

Relative Capture in the Judiciary: Evidence from a Natural Experiment in China

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Abstract

Ever since the *Federalist Papers*, there has been a common view that the lower the level of government, the greater is the extent of capture by vested interests. Relying on the analytical framework of relative capture, I argue that interest groups have different incentives and capacities to capture different levels of government. I test the theory by investigating how judges at different judicial levels adjudicate corporate lawsuits in China. Exploiting a natural experiment in which the Supreme People's Court dramatically raised the threshold for entering higher-level courts in 2008, I report evidence that while privately owned enterprises are more likely to win in lower-level courts, state-owned enterprises are more likely to win in higher-level courts. These results are robust to a variety of tests, including considering selection bias, firms' political connections, and using different samples. The findings challenge an underlying assumption in the decentralization literature and have important policy implications for countries that have centralized their political systems in order to avoid local biases.

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Going back to Alexander Hamilton, James Madison, and John Jay in the *Federalist Papers* (No.10), a common view is that the lower the level of government, the greater is the extent of capture by vested interests (Hamilton, Madison and Jay 1961). Empirical work finds that granting more autonomy to local governments facilitates elite capture of political processes, creating a company town atmosphere in which regulations and policies are designed to protect the regulated businesses (Lorentzen, Landry and Yasuda 2014; Campos and Hellman 2005; Reinikka and Svensson 2004). Some recent work shows that decentralized systems are more corrupt (Treisman 2000; Goldsmith 1999) and have weaker law enforcement than centralized systems (Cai and Treisman 2004), and others find that centralization has significant effects on eliminating elite corruption at the local level and generating better group outcomes (Malesky, Nguyen and Tran 2014; Grossman and Baldassarri 2012).

If this presumption is correct, the advantages of decentralization—to bring governments “closer to the people,” induce competition among local governments, and make it easier for citizens to hold their representatives accountable—would be compromised by greater capture by local elites.¹

Aware of the danger of local capture, many formerly decentralized developing countries have started to centralize their political systems in order to correct “local protectionism.” For example, the National Assembly in Vietnam introduced the Ordinance on Judges and Jurors of People’s Courts in 2002 to “centralize the management of the lower courts” to higher-level courts.² In the same vein, the Chinese Communist Party announced a “rule-of-law” reform package in 2014 to centralize the political and fiscal management of the judicial system, which has been blamed for protecting local vested interests.³

An underlying assumption made by many studies of local capture is that the extent of capture will linearly decrease as one moves up the political hierarchy because politicians at higher levels keep an arm’s-length relationship with interest groups. I challenge this assumption by arguing that capture exists at every level, and that capturers are different at different levels.

¹For a review of the pros and cons of decentralization, please see Wibbels (2006), Treisman (2007), and Bardhan (2002).

²Please see <https://www.bhba.org/index.php/component/content/article/251> (Accessed February 23, 2015).

³Please see <http://goo.gl/QmBk4m> (Accessed February 23, 2015).

Inspired by [Bardhan and Mookherjee \(2000\)](#), I extend the analytical framework of *relative capture* to argue that interest groups have different incentives and capacities in influencing different levels of government. Due to political (for example, electoral institutions) and economic (for example, tax structures) constraints, different levels of government are susceptible to distinct interest groups. For example, a higher-level government's reliance on a certain type of interest group for tax revenue might strengthen that group's incentive and capacity of *higher capture*, while a lower-level government's fiscal dependence on another type of interest group might create a tendency for *lower capture*. The relative capture framework does not make the linear assumption that the lower the level of government, the greater is the extent of capture. Rather, it claims that various levels of government are *relatively* captured by various interest groups.

I test the theory by investigating how judges at different judicial levels adjudicate corporate lawsuits in China. Courts are an important contracting institution that provides the legal framework to protect private contracts ([North 1981](#); [Acemoglu and Johnson 2005](#)), and the quality of legal institutions is highly correlated with a country's long-term economic development ([La Porta et al. 1997](#)), protection of human rights ([Simmons 2009](#)), and quality of government ([La Porta et al. 1999](#)). China has one of the largest decentralized political systems in the world ([Landry 2008a](#)), and its judiciary represents a typical decentralized system in which fiscal, administrative, and political power has been granted to local authorities ([Falleti 2010](#)). A close examination of how the Chinese judiciary handles cases brought by different firms provides important insights into how a decentralized system functions and how various interest groups operate.

Taking advantage of China's fragmented bureaucratic structure, in which the judiciary must answer to the territorial party-state (horizontal authority) rather than a higher-level court (vertical authority), I am able to study how the linkages between litigants and a particular level of party-state influence judges' decisions. Importantly, Chinese judges' incentive structure is shaped by the local government tax system. While the Chinese tax system allocates a bigger percentage of private firms' tax payments to county governments (which control basic people's

courts), state-owned enterprises (SOEs) are the major revenue sources for municipal governments (which control intermediate people's courts). This fragmentation between the horizontal and vertical lines in the political hierarchy creates different opportunities and incentives for judges and firms at different levels. I therefore expect that privately owned firms are more likely to win in lower-level courts (basic people's courts), and that SOEs are more likely to win in higher-level courts (intermediate people's courts).

Rigorously testing relative capture would require a counterfactual in which the same interest group is treated at different levels in the judiciary. However, this counterfactual does not exist in the real world, and corporate lawsuits handled by lower-level courts are systematically different from those adjudicated by higher-level courts. For example, higher-level courts are responsible for disputes with higher financial stakes.

I exploit a natural experiment in which the Chinese Supreme People's Court dramatically raised the threshold for entering higher-level courts in 2008 to examine how firms are treated differently at different levels. For example, before 2008, economic disputes with claims higher than 6 million *yuan* (roughly \$920,000) were under the jurisdiction of intermediate people's courts, and cases less than this amount were under the jurisdiction of basic people's courts. This cutoff point was raised to 50 million *yuan* (roughly \$7.7 million) in 2008. This jurisdictional change shifted a large number of cases between 6 million and 50 million *yuan* (treatment group) that should have been adjudicated by intermediate people's courts before 2008 to basic people's courts after 2008. This policy change therefore creates exogenous variations in firms' exposure to different levels of the judiciary.

Compiling and analyzing a novel dataset of over 4,000 court cases disclosed by publicly traded firms from 1998 to 2013, I report strong evidence supports the relative capture framework. On average, SOEs' win rate is estimated to be 30% higher in intermediate people's courts than in basic people's courts, while non-SOEs' win rate is 40% higher in basic people's courts than in intermediate people's courts.

The findings have important implications for many developing countries that struggle to strike a balance between the private and public sectors. SOEs have been described as a major

interest group that is responsible for obstructing economic reforms (Haggard 1990), bending regulations (Hellman, Jones and Kaufmann 2003), and causing many developmental problems, such as pollution and corruption (Dasgupta et al. 2001). A prevailing remedy for the sins of SOEs is privatization, which has been championed by the World Bank and encapsulated in the Washington Consensus (Lieberman 1993). This view rests on the assumption that state capture will be reduced (or even eliminated) when resources are transferred to the private sector. However, as I show, SOEs and private firms can both capture the state, given certain incentive structures in the political system. Privatization would only shift capture from one level to another.

To my knowledge, this is the first quasi-experimental evidence on whether and how different interest groups are treated differently at different levels in a political system. My findings directly challenge an implicit assumption made by a large literature that the lower the level of government, the greater is the extent of capture by vested interests.

This article also speaks to the literature on the rule of law in authoritarian regimes (Helmke 2002; Moustafa 2007; Ginsburg and Moustafa 2008; Wang 2015) and contributes to the scarce but growing literature on comparative judicial politics (Ferejohn, Rosenbluth and Shipan 2009). This the first article to demonstrate that judges do rule in a biased fashion over business disputes in a country without judicial independence, in order to protect firms that party authorities want to protect. This is qualified or nuanced, however, by the finding that local authorities protect firms only if they contribute revenues to the local coffers.

Relative Capture and the Chinese Judiciary

Following Hellman, Jones and Kaufmann (2000), I define capture as firms using their influence to shape the formulation of the rules of the game or how games are played (with or without private payments to public officials and politicians). Bardhan and Mookherjee (2000) develop a framework of relative capture based on a model of electoral competition subject to the influence of special interest groups. In their basic setup, two parties whose policy platforms are influenced by campaign contributions from a lobby representing the interests of an elite are

competing. Voters vary in their socio-economic status, political awareness, and party loyalty. There is an asymmetry in political participation across different classes: rich and informed voters participate at a higher level than poor and uninformed voters. The model then generates conditions under which capture is more likely to occur at the local or higher levels. One condition relates to interest groups' incentives and capacity to influence elections at different levels. For example, when the electoral process is based on a majoritarian system in which only the dominant party wins and gets represented in national policy-making (as opposed to a system of proportional representation), politicians rely more on campaign contributions to win, and interest groups have higher stakes in the election, which creates a tendency toward greater capture at the national level.

The model is not directly applicable to an authoritarian context, such as China. However, its intuition that politicians and interest groups have different incentives and capacities at different levels is useful for understanding relative capture in a non-democracy. For example, if politicians at different levels are dependent on different interest groups for tax revenue (as opposed to campaign funding as in [Bardhan and Mookherjee's](#) (2000) model), then this might make politicians at different levels vulnerable to different interest groups. Below, I briefly introduce China's judicial system and discuss how the relative capture framework can explain judges' incentives in the judicial hierarchy.

The Chinese judiciary is embedded in a fragmented and decentralized political system in which government organizations at different levels (and in different hierarchies) usually have conflicting policy goals ([Lieberthal 1992](#); [Mertha 2009](#)). This "fragmented authoritarianism" is reflected in the judiciary's design, in which a court is the agent of dual principals. The first (vertical) principal is the higher-level court. Although there is a national four-level judicial hierarchy from the Supreme People's Court at the very top to basic people's courts at the lowest level (Figure 1), the institutional design does not require lower-level courts to obey orders from higher-level courts ([Wang 2015](#), 76-79).⁴ For example, the Supreme People's Court cannot independently appoint presidents or other major court officials at the provincial level;

⁴The decentralized legal system was designed to prevent politicians from using the legal system for political purges, which were prevalent in the early years of the Chinese Communist Party. Please see [Tanner and Green \(2007\)](#).

nor is the Supreme People's Court responsible for financing lower-level courts (Wang 2015, 68-70). While judges in the U.S. judicial system often reverse decisions made by lower-level courts, which has a deterrent effect on lower-level courts' behavior (Kastellec 2011), higher-level courts in China rarely amend lower-level courts' decisions, although they have the power to do so.⁵ As an intermediate people's court judge remarked, "We do our best to respect basic people's courts' decisions. We do not correct if it is a borderline case unless there is a fatal mistake."⁶ Higher-level courts, however, do participate in evaluating lower-level courts and making personnel decisions, but this power is limited.

The second (horizontal) principal is the party-state at the same territorial level. The judiciary in China is treated as a functional department under the authority of the party-state. The party-state at each level, including the Chinese Communist Party committees and the executive branch, takes a leading role in making personnel decisions and budgetary allocations for courts at that territorial level (Wang 2015, 76-77). For example, the county party committee and government have the prerogative to appoint presidents and other major court officials at basic people's courts, and pay the majority of basic people's courts' expenditures, including judges' salaries and bonuses, office supplies, vehicles, and court buildings. So the policy preferences of courts are more responsive to those of the corresponding party-state than the higher-level court.

[INSERT FIGURE 1 HERE]

To understand the policy preferences of Chinese judges, we therefore need to examine the incentive structure of Chinese party-state officials. It has been shown that the priority of local Chinese officials is to maximize tax revenues (Lü and Landry 2014), which is a strong predictor of their promotions (Shih, Adolph and Liu 2012). According to the current fiscal system, the Chinese central government and local governments share the tax revenues. While some corporate and value-added tax (but no sales tax) is collected by the central state, the rest is collected by various levels of local government.⁷ In the current tax-sharing system, after the

⁵Basic people's courts and intermediate people's courts serve as first-instance courts for most cases, and a litigant can appeal once to a higher-level court. The decision by the higher-level court is the final verdict.

⁶Author's interview with a judge, March 31, 2010.

⁷For details of the tax-sharing system, please see Wong and Bird (2008).

center collects the central tax, most SOEs tax revenue goes to the municipal governments (the principal of intermediate people's courts), while most of the privately owned companies' tax revenue belongs to the county-level governments (principal of basic people's courts).⁸

I encountered a case during fieldwork that helps clarify the causal mechanisms.⁹ It involved the firm Great Ocean in the City of Buddha. Great Ocean is an SOE owned and managed by the City of Buddha government, and the City of Buddha is a wealthy city in a southern Chinese province. Great Ocean has strong bargaining power vis-à-vis the city government because its tax payment constitutes the lion's share of the city's revenue. As lawyer Wang remarked, "Great Ocean has a strong voice in this city. To a large extent, it can influence city policies and demand special treatment, otherwise it can threaten to lay off people or hide revenue."¹⁰ In 2009, Great Ocean was sued by its stockholders for concealing information, and the case was accepted by the Intermediate People's Court in the City of Buddha. The court considered this a "sensitive" case because the defendant was a local SOE. Under pressure from the city government, the court delayed the process and tried to convince the plaintiffs to settle the dispute outside the court through mediation. The plaintiffs refused. The court finally had to hold court hearings, and the final verdict was made by the court adjudication committee (an ad hoc committee that consists of major officials of the court rather than a panel of judges). The court ruled that Great Ocean won, but to discourage the stockholders from appealing, Great Ocean needed to pay a lump sum of compensation to the plaintiffs, although the amount was lower than what the plaintiffs originally claimed. Lawyer Wang commented, "Chinese local government only intervenes in individual cases if 1) one of the parties has a special identity, for example if a government official or an influential firm is involved, or 2) if the case outcome can impact

⁸There is no uniform formula for sharing across localities, but the rule is that local "critical" enterprises' (most of them are SOEs) taxes are collected by municipal governments, while others (most are non-SOEs) are collected by the county government. Please see <http://zhidao.baidu.com/question/431381018547758724.html> (Accessed February 16, 2015). Please see examples in Harbin (<http://www.harbin.gov.cn/info/news/index/detail1/193751.htm>), Anshan (http://www.ascz.gov.cn/asp/single.aspx?id=312&category_id=1206), and Xinxiang (<http://125.42.176.130/sitegroup/root/html/4028815814af27b40114af5e7e62010d/050124170721.html>).

⁹The case is collected from my qualitative interviews, so the names of the company, the locality, and interviewees remain anonymous to protect the interviewees, but transcripts of these interviews are available upon request.

¹⁰Author's interview with a lawyer, March 29, 2010.

social stability.”¹¹ A judge said, “Here in this place, there is a saying: ‘No matter if it is a black firm or a white firm; as long as it pays taxes, it is a good firm.’”¹²

The case illustrates that tax incentives are a major driving force behind courts’ decisions. As agents of their territorial party-state, we would expect intermediate people’s courts to favor SOEs and basic people’s courts to protect non-SOEs. And because higher-level courts usually defer to lower-level courts’ decisions, we should expect courts to sincerely reveal their policy preferences through their judicial decisions. Hypothesis 1 summarizes this theoretical expectation:

Hypothesis 1: While SOEs are more likely to win in intermediate people’s courts, non-SOEs are more likely to win in basic people’s courts, *ceteris paribus*.

EMPIRICS

There are two empirical challenges to comparing win rates at different levels in an authoritarian state. First, unlike studies of the U.S. federal judicial system that can utilize widely published cases (Sunstein et al. 2006), the judiciary in authoritarian regimes is often one of the most opaque sectors in the political system, so no systematic dataset of court cases is available. Most studies of legal behavior have relied on survey measures of litigants’ preference for courts (Gallagher 2006; Wang 2015) or whether disputants chose to go to courts (Landry 2008b; Ang and Jia 2014). Very few empirical studies have been able to systematically examine court outcomes in authoritarian regimes, especially at the local level.¹³ Second, because of the heterogeneity of cases across different judicial levels, simply regressing case outcomes on the judicial level could produce biased estimates. For example, cases adjudicated by higher- and lower-level courts are systematically different. A conventional regression framework often fails to control for unobservable firm- and court-level characteristics, which could produce omitted variable bias. In addition, litigants can self-select into a certain level of court to obtain a more desirable outcome, which could produce selection bias.

¹¹ Author’s interview with a lawyer, March 29, 2010.

¹² Author’s interview with a judge, April 8, 2010.

¹³ One study that uses actual court outcomes is Helmke (2002), but it is at the top level.

Data

I use a unique, manually coded dataset of firm litigations disclosed by Chinese publicly traded firms from 1998 to 2013 to conduct this study. In 1998, the Shanghai and Shenzhen Stock Exchange issued a rule that required listed companies to disclose their involvement in litigations (Lu, Pan and Zhang 2015). This mandatory disclosure requirement covers all lawsuits that involved publicly traded firms since 1998. I manually collected 4,275 court litigations from company reports (annual, semi-annual, quarterly, and special) and constructed the Chinese Listed Firms' Litigation Dataset (CLFLD).¹⁴ The CLFLD contains information about each case's legal issue, type, litigants, claim (in *yuan*), outcome, timing, court name and level, and others. The vast majority of cases in the CLFLD are economic disputes. I then merged the CLFLD with another dataset that contains firm-level variables, such as state ownership, registration location, age, total assets, and industry. These variables are collected from the China Securities Market and Account Research (CSMAR) database.¹⁵ This article is the first empirical study to use CLFLD to examine judicial outcomes at different levels.¹⁶ Table 1.1 in the web appendix presents the descriptive statistics of the dataset.

Among the 4,275 observations, there are great variations in types of cases and firms: 39.12% are loan disputes, 47.33% contract disputes, and 13.55% other types of dispute, and 73.19% involved SOEs, and 26.81% non-SOEs. The average win rate of SOEs is 45.00%, and that of non-SOEs is 40.66%. I am not able to judge whether a firm *should* have won or lost in a particular case, which would require a close examination of all the evidence available and an application of relevant laws. However, this study is only concerned about *relative* win rates, so the benchmark win rate is a lesser concern.

Because only the discloser is publicly traded and, therefore, publicizes firm information, we know little about its competitor in the litigation unless it is also publicly traded. For 636 unique pairs, I am able to find information on both sides of the litigation; 36.64% of these pairs are

¹⁴Every case is double-coded by me and a group of trained research assistants.

¹⁵I accessed CSMAR through Wharton Research Data Services, a web-based business data research service from The Wharton School at the University of Pennsylvania. <http://wrds-web.wharton.upenn.edu> (Accessed August 9, 2013).

¹⁶Another study that uses data from the same source is Lu, Pan and Zhang (2015), but it explores a different research question.

SOEs versus non-SOEs, 49.37% are SOEs versus SOEs, and 13.99% non-SOEs versus non-SOEs. For the main analysis, I use all 4,275 observations to leverage statistical power, assuming that the natural experiment assigned competitors into treatment and control at random. I will also show that when I focus on the subset of 636 cases, the results are similar.

In total, 1,034 cases (24.64%) were adjudicated by basic people's courts, 2,689 cases (64.08%) by intermediate people's courts, 467 cases (11.13%) by high people's courts, and six cases (0.14%) by the Supreme People's Court. The distribution is skewed because basic people's courts and intermediate people's courts serve as the first-instance courts for most cases, and higher-level courts only handle appeals. In the following sections, I only present results using cases in basic people's courts and intermediate people's courts. I present the results using cases in higher-level courts in the web appendix (Section IV).

Identification Strategy

To obtain an unbiased estimate of the effect of judicial hierarchy on judicial outcomes, I exploit a regulative change in 2008 that dramatically raised the monetary threshold for cases to enter each level of court. The thresholds vary across municipalities, but I use Guangzhou City in Guangdong Province as an example. Prior to 2008, economic disputes that had claims under 6 million *yuan* were under the jurisdiction of basic people's courts in Guangzhou, disputes with claims between 6 million *yuan* and 100 million *yuan* were under the jurisdiction of the intermediate people's court in Guangzhou, and disputes with claims over 100 million *yuan* were under the jurisdiction of the high people's court in Guangdong Province. Because of increasing claims in economic disputes as the Chinese economy grew, and the subsequent heavier burden on higher-level courts, the Supreme People's Court in 2008 announced a change in the jurisdictions of each level of court. After 2008, in Guangzhou City, economic disputes under 50 million *yuan* were under the jurisdiction of basic people's courts, disputes between 50 million *yuan* and 300 million *yuan* were under the jurisdiction of the intermediate people's court, and disputes above 300 million *yuan* fell to the high people's court. Figure 2 summarizes the changes.

[INSERT FIGURE 2 HERE]

The jurisdictional change in 2008 created an exogenous variation in cases' exposure to various court levels. For example, economic disputes between 6 million and 50 million *yuan* that would have been adjudicated by intermediate people's courts before 2008 (Cell 3 in Figure 2) were adjudicated by basic people's courts after 2008 (Cell 4 in Figure 2). I call these cases Treatment Group I, because they were exposed to the treatment of basic people's courts after 2008. The jurisdictional change also created control groups. For example, disputes between 50 million and 100 million *yuan* have always been under the jurisdiction of intermediate people's courts both before (Cell 5 in Figure 2) and after 2008 (Cell 6 in Figure 2). Restricting my sample to the [6 million, 100 million] range, a case's exposure to the treatment (adjudication by a basic people's court) was determined by both the amount of its claim and the year it was accepted. After controlling for region and industry fixed effects, an interaction term between dummy variables indicating post-2008 and Treatment Group I is a plausible exogenous variable, and is used as an instrument in the win rate equation. Similar strategies were used to estimate the effect of exposure to education on earnings (Card and Krueger 1992; Lemieux and Card 2001; Duflo 2001).

The basic idea behind the identification strategy is illustrated in Table 1, which shows exposure to basic people's courts and intermediate people's courts and the win rates of SOEs versus non-SOEs in different "cells." While 11.25% of cases in Cell 3 (before 2008) were adjudicated by basic people's courts, this percentage increases to 53.71% in Cell 4 (after 2008). Likewise, while 85.58% of cases in Cell 3 were adjudicated by intermediate people's courts, this percentage decreases to 45.14% in Cell 4. This 40.44% decrease is significant compared to the 3.26% decrease in the control group. This difference-in-difference (DID) estimate is also statistically significant in a regression framework with year, province, and industry fixed effects, which is presented in Section II of the web appendix. Similarly, within the same range of claims, the win rates of SOEs and non-SOEs also experienced significant changes after 2008.¹⁷ While the

¹⁷The ownership data are from CSMAR and the Chinese Economic Censuses of 2004 and 2008. Specifically, a firm is coded as an SOE if its ultimate shareholder is the government, including any departments in the government, such as the Bureau of State Asset Management or the Finance Bureau. This coding rule is consistent with prior studies, please see Wang, Wong and Xia (2008).

win rate of SOEs experienced a -2.55% (s.d. = 3.87) change in the treatment group, this change is -14.63% (s.d. = 4.56) in the control group, and the DID is therefore -12.08% (s.d. = 5.98) and significant at the 0.01 level. This implies that SOEs are less likely to win in basic people's courts. However, for non-SOEs the win rate is higher in basic people's courts (DID = 23.32%, s.d. = 8.24, $p < 0.01$). The remainder of this article will elaborate on this strategy to produce more convincing results.¹⁸

[INSERT TABLE 1 HERE]

Difference-in-Difference Estimates

To exploit the variation in treatment across cases and time periods, this strategy can be generalized to a regression framework. First, I conduct an intention-to-treat analysis (Dunning 2012, 138) to estimate the reduced-form relationship between being in the treatment group after 2008 and a firm's win rate. The intention-to-treat analysis focuses on the groups created by the randomization introduced by the 2008 jurisdictional change, although in reality the randomization was not strictly enforced. As Dunning (2012, 138) argues, intention-to-treat analysis may produce inaccurate estimates of the effect of the treatment (for instance, if many subjects in the assigned-to-treatment group did not actually receive the treatment), but analyses of natural experimental data should almost always present intention-to-treat analysis.

This suggests running the following regression:

¹⁸An alternative identification strategy is a regression discontinuity (RD) design to compare cases just above and just below the cutoff point. The success of an RD design relies on the assumption that there is no sorting around the cutoff point (Imbens and Lemieux 2008, 632). If firms manipulated the claim numbers to self-select into a certain level of court, anticipating a higher win rate, the no-manipulation assumption of the RD design is violated. Since I conduct a McCrary density test (McCrary 2008), which rejects the null hypothesis of continuity of the density of dispute claims around the cutoff points, I do not use an RD design. For results of the McCrary density test, please see Section III in the web appendix.

$$\begin{aligned}
Pr(Win_i = 1) = & \text{logit}(c_1 + \gamma_1 Post2008_i \\
& + \gamma_2 Treatment\ Group\ I_i \\
& + \gamma_3 Post2008_i \times Treatment\ Group\ I_i \\
& + X\Gamma + \alpha_{1k} + \theta_{1d} + \epsilon_i), \tag{1}
\end{aligned}$$

where Win_i is a binary variable indicating whether the firm that announced the case won, c_1 is a constant, $Post2008_i$ is an indicator for cases accepted after 2008, $Treatment\ Group\ I_i$ is an indicator for cases that had claims within the Treatment Group I range, X is a vector of covariates that might not be balanced pre- and post-treatment, α_{1k} denotes province dummies and θ_{1d} industry dummies. γ_3 is the DID estimator, and I expect that $\gamma_3 < 0$ for regressions using SOEs and $\gamma_3 > 0$ for those using non-SOEs.

All models include the following controls. $Assets(log)$ is the natural log-transformed total assets of the firm, Age is the age of the firm, and prior studies show that bigger and older firms are more likely to go to court (Ang and Jia 2014, 326). $Contract$ is an indicator for contract disputes (as apposed to loan cases, tort cases, or other cases). Prior studies have shown differential win rates across case types, with more predictability among contract cases in which both parties can observe the contractual terms and relevant actions (Kessler, Meites and Miller 1996; Shavell 1996; Siegelman and Waldfogel 1999; Lu, Pan and Zhang 2015). Finally, a large literature on the Chinese legal system has been focused on “local protectionism,” in which courts usually favor locally based litigants (O’Brien and Li 2004; Peerenboom 2002; Wang 2015). The CLFLD includes information about firms’ registration locations and courts’ locations. Because two firms are involved in a dispute, there are four possible scenarios: 1) the announcing firm and the court share the same location (at the municipal level), and the other firm is registered in a different location (I code this as *Home* (10.37% of cases)); 2) the other firm and the court share the same location, and the announcing firm is registered in a different location (I code this as *Road* (14.18% of cases)); 3) none of the firms share the same location

with the court, so the court is a third party (I code this as *Third* (4.14% of cases)); or 4) both firms share the same location with the court (I code this as *Derby* (71.30% of cases)). So the vast majority of disclosing firms (81.67%) go to their local courts to litigate. I include *Road*, *Third*, and *Derby* in the regressions, leaving the *Home* category as the reference group.

I first restrict my sample to cases in Treatment Group I or Control Group I.¹⁹ The estimated effect is then the treatment effect of adjudication after 2008 within Treatment Group I (higher exposure to basic people's courts than to intermediate people's courts) on the probability of winning. I then divide my sample into SOEs and non-SOEs based on the announcing firm's ownership and estimate the regressions separately using two sub-samples. Figure 3 presents the logistic regression results with standard errors clustered at the provincial level.

[INSERT FIGURE 3 HERE]

For SOEs, being in Treatment Group I after 2008, which increased cases' exposure to basic people's courts (as opposed to intermediate people's courts), significantly decreases the probability of winning. Firm size (measured by *Assets(log)*) and age (measured by *Age*) help firms win, which is consistent with prior findings (Ang and Jia 2014). SOEs are more likely to win contract disputes in which two parties have symmetric information (Kessler, Meites and Miller 1996; Shavell 1996; Siegelman and Waldfogel 1999; Lu, Pan and Zhang 2015). Surprisingly, I find no "home-field advantage" for SOEs; they have roughly the same win rates in all four scenarios (Home, Road, Third, and Derby). I interpret this as a result of SOEs' broad political connections that are not limited to one locality. Future research can examine how SOEs employ political connections outside jurisdictional boundaries to gain leverage in courts.

For non-SOEs, however, more exposure to basic people's courts significantly increases their probability of winning. Firm size and age have negative signs and are not statistically significant. Similar to SOEs, non-SOEs are more likely to win contract disputes. Lastly, there is a significant "home-field advantage" for non-SOEs: they are less likely to win in "road" courts than in "home" courts because they usually only have local ties that do not cross boundaries.

¹⁹I present the results using cases in Treatment Group II or Control Group II in Section IV in the web appendix.

The intention-to-treat analysis results are largely consistent with Hypothesis 1: SOEs are more likely to win in intermediate people's courts, while non-SOEs are more likely to win in basic people's courts. However, an unbiased estimate of the local average treatment effect (LATE) requires full compliance. As Table 1 indicates, although most cases ended up in the right court based on the amount of their claims, a small number of cases went to the "wrong" courts. This might be the result of unobservable case/firm/court characteristics (such as the case's political sensitivity or court burden) or simply mistakes. In addition, because only listed firms' information is publicly available, if one of the firms involved in the dispute is not public, that firm's information is unobservable to the researcher. To more accurately estimate the LATE, I use an instrumental variable (IV) estimator, which is elaborated in the next section.

Two-Stage Least-Squares Estimates

Because a case's claim amount and timing jointly determine its probability of being adjudicated by a basic people's court, I can use an interaction term between dummy variables indicating a case's designated group (treatment or control) and acceptance year to predict the probability that it will be accepted by a basic people's court, and then I can use this probability to predict the likelihood that it will win. If we assume that the interaction term has no effect on the probability of winning other than by changing the court level (exclusion restriction), one can use the interaction term to conduct IV estimates of the effect of judicial level on judicial outcomes.

To meet this exclusion restriction assumption, I need to assume that the 2008 regulatory change assigned cases to treatment or control groups at random or at least as-if random. One scenario that could potentially violate this assumption is if firms strategically self-selected into one group or another in anticipation of the jurisdictional change. Imagine a privately owned firm that had a 10 million *yuan* dispute in Guangzhou City, which would go to an intermediate people's court in 2007. However, after learning that the cutoff point would be raised in 2008, and that the case would instead end up in a basic people's court, which would increase its probability of winning, the firm might have decided to wait until after 2008 to pursue the case. Or firms might have chosen to file under the new 2008 system when previously they would not

have, or vice versa.

I present several pieces of evidence to show that this strategic sorting is difficult. First of all, the timing of the 2008 Supreme People’s Court ruling was exogenously determined. The 2008 jurisdictional change was made in direct response to the passing of the new Civil Procedural Law, which came into effect on April 1, 2008. The new law changed its Article 178 to prohibit a party from applying to the people’s court that originally tried the case for retrial. After the law came into effect, all appeals were required to be filed in a higher-level court. This change dramatically increased the burden of higher-level courts, especially intermediate people’s courts and high people’s courts. The 2008 jurisdictional change was then made to alleviate the burden of higher-level courts.²⁰ Second, the thresholds for various levels of courts vary significantly across localities even within the same province. For example, while the new threshold for entering intermediate people’s courts in Guangzhou City is 50 million *yuan*, it is 30 million in Zhongshan City, and 20 million in Chaozhou City. The lack of a nationally unified standard made strategic calculations difficult. Third, there is no evidence in the data that this strategic behavior occurred. There were 92 cases involving non-SOEs entering courts in 2007 and 90 in 2008. Similarly, there were 289 cases involving SOEs in 2007 and 270 in 2008. I do not observe any irregularities around 2008.

Under the assumption that the interaction between cases’ designated group and timing has no direct effect on their probability of winning, the interaction term is available as a valid instrument for the treatment. This strategy, an IV estimator, has been used in other similar situations (Card and Krueger 1992; Lemieux and Card 2001; Duflo 2001). This instrument has been shown to have good explanatory power in the first stage, which is presented in Section V in the web appendix. I use two-stage least squares (2SLS) to fit the following equation:

$$\begin{aligned} Win_i = & c_1 + \gamma_1 Basic_i(instrumented) \\ & + X\Gamma + \alpha_{1k} + \theta_{1d} + \epsilon_i, \end{aligned} \tag{2}$$

²⁰For more details, please see http://news.xinhuanet.com/legal/2008-01/28/content_7509567.htm (Accessed February 19, 2015).

where $Basic_i$ is a binary variable indicating whether the case was adjudicated by a basic people’s court (intermediate people’s court as the reference group), and it is instrumented by $Post2008_i \times Treatment\ Group\ I_i$. γ_1 is the quantity of interest—the LATE of basic people’s courts—and I expect that $\gamma_1 < 0$ for regressions using SOEs and $\gamma_1 > 0$ for those using non-SOEs.

[INSERT FIGURE 4 HERE]

Figure 4 shows the results, which are largely consistent with the DID estimates. For SOEs, adjudication by basic people’s courts (as opposed to intermediate people’s courts) significantly decreases their probability of winning. Firm size and age have a positive effect on the probability of winning, and SOEs are more likely to win contract disputes. Similarly, I do not find a “home-field advantage” for SOEs. Conversely, basic people’s courts significantly increase non-SOEs’ probability of winning. However, firm size and age do not seem to matter for non-SOEs. They are more likely to win contract disputes, and there is a significant “home” effect for non-SOEs: they are more likely to win at home than on the road.

So far, I have only included variables measuring one side of the litigation (the discloser) without considering its competitor, under the assumption that the 2008 policy change also randomized the competitors into treatment and control. The evidence above indicates that private firms are more likely to win in lower-level courts regardless of whether their opponents are private or state owned. Below, I provide direct evidence that even facing an SOE, a private enterprise is more likely to win in lower-level courts. Unfortunately, not every firm on the other side is publicly traded and, therefore, releases its ownership information. For 636 unique pairs of firms that are both listed, I am able to collect ownership information on both sides. Below, I restrict my analysis to litigations between a non-SOE and an SOE. My theory predicts that non-SOEs are more likely to win against SOEs in basic people’s courts than in intermediate people’s courts. Table 2 shows the IV estimates with $Basic$ instrumented by $Post2008 \times Treatment\ Group\ I$. Again, both province and industry fixed effects are included, and I cluster standard errors at the provincial level.

[INSERT TABLE 2 HERE]

As shown, non-SOEs are more likely to win against SOEs in basic people's courts than in intermediate people's courts. Put differently, SOEs are more likely to lose to non-SOEs in basic people's courts than in intermediate people's courts. The point estimate of *Basic* is very similar to that in Figure 4, indicating that the 2008 change indeed randomized competitors into control and treatment groups. This provides the strongest, direct evidence to support my theory.

In sum, both the DID and 2SLS results support Hypothesis 1 that there is relative capture in the Chinese judicial hierarchy: while SOEs are more likely to win in intermediate people's courts, non-SOEs are more likely to win in basic people's courts. I now turn to three potential sources that could bias my estimates.

Robustness Checks

I conduct three checks to test the robustness of the results against potential sources of bias. First, because cases will enter CLFLD only if firms have chosen to go to court, my analysis does not include firms that settled their disputes outside the courts, such as through arbitration or mediation (alternative dispute resolution). This could create a selection bias if, for instance, SOEs are more likely to win because they are more likely to go to court in the first place when they have a dispute. Prior studies have indeed shown that SOEs are more likely to litigate than non-SOEs (Wang 2015, 94). A similar selection bias is presented in the discussion of how to estimate the treatment effect of signing international treaties on countries' compliance behavior: because compliant countries are more likely to sign these treaties in the first place, simply regressing compliance on treaties could introduce significant selection bias (Simmons 2000; Von Stein 2005). A conventional solution to selection bias is to adapt a Heckman selection model to first predict firms' likelihood to litigate and then use this likelihood to predict their win rate.²¹ However, as Simmons and Hopkins (2005) show, Heckman-style models share several important weaknesses, including their sensitivity to specification, possible problems of collinearity, and heavy reliance on distributional assumptions. For these reasons, recent methodological work on selection bias has focused on finding alternatives to the Heckman approach, often through

²¹For more on Heckman's initial work on selection bias, please see Heckman (1979).

semiparametric or nonparametric models (Heckman et al. 1998). Nonparametric approaches include matching control for bias on observables without making the strong distributional assumptions required by Heckman-type models. And in recent work, nonparametric approaches have also demonstrated their utility when confronting thorny problems related to nonrandom assignment to treatment (Imai 2005).

I wish to compare the firms' win rates in basic people's courts with those in intermediate people's courts. I assume that the 2008 jurisdictional change assigned cases to treatment and control groups at random or at least as-if random. Thus matching cases in Cells 3 and 4 in Figure 2 could create a balanced, matched dataset. I employ a genetic matching procedure, which is shown to achieve a better balance between the "control" and "treatment" groups (Diamond and Sekhon 2013).²² Using the matched data, I conduct genetic matching to estimate the treatment effect of *BASIC*—a binary indicator of basic people's courts. The results are in Table 3. Consistent with prior parametric models, SOEs are less likely to win in basic people's courts, whereas non-SOEs are more likely to win in basic people's courts. Substantively, SOEs' win rate is estimated to be 30% higher in intermediate people's courts than in basic people's courts, while non-SOEs' win rate is 40% higher in basic people's courts than in intermediate people's courts. The effect's size is nontrivial.

[INSERT TABLE 3 HERE]

Second, some recent studies have pointed to the importance of firms' political connections in determining their preference for litigation (Lu, Pan and Zhang 2015; Ang and Jia 2014; Wang 2015). If political connections are correlated with both court-level and judicial outcomes, my previous estimates would suffer from omitted variable bias. To measure firms' political connections, I obtained the biographical information of all board members (chairperson, president, vice-president, CEO, executive director, non-executive director, or secretary) for all of the involved companies from Wind Info, a leading integrated service provider of financial data based in Shanghai.²³ I then manually coded the career information of each board member in each

²²My results indicate dramatic improvements in balance (Section VI in the web appendix).

²³<http://www.wind.com.cn/En/> (Accessed August 2, 2013).

firm to determine whether a member was politically connected.²⁴ This “board” approach is consistent with the identification of political connections in the previous literature (Agrawal and Knoeber 2001; Boubakri, Cosset and Saffar 2008; Sun, Xu and Zhou 2011).

I focus on the following three types of connections: 1) GOVERNMENT CONNECTION—a firm is connected to the Chinese government if at least one of its board members was a government official; 2) PARLIAMENT CONNECTION—a firm is connected to China’s parliament (people’s congress or people’s consultative conference) if at least one of its board members was a member of parliament; and 3) LEGAL CONNECTION (a subset of GOVERNMENT CONNECTION)—a firm is connected to China’s legal organizations if at least one of its board members was an official in police departments, the courts, procuratorates, or legal bureaus. Adding these variables does not change my original results (Section VII in the web appendix).

Third, there are two types of SOEs in China: a few centrally controlled SOEs and many locally controlled SOEs. Central SOEs only pay taxes to the central government. If courts’ preferences at different levels are shaped by local governments’ tax incentives, then I should exclude central SOEs from my analysis. I hence exclude the small number of central SOEs in my sample and find that my prior results still hold (Section VIII in the web appendix).

Discussion and Conclusion

Recently, many countries that have experienced the disadvantages of decentralization have begun to centralize their political systems to distance politics from special interests. However, whether these recentralization reforms can reduce capture is open to question and represents an area for future empirical research.

I argue that the framework of relative capture is useful in examining politicians’ and interest groups’ incentives at different levels of government. The key insight is that a political hierarchy can be relatively captured by different interest groups at different levels. I then use a novel dataset of real court cases disclosed by Chinese publicly traded firms to show that cases brought

²⁴Every board member was double-coded by a group of research assistants and me. Table 7.1 in the web appendix shows two examples of board members’ biographies.

by different firms win at different rates at different levels of the judiciary. I argue that the Chinese local governments' tax incentives shape the preferences of local judges: courts are more likely to favor firms that provide a major source of revenue to the party-state at the same territorial level. Exploiting a natural experiment in which the Supreme People's Court in China dramatically raised the monetary threshold for cases to enter higher-level courts, which created exogenous variation in cases' exposure to various levels of courts, I use an IV strategy to estimate the treatment effect of judicial level on judicial outcomes. I report strong evidence that while SOEs are more likely to win in intermediate people's courts, non-SOEs are more likely to win in basic people's courts. The results are robust to corrections of selection bias using genetic matching, considering firms' political connections, and excluding centrally controlled SOEs.

These points lead to an important policy implication. Recently, many developing countries, including China and Vietnam, have carried out judicial reforms to promote the rule of law. An important component of these reforms is to centralizing the judiciary to avoid "local protectionism." However, as this study shows, higher-level courts favor a different type of firm; without fundamental changes to the political structure, centralizing judicial management in higher-level courts would only transform local protectionism into "higher-level protectionism" that favors litigants with connections to higher-level governments.

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Table 1: Means of Court Level and Win Rates by Firm Ownership and Cells

	Pre-2008	Post-2008	Difference
Control Group I	Cell 5	Cell 6	
Observations	750	245	
Intermediate	83.624 (1.357)	86.885 (2.165)	-3.261 (2.675)
Win Rate of SOEs	35.372 (1.945)	50.000 (4.272)	-14.628 (4.556)
Win Rate of Non-SOEs	35.714 (4.286)	39.394 (4.936)	-3.680 (6.521)
Treatment Group I	Cell 3	Cell 4	
Observations	1,175	355	
Intermediate	85.579 (1.030)	45.143 (2.664)	40.437 (2.379)
Basic	11.245 (0.926)	53.714 (2.669)	-42.470 (2.234)
Win Rate of SOEs	42.950 (1.631)	45.500 (3.530)	-2.550 (3.869)
Win Rate of Non-SOEs	28.634 (3.007)	55.634 (4.184)	-26.999 (5.040)

Notes: The sample is comprised of Chinese publicly traded firms in the CLFLD. The ownership data are from CSMAR and the Chinese Economic Censuses of 2004 and 2008. Specifically, a firm is coded as an SOE if its ultimate shareholder is the government, including any departments in the government, such as the Bureau of State Asset Management or the Finance Bureau. Standard deviations are in parentheses.

Table 2: Effect of Basic Court on Win Rates of Non-SOEs versus SOEs

Non-SOEs vs SOEs	
	Coefficient (Clustered S.E.)
Basic	0.275** (0.114)
ASSETS(log)	0.005 (0.017)
AGE	-0.004 (0.007)
CONTRACT	0.085** (0.037)
ROAD	-0.126*** (0.047)
THIRD	0.020 (0.095)
DERBY	-0.036 (0.061)
PROVINCE F.E.	YES
INDUSTRY F.E.	YES
INTERCEPT	0.351 (0.467)
<hr/>	
N	531
Wald χ^2	8517.49
R^2	0.185

Notes: This figure presents the 2SLS regression estimates of Equation (2) while restricting the sample to non-SOEs versus SOEs. The dependent variable is *Win*—a binary variable indicating whether the announcing firm won the case. *Basic* is an indicator for cases adjudicated by basic people’s courts (as opposed to intermediate people’s courts). It is instrumented by $POST2008 \times TREATMENT\ GROUP\ I$. *ASSETS(log)* is the natural log-transformed total assets of a firm. *AGE* is the firm’s age. *CONTRACT* indicates contract disputes. *ROAD* is an indicator that equals 1 if the other firm and the court share the same location, and the announcing firm is registered in a different location. *THIRD* is an indicator that equals 1 if none of the firms share the same location with the court, so the court is a third party. *DERBY* is an indicator that equals 1 if both firms share the same location with the court. Standard errors are clustered at the provincial level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of Adjudication by a Basic Court on Win Rates of SOEs and Non-SOEs: Genetic Matching Results

	SOEs	Non-SOEs
	Coefficient	Coefficient
	(Bootstrap S.E.)	(Bootstrap S.E.)
Basic	-0.298*** (0.099)	0.409*** (0.056)
N	91	51

Notes: This table reports the results of genetic matching using a matched dataset of cases assigned to treatment (adjudication by basic people’s courts) or control groups (adjudication by intermediate people’s courts) by the 2008 jurisdictional change. The dependent variable is *Win*—a binary outcome indicating whether the announcing firm won the case. Bootstrap standard errors are in parentheses. Section VI in the web appendix shows the balance tests. *p<0.1, **p<0.05, ***p<0.01.

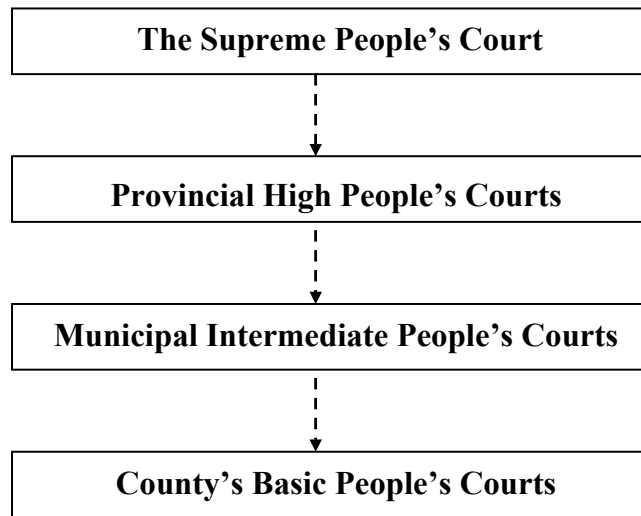


Figure 1: Hierarchy of China's Judiciary System

Notes: This figure is from Wang (2015, 62).

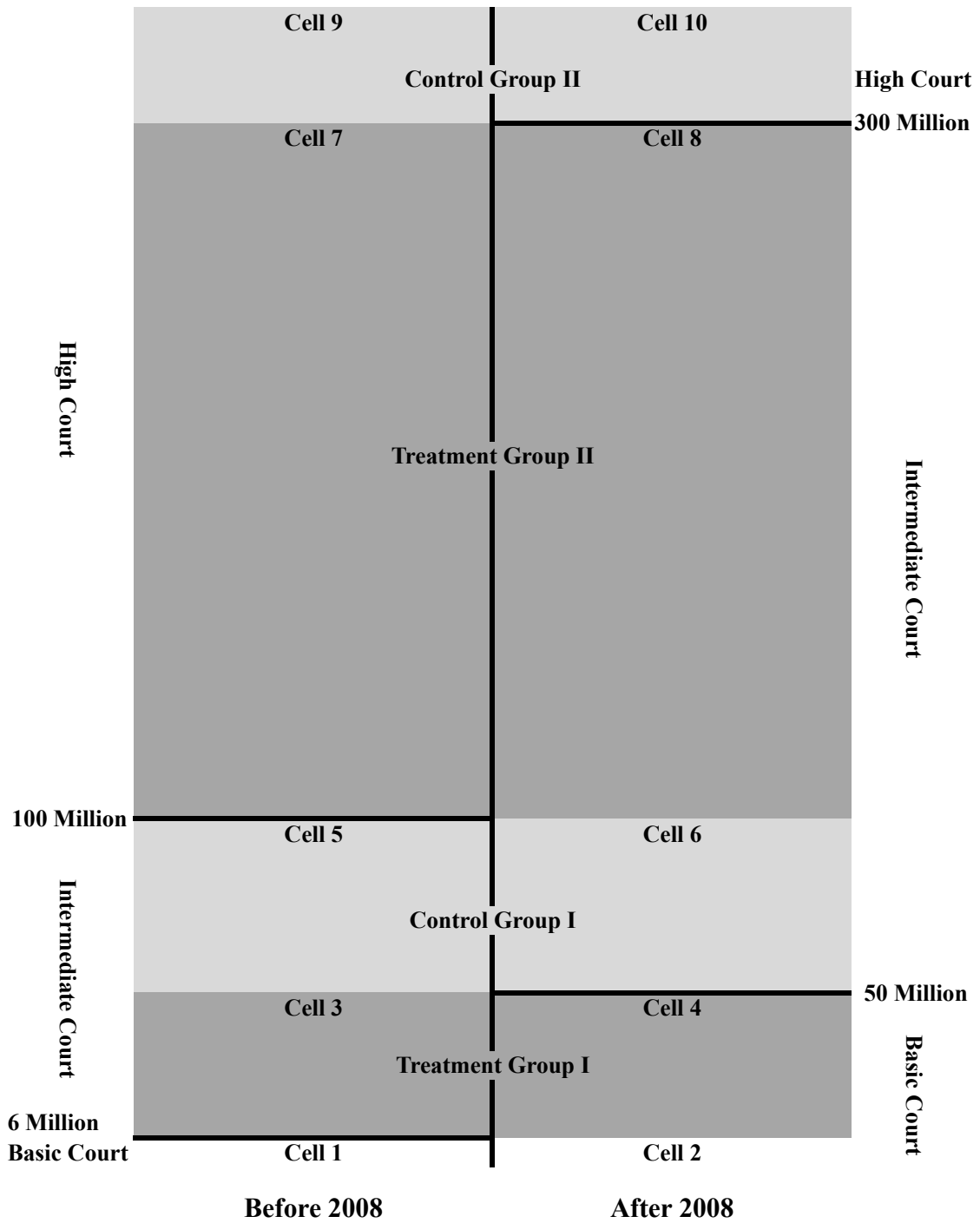


Figure 2: Court’s Jurisdictional Changes in Guangzhou City in Guangdong Province

Notes: This figure presents the two “regimes” before and after the 2008 jurisdictional change. The specific numbers are based on the policies in Guangzhou City in Guangdong Province. The cutoff points vary across municipalities.

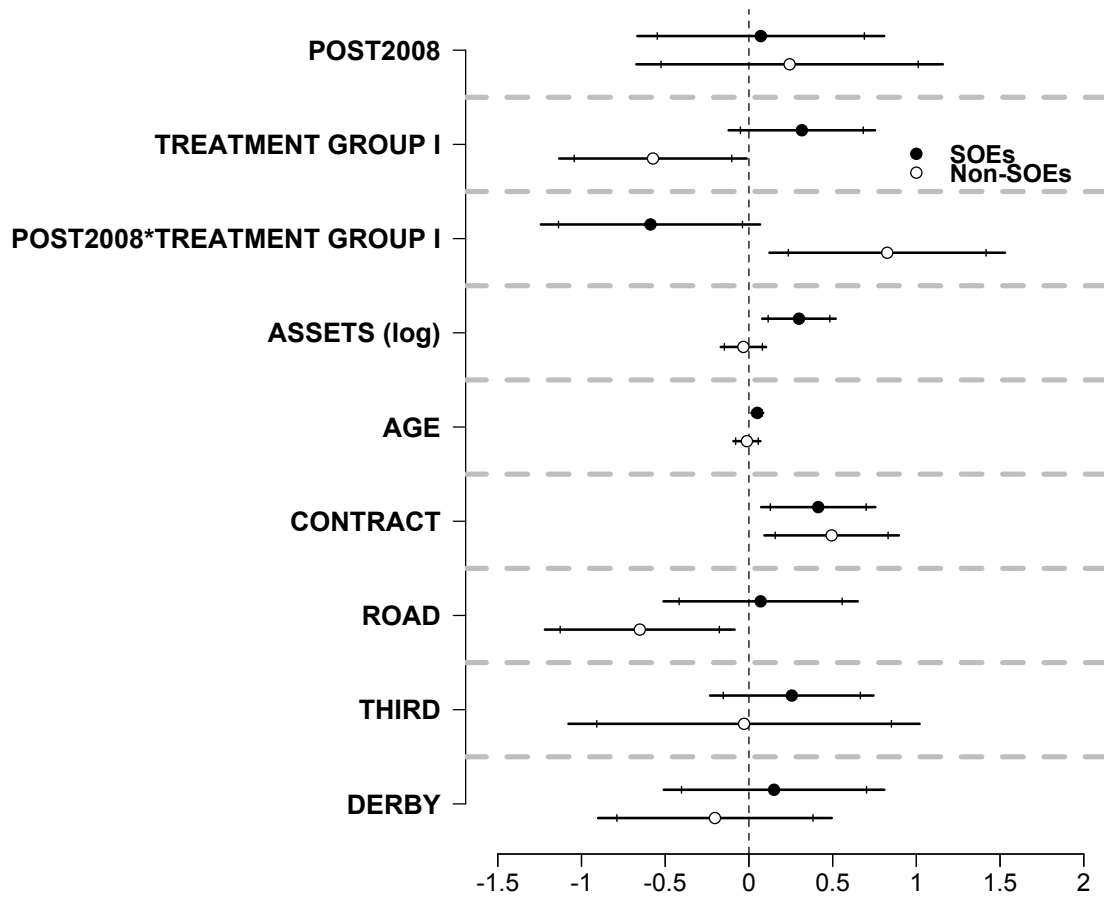


Figure 3: Effect of Exposure to Basic People's Courts on Win Rate: DID Estimates

Notes: This figure presents the logistic regression estimates of Equation (1). The dependent variable is *Win*—a binary variable indicating whether the announcing firm won the case. *POST2008* is an indicator for cases accepted after 2008. *TREATMENT GROUP I* is an indicator for cases that fell into the Treatment Group I range in Figure 2. *ASSETS(log)* is the natural log-transformed total assets of a firm. *AGE* is the firm's age. *CONTRACT* indicates contract disputes. *ROAD* is an indicator that equals 1 if the other firm and the court share the same location, and the announcing firm is registered in a different location. *THIRD* is an indicator that equals 1 if none of the firms share the same location with the court, so the court is a third party. *DERBY* is an indicator that equals 1 if both firms share the same location with the court. The black dots are estimates using the SOE sample; the white dots are estimates using the non-SOE sample. Lines represent the 95% confidence intervals, and the small bars 90% confidence intervals, both of which are based on clustered standard errors at the provincial level.

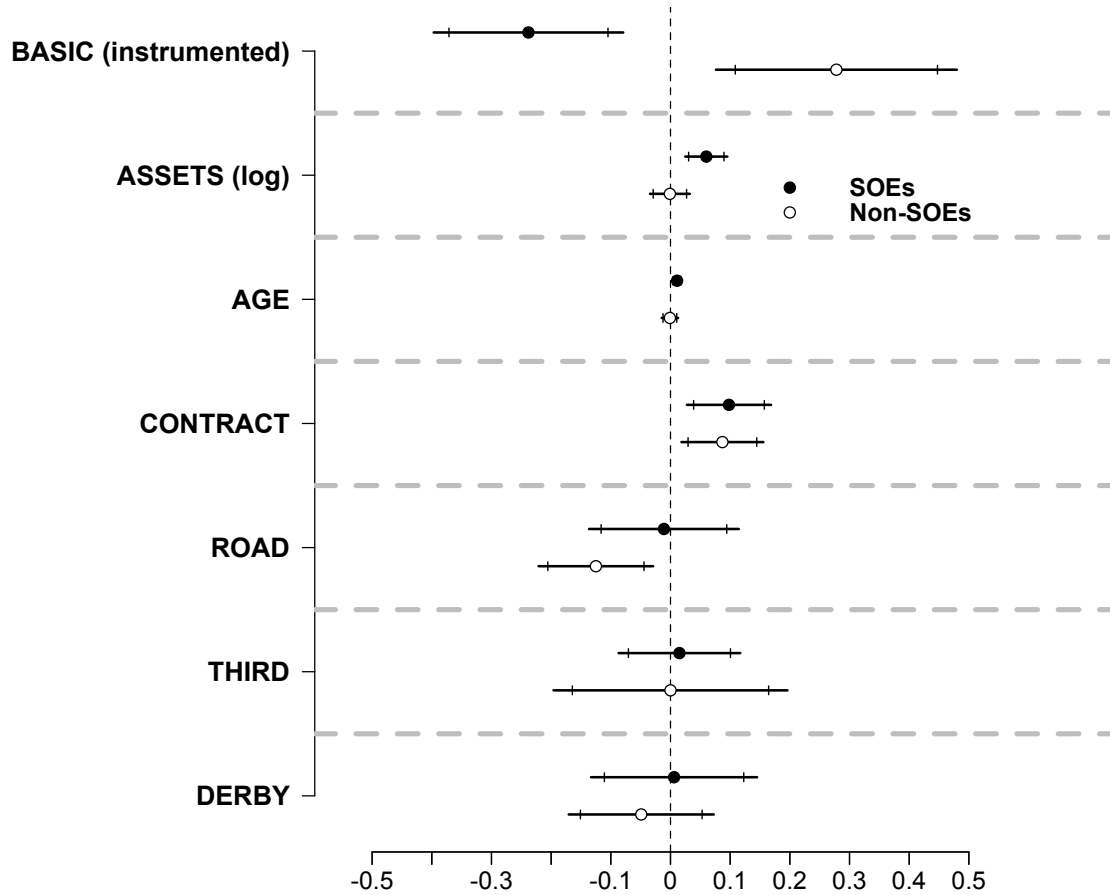


Figure 4: Effect of Exposure to Basic People's Courts on Win Rate: 2SLS Estimates

Notes: This figure presents the 2SLS regression estimates of Equation (2). The dependent variable is *Win*—a binary variable indicating whether the announcing firm won the case. *Basic* is an indicator for cases adjudicated by basic people's courts (as opposed to intermediate people's courts). It is instrumented by $POST2008 \times TREATMENT\ GROUP\ I$. *ASSETS(log)* is the natural log-transformed total assets of a firm. *AGE* is the firm's age. *CONTRACT* indicates contract disputes. *ROAD* is an indicator that equals 1 if the other firm and the court share the same location, and the announcing firm is registered in a different location. *THIRD* is an indicator that equals 1 if none of the firms share the same location with the court, so the court is a third party. *DERBY* is an indicator that equals 1 if both firms share the same location with the court. The black dots are estimates using the SOE sample; the white dots are estimates using the non-SOE sample. Lines represent the 95% confidence intervals, and the small bars 90% confidence intervals, both of which are based on clustered standard errors at the provincial level.