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Relative Capture: Quasi-Experimental Evidence From the Chinese Judiciary

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Abstract

There has been a long-held view since the *Federalist Papers* 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 challenge this assumption by arguing 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 quasi-experiment in which the Supreme People's Court dramatically raised the threshold for entering higher level courts in 2008, I show that privately owned enterprises are more likely to win in lower level courts, and state-owned enterprises are more likely to win in higher level courts. I then employ qualitative interviews to explore the mechanisms behind them. The findings challenge an underlying assumption in the decentralization literature and have important policy implications for countries that are trapped in centralization/decentralization cycles.

Keywords

relative capture, decentralization, authoritarian judiciary, China, quasi-experiment

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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, & Jay, 1961). Empirical work finds that granting autonomy to local governments facilitates elite capture and creates a "company town" atmosphere (Campos & Hellman, 2005; Malesky & Taussig, 2009; Reinikka & Svensson, 2004; van der Kamp, Lorentzen, & Mattingly, 2016). Some recent work shows that decentralized systems are more corrupt and have weaker law enforcement than their centralized counterparts, and other studies find that centralization can significantly help eliminate local elite corruption and generate better group outcomes (Cai & Treisman, 2004; Grossman & Baldassarri, 2012; Malesky, Nguyen, & Tran, 2014; Treisman, 2000).

A sizable literature on local capture studies countries that decentralized their political and fiscal systems during economic reforms, such as China and Vietnam. For example, Lorentzen, Landry, and Yasuda (2014) show that large industrial firms in China prevent local governments from implementing environmental transparency measures. Mattingly (2016) argues that village elites in China use their influence to capture rents and confiscate property. Malesky et al. (2014) demonstrate that centralization has reduced elite corruption in Vietnam and improved public service delivery.

In response to local capture, China and Vietnam have *re*centralized their political systems to correct "local protectionism." For example, the Chinese Communist Party announced a "rule-of-law" reform in 2014 to centralize the political and fiscal management of the judiciary, which has been accused of protecting local vested interests.[1] Similarly, 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]

Most studies on local capture, however, have focused on a single level of local government and analyzed the set of actors that capture that level. But capture also takes place at higher levels. Wang (2016) shows that in the last two decades, Chinese firms have increasingly built connections with higher level governments to exert influence across localities. Likewise, Eaton and Kostka (2017) demonstrate that central state-owned enterprises (SOEs) have been shielded from the enforcement of environmental regulations in China. Therefore, we need a general theory about who captures what level of government—and why.

I argue that *capture exists at every level*, and that the *capturers are different at different levels*. I use the analytical framework of *relative capture* to contend that interest groups have different incentives and capacities to influence different levels of government. Due to political and economic

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 strengthens that group's incentive and capacity to engage in *higher* capture; a lower level government's fiscal dependence on another type of interest group creates a tendency for *lower* capture. The relative capture framework does not make the monotonic 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.

In contrast to previous studies that focus on only one level of government, I examine how capture works at multiple levels. Empirically, I investigate how judges at different judicial levels adjudicate corporate lawsuits in China. China has one of the world's largest decentralized political systems, and its judiciary represents a typical decentralized system in which local authorities have been delegated considerable fiscal, administrative, and political power (Landry, 2008a; Mertha, 2005). By examining how judges at different tiers handle cases brought by different firms, we can inspect both how a decentralized system functions and how various interest groups operate.

China has a fragmented bureaucratic structure (Lieberthal, 1992; Mertha, 2009) in which the judiciary answers to the territorial party-state (horizontal authority) rather than a higher level court (vertical authority). I study how the linkages between litigants and the party-state influence judges' decisions. Chinese judges' incentive structure is shaped by the local government tax system. While a larger percentage of private firms' tax payments goes to county governments (which control basic courts), SOEs are the major revenue source for prefectural governments (which control intermediate courts). This fragmentation between the horizontal and vertical lines of authority creates different opportunities and incentives for judges and firms at different levels. I expect that privately owned firms are more likely to win in lower level courts (basic courts), and that SOEs are more likely to win in higher level courts (intermediate courts).

Rigorously testing relative capture would require a counterfactual in which the same interest group is treated at different levels in the judiciary. But this counterfactual does not exist, 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 quasi-experiment in which the Chinese Supreme People's Court dramatically raised the monetary threshold for cases heard by higher level courts in 2008. For example, before 2008, economic disputes with claims higher than 6 million *yuan* (roughly US$920,000) were under the jurisdiction

of intermediate courts; cases less than this amount were under the jurisdiction of basic courts. This cutoff was raised to 50 million *yuan* (roughly US$7.7 million) in 2008. The jurisdictional change shifted many cases between 6 million and 50 million *yuan* (treatment group) that would have been adjudicated by intermediate courts before 2008 to basic courts after 2008. This policy change therefore creates exogenous variations in firms' exposure to different judicial levels.

I compiled and analyzed an original data set of over 4,000 court cases disclosed by publicly traded firms from 1998 to 2013 in which I detect relative capture. For every four cases, SOEs win one more in intermediate than in basic courts, while non-SOEs win one more in basic than in intermediate courts.

Because it is difficult to collect systematic, quantitative evidence on covert firm operations, I employ qualitative interviews to explore how firms influence governments and how governments and courts respond to corporate pressure. Drawing on my interviews with officials, judges, firm managers, and lawyers, I show that firms use voice and exit to influence court decisions, and that judges bow to the pressure of local fiscal imperatives.

The findings have implications for developing countries that struggle to balance their private and public sectors. SOEs are a major interest group that obstructs economic reforms, bends regulations, and causes developmental problems, such as pollution and corruption (Dasgupta, Laplante, Mamingi, & Wang, 2001; Haggard, 1990; Hellman, Jones, & Kaufmann, 2003). The World Bank champions privatization as a remedy for the sins of SOEs, an approach that is encapsulated in the Washington Consensus (Lieberman, 1993). This approach assumes that state capture will be reduced (or even eliminated) when resources are transferred to the private sector. As I show, however, SOEs and private firms can both capture the state, if certain incentive structures are present. Privatization only shifts capture from one level to another.

I present, to my knowledge, the first quasi-experimental evidence on whether (and how) interest groups capture governments at different levels. My findings challenge the long-held assumption that the lower the level of government, the greater is the extent of capture by vested interests.

This article also contributes to the literature on authoritarian institutions. My findings speak to authoritarian rule of law and add to the scarce but growing literature on comparative judicial politics (Ferejohn, Rosenbluth, & Shipan, 2009; Gallagher, 2017; Ginsburg & Moustafa, 2008; Helmke, 2002; Moustafa, 2007). This is the first empirical study to quantitatively demonstrate that judge's rulings on business disputes are biased if a country lacks judicial independence. This conclusion is nuanced by the finding that local

authorities protect firms that contribute revenues to the local coffers. In addition, this article complements the recent literature on institutions in autocracies. While most scholars have focused on how autocrats use democratic institutions, such as legislatures and elections, to co-opt the opposition and extend their term in office (Brownlee, 2007; Gandhi & Przeworski, 2007; Magaloni, 2006; Svolik, 2012; Wright, 2008), I show that interest groups can exploit these institutions to benefit themselves.[3]

Relative Capture and the Chinese Judiciary

In this section, I elaborate on the theoretical framework of relative capture, introduce the Chinese judicial structure, and apply the theory to the Chinese context.

Theoretical Framework

Following Hellman et al. (2003), I define capture as a situation in which firms use their influence to shape the rules of the game or how games are played. Previous work has assumed that interest groups are more likely to capture lower rather than higher level governments because lobbying local governments is less costly. To influence higher level governments, interest groups need to overcome daunting collective action problems as the issue at stake becomes more complicated and bureaucratic layers multiply. This assumption of local capture is reflected in classic principal–agent models in which the principal values general welfare, whereas the agent caters to special interests (Fudenberg & Tirole, 1991).

Interest groups, however, can reap more benefits by capturing higher level governments. Higher level governments make and enforce policies within a larger geographic area, and thus can break local boundaries and grant interest groups preferential treatment to a greater extent. By capturing higher level bureaucracy, interest groups can expand their rent-seeking activities to a larger administrative area or even the whole country. For many firms, the benefits of lobbying higher level governments outweigh the higher costs. So interest groups with different capacities and incentives can capture different levels of politics.

Bardhan and Mookherjee (2000) develop a framework of *relative capture* based on a model of electoral competition that is subject to the influence of special interest groups. In their basic setup, two parties are competing, and their policy platforms are influenced by campaign contributions from a lobby. The model generates the 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, under majoritarian electoral systems, only the dominant party wins and gains representation in national policy making, politicians rely more on campaign contributions to win, and interest groups have higher stakes in the election, which creates greater capture at the national level.

Although Bardhan and Mookherjee's model is not directly applicable to an authoritarian context, its insight that politicians and interest groups have different incentives and capacities at different levels has important implications for how relative capture operates in autocracies. For example, if politicians at different levels depend on different interest groups for tax revenue (equivalent to campaign funding in Bardhan & Mookherjee's [2000] model), then this might make politicians at different levels vulnerable to different interest groups. Below, I briefly describe China's judicial system and discuss how relative capture can explain judges' incentives.

The Chinese Context

The Chinese judiciary is embedded in a fragmented and decentralized political system in which government organizations at different levels (and in different hierarchies) have conflicting policy goals (Lieberthal, 1992; Mertha, 2009). This "fragmented authoritarianism" controls the judiciary in which the court is the agent of dual principals. The first (vertical) principal is the higher level court. Although there is a four-level judicial hierarchy from the Supreme People's Court to basic people's courts (see Figure 1), lower level courts are not required to obey orders from higher level courts (Wang, 2015, pp. 76-79). Higher level courts do, however, have limited power to evaluate lower level courts and make personnel decisions.[4] For example, the Supreme Court cannot independently appoint presidents or other major court officials at the provincial level; nor is the Supreme Court responsible for financing lower level courts (Wang, 2015, pp. 68-70). Whereas 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 court judge remarked, "We do our best to respect basic courts' decisions. We do not correct if it is a borderline case unless there is a fatal mistake."[6]

The second (horizontal) principal is the territorial party-state. The party-state at each level, including the 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, pp. 76-77). For example, the county party committee and government have the prerogative to

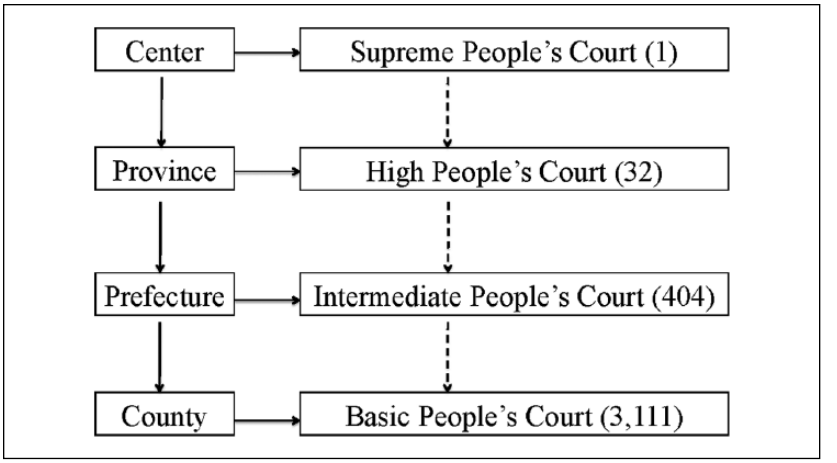

Figure 1. Hierarchy of China's judiciary.
This figure presents the structure of the Chinese judiciary. The numbers in parentheses represent the number of courts at each level.

appoint presidents and other major officials at basic courts, and pay the majority of basic courts' expenditures, including judges' salaries and bonuses, office supplies, vehicles, and buildings. Therefore courts' policy preferences are more responsive to those of the corresponding party-state than the higher level court.

To understand judges' policy preferences, we therefore need to examine the incentive structure of party-state officials. Local officials' priority is to maximize tax revenues, which help their promotions (X. Lü & Landry, 2014; Shih, Adolph, & Liu, 2012). In the current fiscal system, the central government and local governments share tax revenues. While the center collects some corporate and value-added tax (but not sales tax), various levels of local government collect the rest.[7] In the current tax-sharing system, after the center collects the central tax, most SOE tax revenue is distributed to the prefectural governments (the principal of intermediate courts), while most private company tax revenue belongs to the county-level governments (principal of basic courts).[8]

As agents of their territorial party-state, intermediate courts favor SOEs, and basic courts non-SOEs. Because higher level courts usually defer to lower level court decisions, it is difficult for SOEs to influence basic court decisions by pressuring intermediate courts. SOEs may lobby prefectural governments or party committees to intervene in cases adjudicated by basic courts, but the costs are high (because of the extended layers of bureaucratic

control) and the benefits are low (winning a low-stake case at the county level). This indirect lobbying is ineffective if the prefecture has other priorities, or if the county has some bargaining power vis-à-vis the prefecture. For example, prefectural governments need to rely on county governments to fulfill some central mandates, such as population control, social stability maintenance, and economic development; prefectures sometimes concede on other issues to maintain counties' cooperation.[9] Hypothesis 1 summarizes my theoretical expectation:

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

Empirics

I face two empirical challenges in exploring relative capture in an authoritarian judiciary. First, unlike studies of the U.S. judicial system that can utilize widely published cases, the judiciary in authoritarian regimes is opaque, so no systematic court data are available. Legal studies have relied on subjective measures of litigants' *preferences* for litigation (Ang & Jia, 2014; Gallagher, 2006; Landry, 2008b). Few empirical studies have been able to systematically examine court outcomes in authoritarian regimes, especially at the local level.[10] The second challenge is that, due to the heterogeneity of cases across different levels, simply regressing case outcomes on the judicial level produces biased estimates. A conventional regression framework fails to control for unobservable firm- and court-level covariates, which produces omitted variable bias. In addition, litigants can self-select into a sympathetic court, leading to selection bias.

Data

I use a manually coded data set of litigations disclosed by Chinese publicly traded firms from 1998 to 2013. In 1998, the Shanghai and Shenzhen Stock Exchange issued a rule that required listed companies to disclose their litigations.[11] This mandatory disclosure requirement covers all lawsuits involving publicly traded firms since 1998. I manually collected 4,275 court litigations from company reports (annual, semiannual, quarterly, and special reports) to construct the Database of Chinese Corporate Lawsuits (DCCL).[12] The DCCL contains information on each case's legal issue, type, litigants, claim (in *yuan*), outcome, timing, court name, and level, as well as additional details. Most cases in the data set are economic disputes. I then merged the DCCL with a data set that contains firm-level variables—such as state share,

registration location, age, total assets, and industry—collected from the China Securities Market and Account Research (CSMAR) database.[13] Table 1.1 in the online appendix presents the main variables' summary statistics.

Of the 4,275 observations, 39.12% are loan disputes, 47.33% contract disputes, and 13.55% other types of disputes; 73.19% involved SOEs, and 26.81% non-SOEs.[14] The average win rate of SOEs is 45.00%, and that of non-SOEs is 40.66%. I cannot judge whether a firm *should* have won or lost, which requires scrutinizing all the available evidence and applying relevant laws. I am only interested in *relative* win rates. In a fair judicial system, a case should have the same outcomes at different judicial levels; different win rates at different levels would indicate different standards in the system.

Because only the discloser is listed and publicizes firm information, we know little about its litigating opponent unless it is also listed. For 636 unique pairs, I find information on both sides of the litigation: 36.64% of these pairs are SOEs versus non-SOEs, 49.37% are SOEs versus SOEs, and 13.99% are non-SOEs versus non-SOEs. For the main analysis, I use all 4,275 observations to leverage statistical power, assuming that the quasi-experiment randomly assigned opponents into treatment and control groups. I will show that when I focus on the subset of 636 cases, the results are similar.

Basic courts adjudicated 1,034 cases (24.64%), intermediate courts 2,689 cases (64.08%), high courts 467 cases (11.13%), and the Supreme Court six cases (0.14%). The distribution is skewed because basic and intermediate courts serve as the first-instance courts for most cases, and higher level courts only handle appeals. The following sections present the results from basic and intermediate courts; the results from higher level courts are available in the online appendix.

Identification Strategy

My identification strategy relies on a regulatory change that exogenously determines cases' exposure to various court levels. In 2008, the Supreme People's Court announced a regulatory change that dramatically raised the monetary threshold for cases to enter higher level courts. The thresholds vary across prefectures. I use Guangzhou City in Guangdong Province as an example. Before 2008, economic disputes that had claims under 6 million *yuan* were under the jurisdiction of basic courts, disputes between 6 million and 100 million *yuan* were under the jurisdiction of the intermediate court, and disputes over 100 million *yuan* were under the jurisdiction of the high court. As economic disputes increasingly had higher stakes, which placed a heavier burden on higher level courts, in 2008 the Supreme People's Court announced a jurisdictional change for each judicial level. After 2008, in

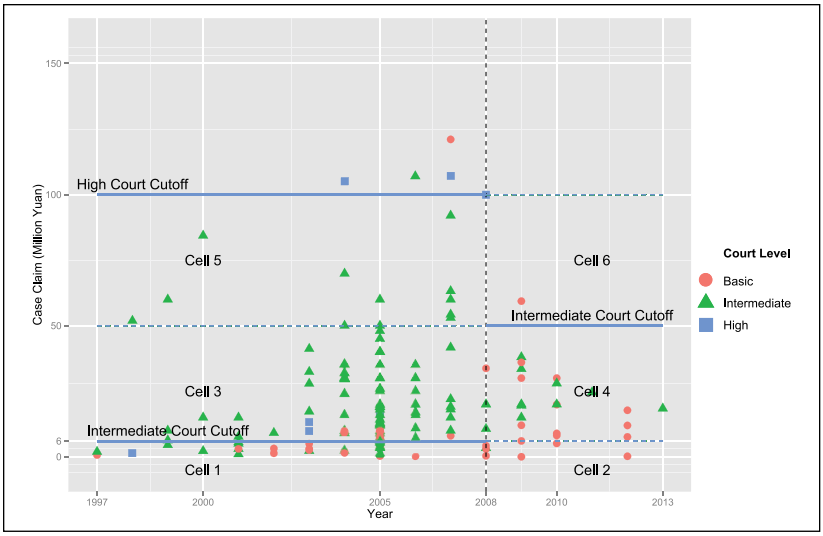

Figure 2. Courts' jurisdictional changes in Guangzhou city in Guangdong Province. This figure presents the two "regimes" before and after the 2008 jurisdictional change. The data and specific cutoffs are based on Guangzhou city in Guangdong Province. The cutoff points vary across prefectures. Circles represent cases that were adjudicated by basic courts, triangles represent cases adjudicated by intermediate courts, and cubes represent cases adjudicated by the high court.

Guangzhou, economic disputes under 50 million *yuan* were under the jurisdiction of basic courts, disputes between 50 million and 300 million *yuan* were under the jurisdiction of the intermediate court, and disputes above 300 million *yuan* were directed to the high court. Figure 2 summarizes the changes illustrated by real cases in the database.

As a result, economic disputes between 6 million and 50 million *yuan* that would have been adjudicated by intermediate courts before 2008 (Cell 3 in Figure 2) were adjudicated by basic courts after 2008 (Cell 4 in Figure 2). I call these cases Treatment Group I, because they were exposed to the treatment of basic courts after 2008. This is reflected in Figure 2: There are very few circles (basic court cases) in Cell 3 but more circles in Cell 4. The jurisdictional change also created control groups. For example, disputes between 50 million and 100 million *yuan* (Control Group I) have always been under the jurisdiction of intermediate 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 (basic court) was determined by both the amount of its claim and the year it was accepted. After

Table 1. The Allocation of Cases to Different Judicial Levels Before and After 2008.

	Pre-2008	Post-2008	Difference
Control Group I	Cell 5	Cell 6	
Observations	745	244	
Intermediate	83.624%	86.885%	−3.261%
	(1.357)	(2.165)	(2.675)
Treatment Group I	Cell 3	Cell 4	
Observations	1,165	350	
Intermediate	85.579%	45.143%	40.437%
	(1.030)	(2.664)	(2.379)
Basic	11.245%	53.714%	−42.470%
	(0.926)	(2.669)	(2.234)

The sample is comprised of Chinese publicly traded firms in the DCCL. Standard deviations are in parentheses. DCCL = Database of Chinese Corporate Lawsuits.

controlling for province 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.

Table 1 illustrates the identification strategy, showing exposure to basic and intermediate courts before and after 2008. While basic courts adjudicated 11.25% of cases in Cell 3 (before 2008), this percentage increases to 53.71% in Cell 4 (after 2008). Likewise, while intermediate courts heard 85.58% of cases in Cell 3, this percentage decreases to 45.14% in Cell 4. This 40.44% decrease is significant compared with the 3.26% decrease in the control group. The difference-in-differences (DID) estimate is also statistically significant in a regression framework with province and industry fixed effects (Table 2.2 in the online appendix).

Similarly, as Figure 3 demonstrates, the win rates of SOEs and non-SOEs also experienced significant changes after 2008. As Panel (a) shows, while the average win rate of SOEs in the treatment group (dashed line) before 2008 is 42.95%, it increases to 45.50% after 2008, which is a 2.55% (SD = 3.87) difference. As the change in the control group (solid line) is 14.63% (SD = 4.56), the DID is −12.08% (SD = 5.98) and significant at the 0.01 level. This implies that SOEs are less likely to win in basic courts. Conversely, according to Panel (b), the average win rate for non-SOEs in the treatment group (dashed line) before 2008 is 28.63%, and changes to 55.63% after 2008, which creates a difference of 27.00% (SD = 5.04). By contrast, the change in the control group (solid line) is 3.68% (SD = 6.52), so the DID is

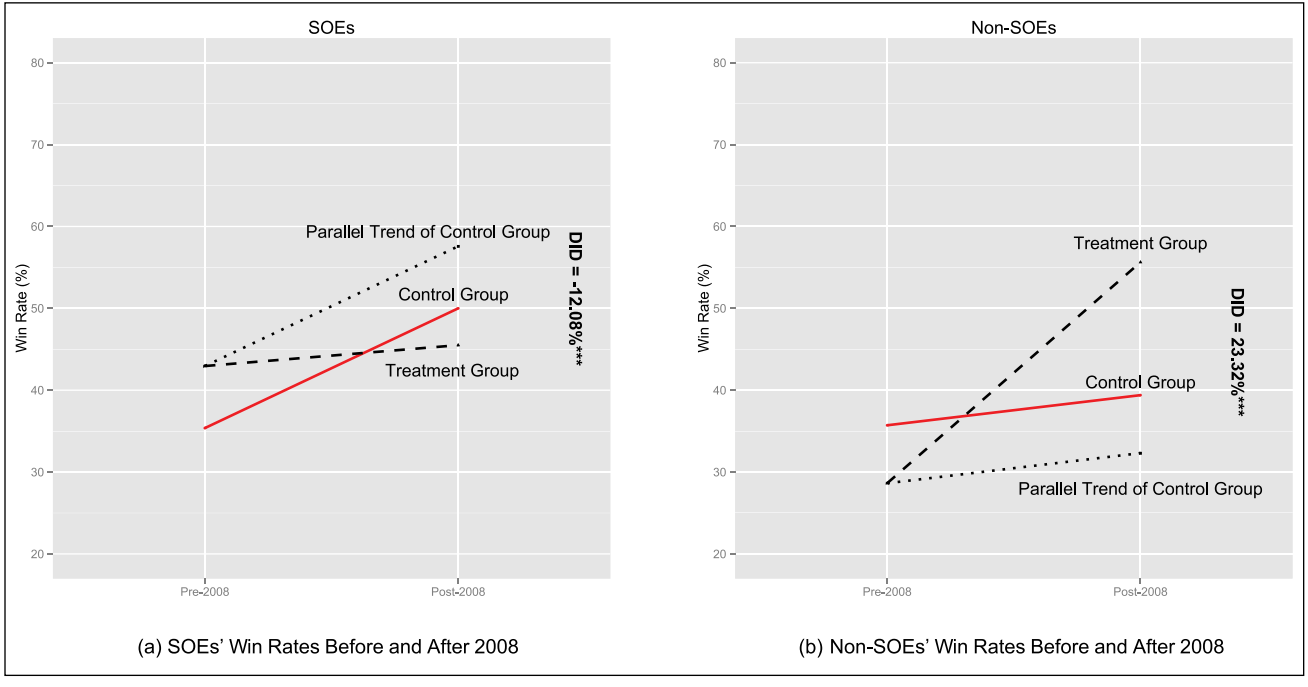

Figure 3. The 2008 policy change and win rates of SOEs and non-SOEs with real data.
The graphs present the win rates of SOEs (Panel [a]) and non-SOEs (Panel [b]) before and after 2008. The dashed lines represent cases in the treatment group, the solid lines represent cases in the control group, and the dotted lines represent the parallel trend of the control group. The DID estimate is therefore the gap between the dashed and dotted lines. SOEs = state-owned enterprises; DID = difference-in-differences.

23.32% ($SD = 8.24$, $p < .01$), indicating that non-SOEs are more likely to win in basic courts. The patterns are similar when I plot year-by-year data (Figure 2.1 in the online appendix).

I use pretreatment data to explicitly test the parallel trend assumption that is required for DID. I find that before 2008, cases in the treatment and control groups generally follow a common trend, but after 2008, SOEs are more likely to lose and non-SOEs are more likely to win in the treatment group (see Table 2.1 in the online appendix).[15] The remainder of this article will elaborate on this strategy to produce more convincing results.[16]

Difference-in-Differences Estimates

I generalize the DID strategy to a regression framework. First, I conduct an intention-to-treat (ITT) analysis to estimate the reduced-form relationship between being in the treatment group after 2008 and a firm's win rate. The ITT analysis focuses on the groups created by the randomization introduced by the 2008 jurisdictional change although the randomization was not strictly enforced. This suggests running the following regression:

$$\begin{aligned} Win_i = {} & c_1 + \gamma_1 Post2008_i + \gamma_2 Treatment\ Group\ I_i \\ & + \gamma_3 Post2008_i \times Treatment\ Group\ I_i \\ & + \mathbf{X}\Gamma + \alpha_{1k} + \theta_{1d} + \in_i, \end{aligned} \tag{1}$$

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, $\mathbf{X}$, is a vector of covariates that might not be balanced prior to 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.

I consider several firm- and court-level covariates. *Assets* (*log*) is the natural log-transformed total assets of the firm, and *Age* indicates the firm's age. Prior studies show that bigger and older firms are more likely to go to court (Ang & Jia, 2014, p. 326). *Contract Dispute* 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, & Miller, 1996). Finally, a large literature on the Chinese legal system has found that courts usually favor locally based litigants (O'Brien & Li, 2004; Peerenboom, 2002). The DCCL therefore includes information about firms' registration locations and courts' locations. There are

four scenarios, which are labeled using sports idioms: (a) the announcing firm and the court share the same location (at the prefectural level), and the opponent is registered in a different location (I code this as *Home* [10.37% of cases]); (b) the opponent and the court share the same location, and the announcing firm is registered in a different location (coded as *Road* [14.18% of cases]); (c) none of the firms share the same location with the court, so the court is a third party (*Third* [4.14% of cases]); and (d) both firms share the same location with the court (*Derby* [71.30% of cases]). So the vast majority of disclosing firms (81.67%) litigate in their local courts. I include *Road*, *Third*, and *Derby* in the regressions, leaving *Home* as the reference group.

I first restrict my sample to cases in Treatment Group I or Control Group I.[17] The estimate is therefore the treatment effect of greater exposure to basic courts (as opposed to intermediate courts) on the probability of winning. I then divide my sample into SOEs and non-SOEs based on the announcing firm's state share and estimate the regressions separately. Table 2 presents the ordinary least squares (OLS) estimates with standard errors clustered at the prefectural level.[18]

In Table 2, I first present the DID estimates without any controls to avoid posttreatment bias and then gradually add fixed effects and the controls. Columns 1 to 3 show estimates for SOEs, and columns 4 to 6 display estimates for non-SOEs. For SOEs, being in Treatment Group I after 2008, which increased cases' exposure to basic courts (as opposed to intermediate courts), significantly decreases the probability of winning. The model without any controls shows that being in the treatment group decreases SOEs' average win rate by 12.1%. Bigger or older firms are more likely to win, which is consistent with prior findings. SOEs are more likely to win contract disputes in which two parties have symmetric information. But 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 judicial leverage.

For non-SOEs, however, more exposure to basic courts significantly increases their probability of winning. Estimates in column (4) indicate that non-SOEs are 23.7% more likely to win in basic than in intermediate courts. Bigger and older firms have no advantages over smaller and younger ones. Similar to SOEs, non-SOEs are more likely to win contract disputes. Finally, non-SOEs have a significant "home-field advantage": they are less likely to win in "road" courts than in "home" courts because they usually have only local ties.

Table 2. Effect of Exposure to Basic Courts on Win Rate: DID Estimates.

	SOEs			Non-SOEs		
	(1)	(2)	(3)	(4)	(5)	(6)
	Coefficient (clustered *SE*)	Coefficient (clustered *SE*)	Coefficient (clustered *SE*)	Coefficient (clustered *SE*)	Coefficient (clustered *SE*)	Coefficient (clustered *SE*)
Post-2008	0.146**	0.121*	0.019	0.037	0.026	0.049
	(0.063)	(0.063)	(0.073)	(0.077)	(0.090)	(0.090)
Treatment Group I	0.076	0.066	0.069*	−0.071	−0.093	−0.108**
	(0.054)	(0.041)	(0.041)	(0.092)	(0.059)	(0.054)
Post-2008 × Treatment Group I	−0.121*	−0.152**	−0.132*	0.237**	0.172*	0.171*
	(0.071)	(0.065)	(0.069)	(0.098)	(0.096)	(0.097)
Assets (log)	—	—	0.063***	—	—	−0.008
			(0.020)			(0.018)
Age	—	—	0.011***	—	—	−0.003
			(0.004)			(0.009)
Contract Dispute	—	—	0.092**	—	—	0.095**
			(0.039)			(0.039)
Road	—	—	0.013	—	—	−0.122*
			(0.065)			(0.072)
Third	—	—	0.052	—	—	−0.017
			(0.056)			(0.113)
Derby	—	—	0.030	—	—	−0.040
			(0.066)			(0.078)
Province FE	*X*	√	√	*X*	√	√
Industry FE	*X*	√	√	*X*	√	√
Intercept	√	√	√	√	√	√
N	1,865	1,535	1,493	593	593	572
Number of clusters	165	152	151	93	93	91
R^2	.008	.069	.105	.047	.182	.203

This table presents the OLS estimates of Equation 1. The dependent variable is *Win*—a binary variable indicating whether the announcing firm won the case. *Post-2008* is an indicator for cases accepted after 2008. *Treatment Group I* is an indicator for cases that fell into Cells 3 and 4 in Figure 2. *Assets* (*log*) indicates a firm's natural log-transformed total assets. *Age* is the firm's age. *Contract Dispute* indicates contract disputes. *Road* is an indicator that equals 1 if the opponent 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 clustered at the prefectural level are presented in parentheses. DID = difference-in-differences; SOEs = state-owned enterprises; FE = fixed effects; OLS = ordinary least squares.

The *p* values are based on a two-tailed test: *$p < 10\%$. **$p < 5\%$. ***$p < 1\%$.

The ITT results support Hypothesis 1: SOEs are more likely to win in intermediate courts, while non-SOEs are more likely to win in basic courts. An unbiased estimate of the local average treatment effect (LATE), however, requires full compliance. As Table 1 indicates, although most cases ended up in the right court based on their claims, a small number of cases went to the "wrong" courts. Unobservable case/firm/court characteristics (e.g., the case's political sensitivity or court burden) or simply mistakes may cause noncompliance. In addition, because only listed firms' information is publicly available, unlisted firms' information is unobservable to the researcher. To accurately estimate the LATE, I use an instrumental variable (IV) approach.

Two-Stage Least Squares Estimates

Because a case's claim and timing jointly determine its probability of being adjudicated by a basic court, I can use an interaction term between a case's designated group (treatment or control) and acceptance year to predict the probability that it will be accepted by a basic court, and then use this probability to predict the win rate. If one assumes that the interaction term has no effect on winning other than by changing the court level (exclusion restriction), one can use the interaction term to conduct IV estimates. The treatment here (basic court) is compound because basic and intermediate courts vary along multiple dimensions, such as the competence and training of judges, caseloads and funding, and power and prestige.

To satisfy the exclusion restriction, I assume that the 2008 regulatory change assigned cases to treatment or control randomly or as-if random. Below, I demonstrate why the identification strategy is valid.

First, if the regulatory change indeed randomized cases into basic (treatment) and intermediate (control) courts after 2008, we should observe a perfect balance of covariates between these two groups. I find exactly that. As Table 3 presents, no covariates, including *Assets* (*log*), *Age*, *Contract Dispute*, *Home*, *Road*, *Third*, and *Derby*, are significantly different in treatment and control groups at the 95% confidence level.

The exclusion restriction would also be violated if firms strategically self-selected into one group or another in anticipation of a sympathetic court or a jurisdictional change. In one scenario, firms with certain types of political connections may select into a particular level expecting preferential treatment. Table 3, however, shows that firms' political connections are balanced between the treatment and control groups.[19]

In another scenario, imagine a privately owned firm that had a 10 million *yuan* dispute in Guangzhou City, which would have been heard by an

Table 3. Balance Tests of Covariates in Basic and Intermediate Courts after 2008.

Variable	Basic	Intermediate	Difference	95% confidence interval
Assets (log)	21.206	21.060	−0.145	[−0.464, 0.173]
Age	16.624	15.953	−0.670	[−1.862, 0.521]
Contract Dispute	0.559	0.525	−0.034	[−0.139, 0.071]
Home	0.141	0.099	−0.042	[−0.111, 0.028]
Road	0.174	0.180	0.006	[−0.075, 0.087]
Third	0.027	0.037	0.010	[−0.027, 0.047]
Derby	0.658	0.683	0.026	[−0.074, 0.126]
Government connection	0.594	0.615	0.021	[−0.091, 0.134]
Parliament connection	0.259	0.285	0.026	[−0.076, 0.128]
Legal connection	0.057	0.025	−0.031	[−0.080, 0.018]

This table presents the balance tests of covariates in treatment (basic) and control (intermediate) after 2008.

intermediate court in 2007. But after learning that the cutoff would be raised in 2008, and that the case would instead end up in a basic court—which would help it win—the firm might have decided to wait until after 2008 to file the case. Or firms might have chosen to file under the new 2008 system when previously they would not have filed at all (or vice versa).

This strategic sorting, however, is difficult. First, the timing of the Supreme Court ruling was exogenously determined. The jurisdictional change was made in direct response to the passage 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 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 and high courts. The 2008 jurisdictional change was then made to alleviate the burden of higher level courts.[20] Second, the thresholds for various levels vary significantly across prefectures even within the same province. For example, while the new threshold for entering intermediate 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 it hard for firms to strategically calculate when/if/where to file claims. Third, there is no evidence that such 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.

If the interaction between cases' designated group and timing has no direct effect on the win rate, the interaction term is then available as a valid instrument. The lower panel in Table 4 shows that this instrument has good explanatory power in the first stage.[21] The first stage yields large *F* statistics ranging from 65.44 to 103.14, which far exceeds the critical value of 10 required to avoid weak instrument bias (Staiger & Stock, 1997). I use two-stage least squares (2SLS) estimates to fit the following equation:

$$Win_i = c_1 + \gamma_1 Basic_i(instrumented) + X\Gamma + \alpha_{1k} + \theta_{1d} + \in_i, \quad (2)$$

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

The upper panel of Table 4 shows the second-stage results, which are consistent with the DID estimates. SOEs are (23.8%) less likely to win in basic courts than in intermediate courts. Firm size and age have a positive effect on winning, and SOEs are more likely to win contract disputes. Similarly, I do not find a "home-field advantage" for SOEs. Conversely, non-SOEs are (27.8%) more likely to win in basic courts than in intermediate courts. But firm size and age have no effects for non-SOEs. They are more likely to win contract disputes, and non-SOEs are more likely to win at home than on the road.

So far, I have only included variables that measure one side of the litigation (the discloser); I have not considered the opponents. 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 opponent is publicly traded and releases its ownership information. For 636 unique pairs of firms that are both listed, I collected 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 courts than in intermediate courts. Table 5 shows the IV estimates, which provide that this is indeed the case.

The point estimate of *Basic* (column [3]) is very similar to that in Table 4, indicating that the 2008 change indeed randomized opponents into control and treatment groups. This provides the strongest direct evidence to support my theory.

Table 4. Effect of Basic Courts on Win Rates: 2SLS Estimates.

	SOEs	Non-SOEs
	Coefficient (clustered *SE*)	Coefficient (clustered *SE*)
Second stage: Dependent variable = Win		
Basic (instrumented)	−0.238**	0.278***
	(0.104)	(0.106)
Assets (log)	0.060***	−0.001
	(0.017)	(0.022)
Age	0.011***	−0.001
	(0.004)	(0.008)
Contract Dispute	0.098**	0.087**
	(0.039)	(0.036)
Road	−0.011	−0.125**
	(0.068)	(0.062)
Third	0.015	0.000
	(0.056)	(0.111)
Derby	0.006	−0.049
	(0.069)	(0.071)
Province FE	√	√
Industry FE	√	√
Intercept	√	√
N	1,483	570
Number of clusters	151	91
Pseudo R^2	.093	.185
Durbin-Wu-Hausman Test (*p* value)	.156	.103
First stage: Dependent variable = Basic		
Post-2008 × Treatment Group 1	0.391***	0.496***
	(0.046)	(0.052)
Controls	√	√
Province FE	√	√
Industry FE	√	√
Intercept	√	√
N	1,522	584
R^2	.160	.453
F statistics of excluded instrument	65.44	103.14

This table presents the 2SLS regression estimates of Equation 2. The upper panel presents the second-stage results, while the lower panel presents the first-stage results. The dependent variable in the upper panel is *Win*—a binary variable indicating whether the announcing firm won the case. *Basic* is an indicator for cases adjudicated by basic (rather than intermediate) courts. It is instrumented by *Post-2008* × *Treatment Group 1*. *Assets* (*log*) indicates a firm's natural log-transformed total assets. *Age* is the firm's age. *Contract Dispute* indicates contract disputes. *Road* is an indicator that equals 1 if the opponent 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. In the lower panel, the dependent variable is *Basic*. Controls include *Assets* (*log*), *Age*, *Contract Dispute*, *Road*, *Third*, and *Derby*. All specifications include provincial and industry fixed effects. Standard errors clustered at the prefectural level are presented in parentheses. 2SLS = two-stage least squares; SOEs = state-owned enterprises; FE = fixed effects. The *p* values are based on a two-tailed test: *$p < 10\%$. **$p < 5\%$. ***$p < 1\%$.

Table 5. Effect of Basic Courts on Win Rates of Non-SOEs versus SOEs: 2SLS Estimates.

	(1)	(2)	(3)
	Coefficient (clustered *SE*)	Coefficient (clustered *SE*)	Coefficient (clustered *SE*)
Basic (instrumented)	0.434*** (0.135)	0.263** (0.128)	0.275** (0.125)
Assets (log)	—	—	0.005 (0.021)
Age	—	—	−0.004 (0.008)
Contract dispute	—	—	0.085** (0.038)
Road	—	—	−0.126** (0.059)
Third	—	—	0.020 (0.106)
Derby	—	—	−0.036 (0.065)
Province FE	X	√	√
Industry FE	X	√	√
Intercept	√	√	√
N	550	550	531
Number of clusters	91	91	89
R^2	.026	.167	.185

This table 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 (rather than intermediate) courts. It is instrumented by *Post-2008* × *Treatment Group I*. *Assets* (*log*) indicates a firm's natural log-transformed total assets. *Age* is the firm's age. *Contract Dispute* indicates contract disputes. *Road* is an indicator that equals 1 if the opponent 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 clustered at the prefectural level are presented in parentheses. SOEs = state-owned enterprises; 2SLS = two-stage least squares; FE = fixed effects.
The *p* values are based on a two-tailed test: *$p < 10\%$. **$p < 5\%$. ***$p < 1\%$.

In sum, both the DID and 2SLS results support Hypothesis 1 that there is relative capture in the Chinese judiciary. I now check the robustness of my estimates.

Robustness Checks

I conduct five robustness checks. First, recent studies have examined firms' political connections in their preference for litigation (Ang & Jia, 2014; H. Lu, Pan, & Zhang, 2015). If political connections are correlated with both court-level and judicial outcome, this would indicate that my estimates suffer from omitted variable bias. Table 3 indicates that politically connected firms do not select into a certain level, but I need to show that controlling for connections does not change my original results. To measure political connections, I collected the biographical information of these firms' board members (chairperson, president, vice president, CEO, executive director, nonexecutive director, and secretary).[22] I then manually coded the career information of each board member in each firm to determine whether a member was politically connected.[23]

I focus on the following three types of connections: (a) *Government Connection*—A firm is connected to the government if at least one of its board members was a government official. (b) *Parliament Connection*—A firm is connected to parliament (people's congress or people's consultative conference) if at least one of its board members was a member of parliament. (c) *Legal Connection* (a subset of *Government Connection*)—A firm is connected to legal organizations if at least one of its board members was an official in a police department, court, procuratorate, or legal bureau. Adding these variables does not change my original results (Table 6.2 in the online appendix).

Four additional robustness checks include using matching to tackle potential selection bias, removing centrally controlled SOEs, employing a regression discontinuity (RD)-type strategy, and using a triple interaction term between SOE share, Post-2008, and *Treatment Group I*. None of these checks change my original results (Tables 6.3 to 6.8 in the online appendix).

Mechanisms

Although quantitative data help reveal general patterns, I cannot quantify the covert operations of every firm to explore how they influence court decisions. Here, I rely on the fieldwork that I conducted in 2010 to investigate the mechanisms that connect firms and courts. My interviewees include party/government officials, judges, lawyers, and firm managers. Interviews cited below were conducted in Guangdong, Hainan, Jiangxi, and Shanghai. I relied on my personal connections to select the sites and establish initial contact. I then used snowball methods to recruit more interviewees. The goal of the qualitative fieldwork and interviews was not to make any causal inferences,

which was impossible because the cases and interviewees were not randomly selected. Instead, I relied on my interviews to understand the quantitative analyses and provide insight into how the legal system works in practice, how investors lobby courts, and what government officials think they should do to keep investors.[24]

My interviews with firm managers and lawyers showed that firms use a combination of voice and exit to influence the government. As for voice, firms usually convey their requests directly or indirectly through business associations to major leaders.[25] In addition, many firms use the threat of exit to strengthen their bargaining power with the government. For example, interviews conducted in Guangzhou City demonstrate that many Guangzhou firms threaten to move to Shanghai if their voices are not heard by the government.[26]

My interviews with government and court officials indicate that they are largely responsive to these business requests, especially when the businesses are important taxpayers. A judge explicitly told me, "You need to follow the money."[27] There are two ways in which the party-state, under pressure from businesses, manipulates judicial decisions. One is through the political legal committee (政法委)—a powerful party apparatus that controls the police, procuratorates, and courts. For cases that involve important firms, the chairperson of the political legal committee will speak directly to the court president to convey the committee's "official spirit."[28]

The second way in which the party-state manipulates judicial decisions is the through the adjudication committee (审判委员会)—an ad hoc committee within a court that consists of major officials of the court rather than a panel of judges. For these important cases, the adjudication committee will step in and make a politicized decision.[29]

I encountered a case during this fieldwork that illustrates these dynamics.[30] It involved the firm Great Ocean in the City of Buddha, which is located in a southern Chinese province. Great Ocean is an SOE owned and managed by the City of Buddha government. It 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."[31] In 2009, Great Ocean was sued by its stockholders for concealing information, and the case was accepted by the Intermediate Court of 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 court adjudication committee delivered the final verdict. The court ruled that Great Ocean won, but to discourage the stockholders from appealing, Great Ocean needed to pay a lump sum compensation to the plaintiffs, which was less than the plaintiffs originally requested. 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 social stability."[32] 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.'"[33]

In sum, my qualitative interviews helped uncover the firm–government–court links in which powerful businesses usually take deliberate actions to influence court decisions, and judges often have to yield to local fiscal imperatives when adjudicating cases involving important taxpayers.

Discussion and Conclusion

As Rodden (2006, pp. 1-2) remarks, "decentralization and the spread of federalism are perhaps the most important trends in governance around the world over the last 50 years." By the end of the 20th century, estimates of the number of decentralization reforms ranged from 80% of the world's countries to effectively all of them (Manor, 1999). Since then, further reforms have been announced in several dozen countries as diverse as Bolivia, Cambodia, Ethiopia, France, Indonesia, Japan, Peru, South Africa, South Korea, Uganda, and the United Kingdom (Faguet, 2014).

A large literature, however, argues that lower level governments are more susceptible to elite capture. 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 local elite capture.[34]

Many countries, including Vietnam and China, that have experienced the disadvantages of decentralization have begun to *re*centralize to distance politics from special interests. Both decentralization and recentralization reforms have received financial support from international donors, such as the World Bank, the United Nations Development Program, the European Union, the U.S. Agency for International Development, and the Ford Foundation.[35]

Despite the enthusiasm and generosity on both sides, prior work has offered contradictory conclusions. While some report that decentralization is associated with lower levels of corruption (Fisman & Gatti, 2002), some find that decentralization facilitates elite capture and creates more opportunities

for corruption (Mattingly, 2016; Treisman, 2000). Many developing countries lack a straightforward formula and have thus become trapped in what Baum terms "tightening/loosening cycles:" centralizing, decentralizing, and then going back to square one (Baum, 1996).

My study helps reconcile this controversy. 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 data set of real court cases disclosed by Chinese publicly traded firms to show that cases brought by different firms win at different rates at different court levels. The Chinese tax structure shapes judges' preferences: Courts favor firms that pay taxes to the territorial party-state. Exploiting a quasi-experiment in which the Supreme Court 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 estimate the treatment effect of judicial level on judicial outcomes. I demonstrate that while SOEs are more likely to win in intermediate courts, non-SOEs are more likely to win in basic courts. I employ qualitative interviews to substantiate my proposed mechanisms.

My findings generate a policy implication. Politicians and policy makers who want to reduce capture in their countries should carefully examine the relative strength of major interest groups vis-à-vis different levels of government. In countries where the level of economic development is low and most interest groups are small and local, centralization is often effective in reducing local corruption and improving governance. Indeed, Gennaioli and Rainer (2007) find that African countries that are more centralized have a better record of public goods provision, such as education, health, and infrastructure. Conversely, in countries where the economy is more developed and diverse with firms of different sizes, a balance between local autonomy and central control is crucial to avoid capture. For example, many scholars, when comparing the reform experiences of China and Russia, argue that China's transition has been more successful because the Chinese Communist Party has been in a strong position to control its local agents while maintaining local autonomy, while in Russia, old and inefficient firms benefit from preferential treatment by regional politicians whose power is not constrained by central authorities (Blanchard & Shleifer, 2001; Sonin, 2010). In these more diverse economies, emphasizing decentralization without central control often leads to "local protectionism," while too much centralization creates "central protectionism." An important question for many developing countries that are facing this conundrum is to ask "captured by whom?" and design solutions to fit local conditions.

Author's Note

All errors remain my own.

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Notes

1. See http://goo.gl/QmBk4m (accessed February 23, 2015).
2. See https://www.bhba.org/index.php/component/content/article/251 (accessed February 23, 2015).
3. For a similar dynamic in the legislature, see Truex (2014) and Hou (2015).
4. The decentralized legal system was designed to prevent politicians from using it for political purges, which were prevalent in the early years of the Communist Party. See Tanner and Green (2007).
5. Basic and intermediate 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.
6. Author's interview with a judge (personal communication, March 31, 2010).
7. For details of the tax-sharing system, see Wong and Bird (2008).
8. Although there is no uniform formula for sharing across localities, the rule of thumb is that prefectural governments collect local "critical" enterprises' taxes (most of which are SOEs), while those of other entities (most

are non-SOEs) are collected by the county government. See https://goo.gl/iZZoEg (accessed February 16, 2015). Also see examples in Harbin (https://goo.gl/UfC5oX), Anshan (https://goo.gl/wDLPgS), and Xinxiang (https://goo.gl/3BY3EP).

9. China's multiple layers of governance grant autonomy to local governments. See Qian and Xu (1993).
10. Helmke (2002) uses actual court outcomes, but at the national level.
11. See Chapter 11.1 of *Listing Rule of the Shanghai Stock Exchange* (1998; https://goo.gl/QMUpaQ) and *Listing Rule of the Shenzhen Stock Exchange* (1998; https://goo.gl/oWvTVF).
12. Every case is double coded by me and a group of trained research assistants.
13. I accessed China Securities Market and Account Research (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).
14. The state share data are from CSMAR. To simplify presentation, I define SOEs as entities with a state share greater than zero, and non-SOEs as having a zero state share. This classification is consistent with the situation in China, where the state can exert de facto control over a company as long as it has at least a small share in it. I later use a continuous measure (SOE share) for the main analysis and obtain similar results (Table 6.7 in the online appendix).
15. The results for the non-SOEs are statistically insignificant, probably due to the small number of observations, but the signs are in the right direction.
16. An alternative identification strategy is a regression discontinuity (RD) design to compare cases just above and just below the cutoff. The success of an RD design relies on the assumption that there is no sorting around the cutoff. If firms manipulated the claims to self-select into a sympathetic court, this would violate the no-manipulation assumption of the RD design. I conduct a McCrary density test, which rejects the null hypothesis of continuity of the density of dispute claims around the cutoff (Section III in the online appendix). Therefore I do not use an RD design.
17. I present the results using cases in higher level courts—Treatment Group II or Control Group II—in Table 4.2 in the online appendix.
18. I use ordinary least squares (OLS) rather than logit because nonlinear models like logit may have the incidental parameter problem with many dummy variables, which is the case here. I conduct robustness checks using logit models in the online appendix (Table 4.1) and obtain similar results.
19. I discuss how I define and code political connections in the "Robustness Checks" section.
20. For more details, see http://news.xinhuanet.com/legal/2008-01/28/content7509567.htm (accessed February 19, 2015).
21. Table 5.1 in the online appendix presents the full results of the first stage.
22. The data are from Wind Info, a leading integrated service provider of financial data based in Shanghai. See http://www.wind.com.cn/En/ (accessed August 2, 2013).

23. Every board member was double coded by a group of research assistants and me. Table 6.1 in the online appendix shows two examples of board members' biographies.
24. More information about the interviews is available in Section VII of the online appendix.
25. Author's interview with a lawyer (personal communication, March 29, 2010); author's interview with a company deputy president (personal communication, April 27, 2010).
26. Author's interview with a lawyer (personal communication, March 29, 2010).
27. Author's interview with a judge in an intermediate court (personal communication, March 23, 2010).
28. Author's interview with a political legal committee chairman (personal communication, March 15, 2010).
29. Author's interview with a court president (personal communication, March 15, 2010).
30. The transcripts of these interviews are available upon request.
31. Author's interview with a lawyer (personal communication, March 29, 2010).
32. Author's interview with a lawyer (personal communication, March 29, 2010).
33. Author's interview with a judge (personal communication, April 8, 2010).
34. For a review of the pros and cons of decentralization, see Wibbels (2006) and Treisman (2007).
35. For programs supporting centralization reforms, see https://www.bhba.org/index.php/component/content/article/251 (accessed February 23, 2015). For programs supporting decentralization, see http://www1.worldbank.org/publicsector/decentralization/operations.htm (accessed January 19, 2016), http://goo.gl/d9oD2z (accessed January 19, 2016), and http://eu-un.europa.eu/articles/en/article3160en.htm (accessed January 19, 2016).

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