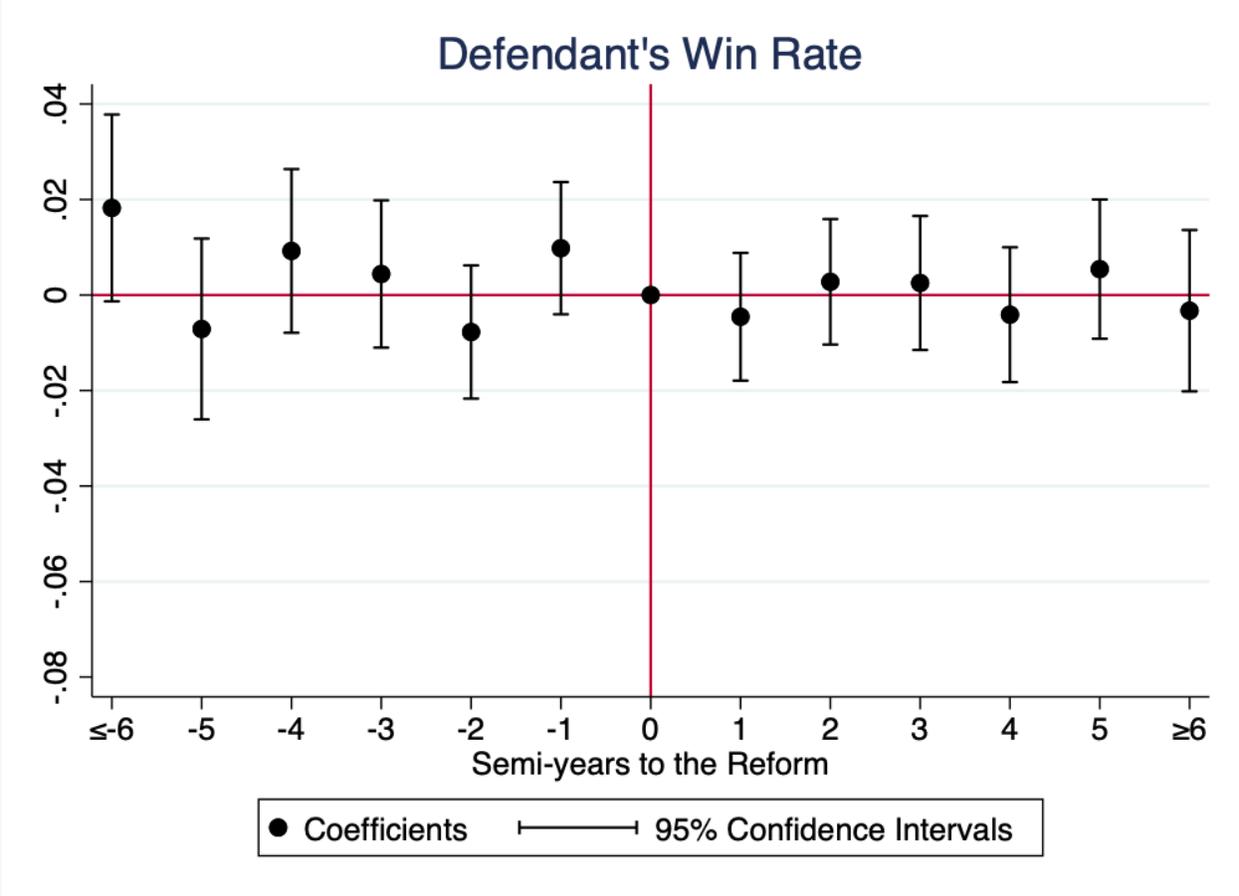


Figure A.8: Placebo Test Using Civil Lawsuits Between Local Firms

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).

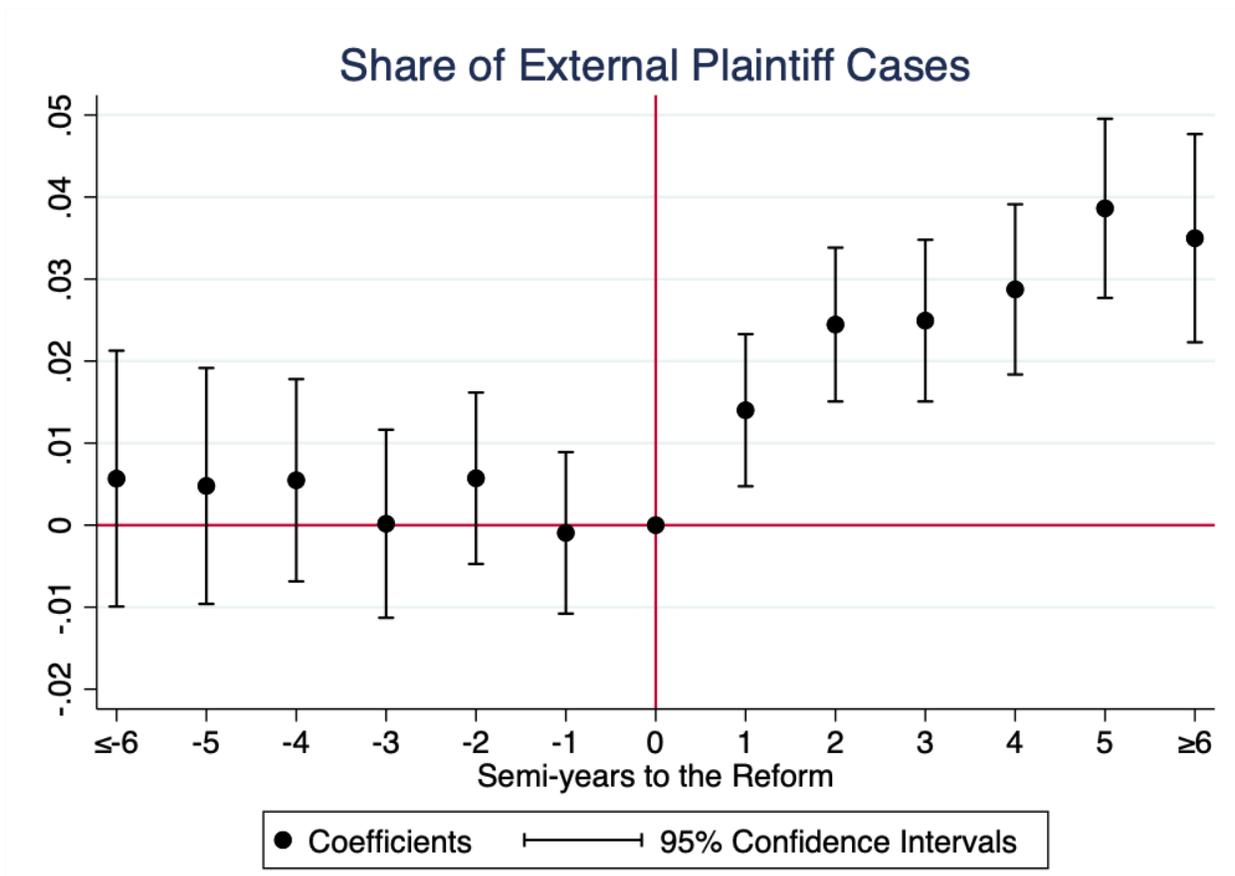
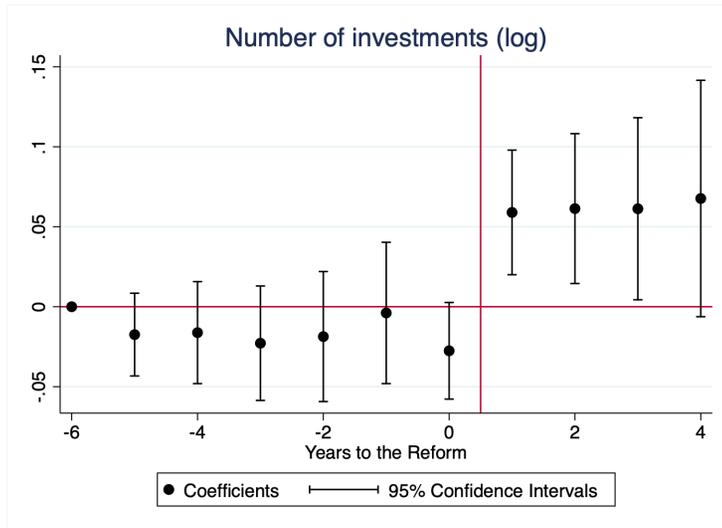
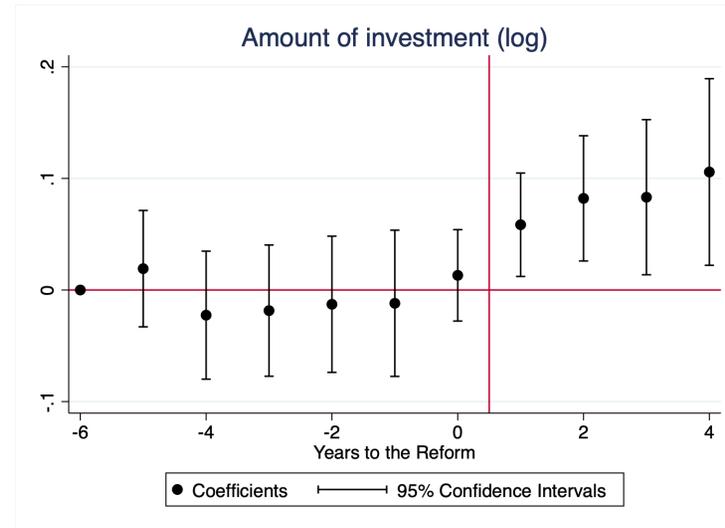


Figure A.9: Judicial Independence Reform and Share of External Plaintiff v.s. Local Defendant Cases

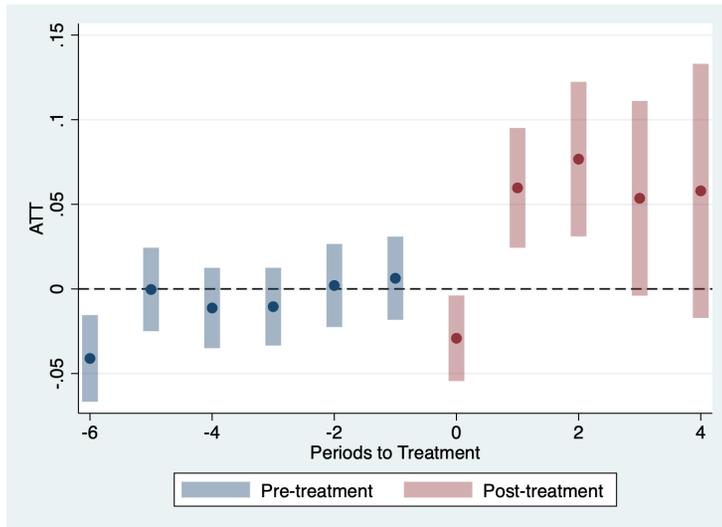
Notes: This figure plots the event study coefficients (as well as 95% confidence intervals) using share of outcome external plaintiff against local defendant cases over all cases between companies as outcome variable following Sun and Abraham (2021).



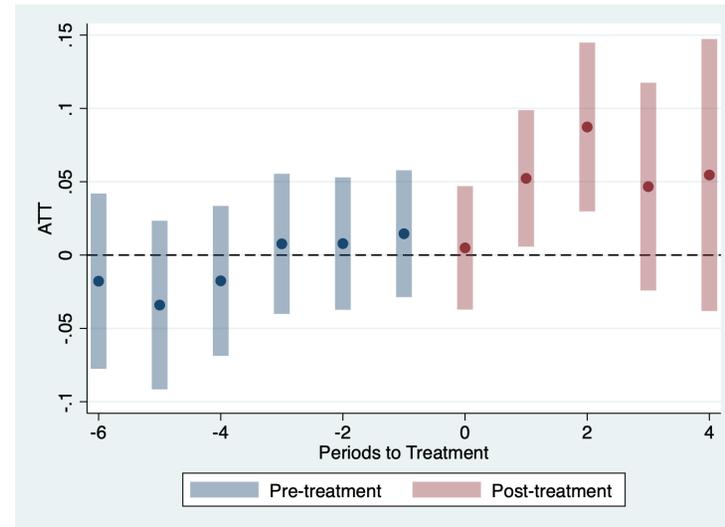
(a) Number of External Investments



(b) Amount of External Investment



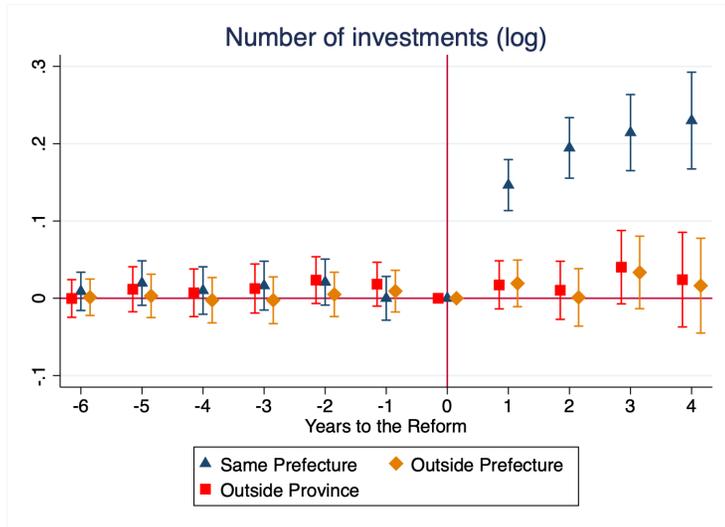
(c) Number of External Investments



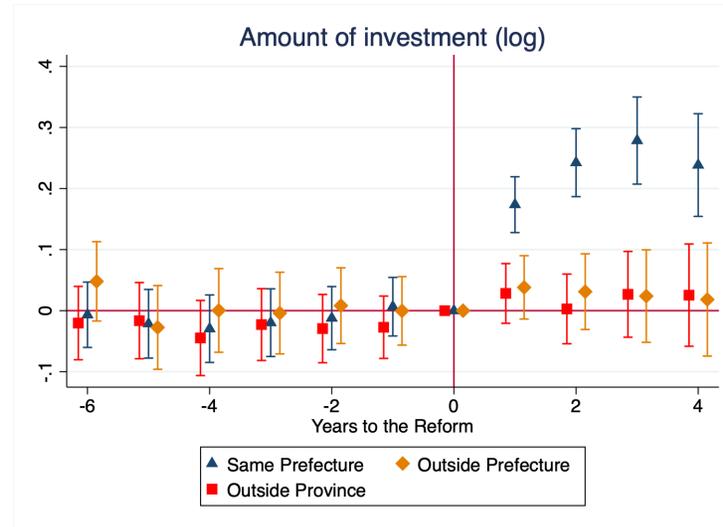
(d) Amount of External Investment

Figure A.10: 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).



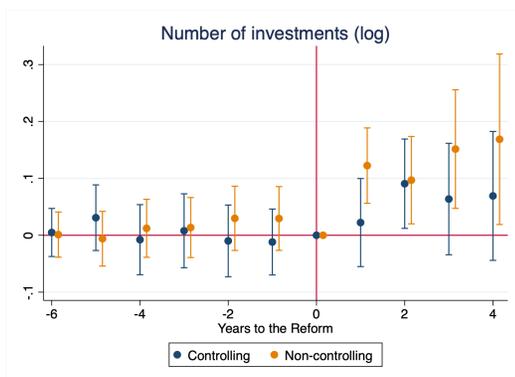
(a) Number of External Investments



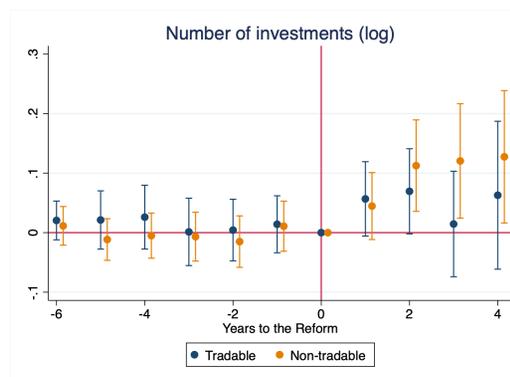
(b) Amount of External Investment

Figure A.11: Judicial Independence Reform and External Investment

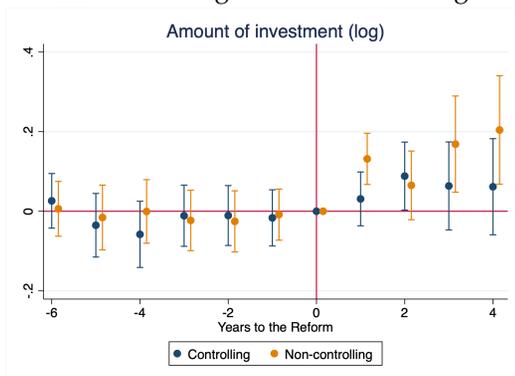
Notes: Panel (a) plots the event study estimates corresponding to Columns (1) to (3) in Panel A of Table 7, following the approach suggested by Sun and Abraham (2021). Panel (b) plots the event study estimates corresponding to Columns (1) to (3) in Panel B of Table 7, following the approach suggested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



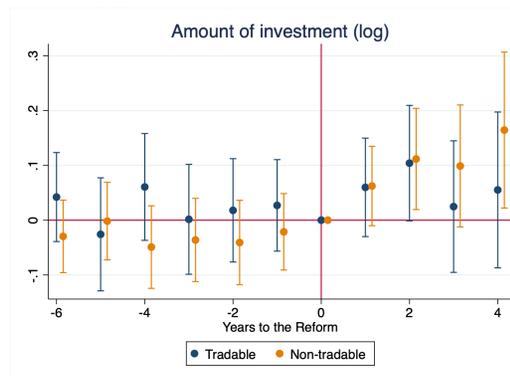
(a) Controlling v.s. Non-controlling



(b) Tradable v.s. Non-tradable



(c) Controlling v.s. Non-controlling



(d) Tradable v.s. Non-tradable

Figure A.12: Judicial Independence Reform and External Investment - Heterogeneity

Notes: Panels (a) and (b) plot the event study estimates corresponding to Columns (1) and (3) of Table A.8, following the approach suggested by Sun and Abraham (2021). Panels (c) and (d) plot the event study estimates corresponding to Columns (2) and (4) of Table A.8, 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 (0.026)	0.017 (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 Judgements 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	Connected	Non-connected	Intra-Province	Inter-Province
	(1)	(2)	(3)	(4)	(5)
<i>Panel A</i>					
Post Reform	-0.040*** (0.008)	-0.053*** (0.014)	-0.039*** (0.008)	-0.058*** (0.010)	-0.013 (0.011)
Court FE	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y
Observations	1,191,854	101,727	1,089,773	596,261	595,498
R-Squared	0.080	0.116	0.083	0.069	0.114
<i>Panel B</i>					
Post Reform	-0.031*** (0.006)	-0.039*** (0.014)	-0.030*** (0.006)	-0.049*** (0.008)	-0.004 (0.010)
Judge FE	Y	Y	Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y
Observations	1,166,520	86,895	1,064,423	571,043	572,419
R-Squared	0.258	0.393	0.267	0.268	0.321
Mean of Outcome	0.44	0.49	0.43	0.40	0.47

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	Connected	Non-connected	Intra-Province	Inter-Province
	(1)	(2)	(3)	(4)	(5)
Post Reform	-0.030*** (0.007)	-0.037*** (0.007)	-0.030*** (0.007)	-0.049*** (0.008)	-0.015* (0.009)
Mean of Outcome	0.44	0.49	0.43	0.40	0.47
Court FE	Y	Y	Y	Y	Y
Seimi-year FE	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 (0.005)	-0.018** (0.009)	0.007 (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

	SOEs	Private Firms	Central SOEs	Big SOEs	Small SOEs
	(1)	(2)	(3)	(4)	(5)
Post Reform	-0.025** (0.010)	-0.030*** (0.005)	0.004 (0.030)	-0.034** (0.017)	-0.029** (0.012)
Mean of Outcome	0.47	0.44	0.57	0.55	0.43
Court FE	Y	Y	Y	Y	Y
Semi-year FE	Y	Y	Y	Y	Y
Observations	23,456	46,192	5,276	12,859	18,058
R-Squared	0.238	0.209	0.430	0.292	0.261

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 SOEs. Columns (2) investigate the average win rates of local defendants that are privately-owned firms. Columns (3) investigate the average win rates of SOE defendants that are centrally-owned. Columns (4) and (5) investigate the local defendants' average win rates for SOEs with registered capital above and below the mean, 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 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 Ruling Enforcement

	Non-compliance Rate	Complete Non-compliance	Partial Non-compliance
	(1)	(2)	(3)
Post Reform	0.001 (0.001)	0.001 (0.001)	-0.000 (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.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 (26.597)	-34.526 (21.765)	75.438 (84.187)	-233.305 (327.923)	-0.398 (0.331)	0.295 (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 External Investment - Heterogeneity

	(1)	(2)	(3)	(4)
Panel A				
Number of Investment (log)				
	Controlling	Non-controlling	Tradable	Non-tradable
Post Reform	0.049 (0.037)	0.131*** (0.037)	0.042 (0.034)	0.078** (0.034)
Mean of Outcome	4.613	4.661	2.908	3.657
Observations	43,711	43,530	40,764	42,252
R-Squared	0.850	0.847	0.822	0.887
Panel B				
Amount of Investment (log)				
	Controlling	Non-controlling	Tradable	Non-tradable
Post Reform	0.051 (0.032)	0.135*** (0.036)	0.049 (0.044)	0.078** (0.037)
Mean of Outcome	10.141	9.304	8.260	8.385
Observations	43,711	43,530	40,764	42,252
R-Squared	0.770	0.752	0.604	0.758
County FE	Y	Y	Y	Y
Year FE	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 that fully control or partially control the invested firm, respectively. Columns (3) and (4) 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

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

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

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

$$= \frac{\epsilon^{-\epsilon}(\epsilon-1)^\epsilon}{\gamma-\epsilon+1} \tau^{-\gamma} \kappa^{\gamma-\epsilon+1} \quad (9)$$

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

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

Total Revenue. The total revenue of nonlocal firms is

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

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

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

The response of producer surplus is

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

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

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

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

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:

$$U_n = \int_{\varphi \in \Phi_n} u(q_n(\varphi)) - p_n(\varphi) q_n(\varphi) dF(\varphi), \quad (10)$$

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

$$q^*(p) = \arg \max_q \{u(q) - pq\} = p^{-\epsilon}. \quad (11)$$

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 distribution $F(\varphi) = \varphi^\gamma$ and decides whether to pay the fixed entry cost f to produce in location n with marginal cost $c_n(\varphi)$. 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:

$$\max_n \xi_n \bar{\pi}_n, \quad \bar{\pi}_n \equiv \int_0^1 \max\{\pi_n(\varphi) - f, 0\} dF(\varphi), \quad (12)$$

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

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

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 $\frac{d \ln(\omega_n/\omega_m)}{d \ln(\bar{\pi}_n/\bar{\pi}_m)} = \theta$.

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

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

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 ; $R_n \equiv \int_{\varphi \in \Phi_n} p_n(\varphi) q_n(\varphi) dF(\varphi)$ is the total revenue in location n . It can be shown that, just as in the baseline model in the main text, $d \ln \mu_n$ is a sufficient statistic for $d \ln U_n$, $d \ln \Pi_n$, and $d \ln R_n$:

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

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 ($\beta^{DiD} = d \ln \mu_n - d \ln \mu_m$). 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 ($d \ln \mu_m \neq 0$), the DiD estimator does not recover $d \ln \mu_n$.

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 $\frac{d \ln \mu_m}{d \ln \tau_n} = \gamma \theta \omega_n$, 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

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

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.