

Bureaucratic Fragmentation and Judicial Outcomes in a Single-Party State

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Abstract

What determines judicial outcomes in an authoritarian state that does not protect judicial independence? Analyzing a novel dataset of over 4,000 court cases disclosed by listed firms from 1998 to 2013 in China, I present the first large-scale empirical study of how judicial hierarchy affects judicial outcomes outside the United States. I argue that the fragmentation in the Chinese bureaucracy creates different incentives for judges at different levels, and this “hierarchy” effect is shaped by local governments’ tax incentives. Exploiting a natural experiment in which the Supreme People’s Court dramatically lifted 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 (SOEs) 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. This study contributes to the scarce literature on comparative judicial politics and moves an important literature that is originated in American politics to a comparative context.

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What determines judicial outcomes in an authoritarian state that does not protect judicial independence? The most popular explanation for cross-national differences in judicial institutions is the legal origins theory, which states that judicial fairness is more likely to occur in common-law countries. [La Porta et al. \(1998\)](#) contend that judges in common-law systems have the power to interpret and create laws, whereas judges in civil-law systems can enforce only existing bodies of law. Another strand of the judicial politics literature focuses on delegative models in which powerful politicians empower (or weaken) the judiciary for political considerations ([Landes and Posner 1975](#); [Ramseyer and Rasmusen 1997](#); [Whittington 1999](#); [Ginsburg 2003](#)). For example, [McCubbins and Schwartz \(1984\)](#) suggest that the legislature needs an independent judiciary to serve as a “fire alarm” for the legislature to keep executive agencies from veering from legislative intent.¹ While the legal origins theory has strong explanatory power in cross-national studies, it falls short of explaining variations in judicial outcomes within a country when its legal origin is fixed. Meanwhile, the delegative models often ignore that courts, like legislators, are strategic actors that have their own preferences ([Helmke and Rosenbluth 2009](#)).

While most existing studies of judicial politics are conducted in the context of liberal democracies, there has been recent scholarly interest in judicial politics in authoritarian regimes ([Ginsburg and Moustafa 2008](#)). Since [Rosberg](#)'s (1995) work on Egypt, the major object of inquiry by scholars of comparative authoritarian judiciaries has been examining whether judges are a tool of the dictatorship or a possible democratizing force ([Helmke 2002](#); [Moustafa 2007](#); [Wang 2015](#)). For example, [Helmke \(2002\)](#) uses a separate-of-power model to explain why the Argentine Supreme Court rules against a weak outgoing government during the democratization period.

A key question in the judicial politics literature that is unanswered in the authoritarian context is whether the design of a judicial hierarchy has any effect on judicial outcomes. While many studies of the U.S. federal judicial system have examined the different opportunities and incentives for judges at each level of a multi-tiered judicial hierarchy ([McNollgast 1995](#); [Songer, Segal and Cameron 1994](#); [Kornhauser 1999](#); [Cameron and Kornhauser 2006](#); [Kastellec](#)

¹For a review of the judicial politics literature, please see [Helmke and Rosenbluth \(2009\)](#).

2011; Clark and Kastellec 2013), there is little, if any, empirical research on judges' incentives at different levels in authoritarian regimes. Understanding judicial decision making in authoritarian regimes is critical, because the quality of legal institutions is highly correlated with a country's long-term economic development (La Porta et al. 1997), human rights protection (Simmons 2009), quality of government (La Porta et al. 1999), and ultimately the survival of authoritarian rulers (Wang 2015).

I present, to my knowledge, the first large-scale empirical study of how judicial hierarchy affects judicial outcomes outside the United States. I first argue that the Chinese judiciary is more consistent with agency models of judicial politics, which holds that lower-level judges have divergent policy preferences from their higher-level principals (Songer, Segal and Cameron 1994; McNollgast 1995). In China's fragmented bureaucratic structure, the judiciary must answer to the territorial party-state (horizontal authority) rather than a higher-level court (vertical authority) (Lieberthal 1992). As a result, while judicial decisions made in the U.S. federal judicial system at different levels are often along racial, partisan, or ideological lines (Songer, Segal and Cameron 1994; Cameron, Segal and Songer 2000; Beim and Kastellec 2014), in a single-party state that does not protect judicial independence, such as China, judges make decisions based on the linkages between the litigants and the particular level of party-state that dictates the judges' well-being. Importantly, Chinese judges' incentive structure is shaped by the tax system of the Chinese local governments. While the Chinese tax system allocates a bigger percentage of private firms' tax payments to the coffers of 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 sources of different opportunities and incentives for judges 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).

Compiling and analyzing a novel dataset of over 4,000 court cases disclosed by publicly traded firms from 1998 to 2013 in China, I test this theory by examining how the win rate

of firms differs across different levels in the judicial system. I report strong evidence that is supportive of the theory. 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.

To accurately estimate the “hierarchy” effect of the judiciary, one has to ask the counterfactual question: How would judges decide differently if the case were adjudicated at a different level? However, this counterfactual does not exist in the real world, and the cases adjudicated at different levels are systematically different, which makes estimating the average treatment effect (ATE) difficult. I take advantage of a natural experiment in which the Chinese Supreme People's Court dramatically lifted the bar for entering higher-level courts in 2008 to estimate the causal effect of judicial level on judicial outcomes. For example, before 2008, economic disputes with claims higher than 6 million *yuan* were under the jurisdiction of intermediate people's courts (cases under 6 million *yuan* were under the jurisdiction of basic people's courts). This cutoff point was raised to 50 million *yuan* in 2008. This jurisdictional change shifted a large number of cases between 6 million and 50 million (treatment group) that should have been adjudicated by intermediate people's courts before 2008 to basic people's courts after 2008. Comparing the win rate of these cases before and after 2008 would give me the local average treatment effect (LATE) of basic people's courts. However, in reality, the cutoff was not strictly enforced: while most cases were adjudicated by the level of courts to which they belong, a small number of cases ended up in the “wrong” courts. Following [Card and Krueger \(1992\)](#), [Lemieux and Card \(2001\)](#), and [Duflo \(2001\)](#), I use a “fuzzy” difference-in-difference (DID) strategy to estimate the causal effect of being adjudicated by basic people's courts on firms' win rates. Specifically, I first use an interaction term between dummy variables indicating the treatment group (cases between 6 million and 50 million) and post-2008 to predict the probability of being adjudicated by basic people's courts, and then I use this probability to predict firms' win rates. Dividing the sample into SOEs and non-SOEs, I report results that are consistent with my theoretical expectations. These results are also robust to a variety of tests, including considering firms' political connections and excluding centrally controlled SOEs.

Another challenge to identifying the causal effect is selection bias: SOEs are more likely to win in intermediate people's courts because they are more likely to go to courts to solve their disputes in the first place (Wang 2015). I follow the empirical strategy in Simmons and Hopkins (2005) to use genetic matching (Diamond and Sekhon 2013) to match cases within the treatment group before and after 2008 on a long list of covariates, and the results are similar to the DID results.

In summary, I find evidence that judicial hierarchy does affect judicial outcomes, and the relationship is complicated, in a fragmented authoritarian regime. The direction of the effect of judicial hierarchy on judicial outcomes depends on the specific linkages between the litigants and the particular level of the party-state. I am not aware of other large-scale quantitative studies on how judicial hierarchy affects judicial outcomes outside the United States. So this article is the first of its kind to systematically investigate judicial outcomes across different judicial levels in a comparative context and in an authoritarian regime. It contributes to the scarce but growing literature on comparative judicial politics (Ferejohn, Rosenbluth and Shipan 2009) and moves an important literature that is originated in American politics to a comparative context.

Fragmented Authoritarianism and the Chinese Judiciary

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 design of the Chinese judiciary, in which a court is the agent of dual principals. The first principal, the vertical one, 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).² For example, the Supreme People's Court cannot independently appoint presidents and other major court officials at the

²The 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).

provincial level, 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.³ 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."⁴ So the system was not designed to "correct errors" as some studies of U.S. judicial politics suggest (Kornhauser 1994; Cameron and Kornhauser 2006), and it does not incentivize judges to take strategic actions in anticipation of sequential actions by higher-level courts. 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 the same 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 highly responsive to those of the corresponding party-state rather 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

³Basic 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.

⁴Author's interview with a judge, March 31, 2010.

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.⁵ In the current tax-sharing system, after the center collects the central tax, most of SOEs' tax goes to the municipal governments (the principal of intermediate people's courts), while most of the privately owned companies' tax belongs to the county-level governments (principal of basic people's courts).⁶

I encountered a case during fieldwork that helps clarify the causal mechanisms.⁷ The case involved firm H in city F. Firm H is an SOE owned and managed by the F city government, and city F is a wealthy city in a southern Chinese province. Firm H 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, "Firm H has a strong voice in this city. To a large extent, it can influence city policies and demand for special treatment, otherwise it can threaten to lay off people or hide revenue."⁸ In 2009, firm H was sued by its stockholders for concealing information, and the case was accepted by the Intermediate People's Court in city F. 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 firm H won, but to discourage the stockholders from appealing, firm H 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 a government official or an influential firm is involved, and 2) if the case outcome can impact

⁵For details of the tax-sharing system, please see [Wong and Bird \(2008\)](#).

⁶There is no uniform formula for sharing across localities, but a 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). For a concrete example in Fuyang City, please see http://www.ahfzb.gov.cn/content/govfile_view.php?id=1138&ty=2 (Accessed February 16, 2015).

⁷The 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.

⁸Author's interview with a lawyer, March 29, 2010.

social stability.”⁹ 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.’”¹⁰

The case illustrates that tax incentives are a major driving force for 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: Judicial hierarchy has a conditional effect on judicial outcomes in China: 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 estimating the effect of judicial hierarchy on judicial outcomes 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 has been available. Most existing 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 2008; Ang and Jia 2014). Very few empirical studies have been able to systematically examine court outcomes, especially at the local level.¹¹ 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 certain level of court to obtain a more

⁹Author’s interview with a lawyer, March 29, 2010.

¹⁰Author’s interview with a judge, April 8, 2010.

¹¹One study that uses actual court outcomes is Helmke (2002), but it is at the top level.

desirable outcome, which could produce selection bias.

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).¹² 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. The CLFLD is then merged 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.¹³ This article is the first empirical study to use CLFLD to examine judicial hierarchy and judicial outcomes.¹⁴ Table 1.1 in the web appendix presents the descriptive statistics of the dataset.

Among the 4,275 court cases, 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%. In addition, 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 6 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

¹²Every case is double-coded by me and a group of trained research assistants.

¹³I 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).

¹⁴Another study that uses data from the same source is Lu, Pan and Zhang (2015), but is for a different research question.

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 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 under 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 levels of courts. For example, economic disputes between 6 million and 50 million that should 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. In addition, the jurisdictional change also created control groups. For example, disputes between 50 million and 100 million 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, the exposure of a case to the treatment (adjudication by a basic people's court) was determined by both the claim of the case 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 were adjudicated by basic people's courts, this percentage increases to 53.71% in Cell 4. Meantime, 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 what happens in the control group, in which there is only a -3.26% change. This DID is also statistically significant in a regression framework with year, province, and industry fixed effects, which is presented in Section II in the web appendix. Similarly, within the same range of claims, the win rates of SOEs and non-SOEs also experienced significant changes after 2008.¹⁵ While the 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 lead to more convincing results.

[INSERT TABLE 1 HERE]

An alternative identification strategy is a regression discontinuity (RD) design to compare cases just above and 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

¹⁵The ownership data are from CSMAR and the Chinese Economic Census 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, the no-manipulation assumption of the RD design is violated. 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, and I hence do not use an RD design.¹⁶

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 intention-to-treat analysis (Dunning 2012, 138) to estimate the DID in Treatment Group I versus Control Group I. The intention-to-treat analysis focuses on the groups created by the randomization designed by the 2008 jurisdictional change, although in reality the randomization was not strictly enforced. As Dunning (2012, 138) argues, intention-to-treat analysis may lead to inaccurate estimates of the effect of 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:

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

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

¹⁶For results of the McCrary density test, please see Section III in the web appendix.

using SOEs and $\gamma_3 > 0$ for regressions 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 opposed to loan cases, tort cases, and 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 the 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 location and courts’ location. Because two firms are involved in a dispute, there are four 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)); 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.¹⁷ 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]

¹⁷I present the results using cases in Treatment Group II or Control Group II in Section IV in the web appendix.

For SOEs, being in Treatment Group I and accepted after 2008, which increased the exposure of a case 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 do not find any “home-field advantage” for SOEs; they have roughly the same win rates in 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. This contrasts non-SOEs with SOEs: the former usually only have local ties that do not cross boundaries.

The intention-to-treat analysis results are largely consistent with Hypothesis 1 that 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 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, court burden) or simply mistakes. To more accurately estimate the LATE, I use a *fuzzy* DID 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 its probability of acceptance by a basic people's court, and then I can use this probability to predict its probability of winning. 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 instrumental variables estimates 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 and control at random or at least as-if random. One scenario that could potentially violate this assumption is when 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 could 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 decided to wait until after 2008 to pursue the case. Or it could also be that firms might choose to file under the new 2008 system when previously they would not have or vice versa.¹⁸

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 as a direct response to the passing of the new Civil Procedural Law, which became effective on April 1, 2008. The new Civil Procedure 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 on April 1, 2008, all appeals were required to be filed to 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 made to alleviate the burden of higher-level courts.¹⁹ Second,

¹⁸I thank an anonymous reviewer for pointing out this possibility.

¹⁹For more details, please see http://news.xinhuanet.com/legal/2008-01/28/content_

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 was happening. 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, a fuzzy or IV DID 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 2SLS to fit the following equation:

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

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 regressions 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 proba-

7509567.htm (Accessed February 19, 2015).

bility 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. Non-SOEs 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.

In sum, both the DID results and the 2SLS results support Hypothesis 1 that judicial hierarchy in China has a conditional effect on firm litigations: 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.²⁰ 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

²⁰For some of 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 win rate of firms in basic people's courts and intermediate people's courts. I assume that the 2008 jurisdictional change assigned cases to treatment and control 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).²¹ 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 2. 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, which are nontrivial.

Second, some recent studies have pointed to the importance of firms' political connections in determining their preference for litigation or litigation outcomes (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 of the board members (chairperson, president, vice-president, CEO, executive director, non-executive director, or secretary) in all of the involved companies from Wind Info, a leading integrated service provider of financial data based in Shanghai.²² I then manually coded the career information of each board member in each firm to determine whether a member is politically connected.²³

²¹My results indicate dramatic improvements in balance (Section VI in the web appendix).

²²<http://www.wind.com.cn/En/> (Accessed August 2, 2013).

²³Every 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.

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 the parliament; 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’ different preferences at different levels are shaped by local governments’ tax incentives, then I should exclude central SOEs from my analysis. I hence exclude a 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

As Ginsburg and Moustafa (2008) contend, “Scholars have generally assumed that courts in authoritarian states are pawns of their regimes ... nearly all studies in comparative judicial politics have focused on democratic and democratizing countries.” With the recent rise in scholarly interest in political institutions in authoritarian regimes (Gandhi 2008; Svolik 2012), scholars have started to examine the conditions under which autocrats support the “rule of law” (Helmke 2002; Moustafa 2007; Wang 2015). While there is a long tradition in American politics for the judiciary to be an important policy maker (Dahl 1957), we know little about what determines judges’ policy preferences in authoritarian regimes.

I present the first large-scale, empirical study of how judges at different judicial levels have different policy preferences using a novel dataset of real court cases disclosed by Chinese

publicly traded firms from 1998 to 2013. I argue that the fragmented nature of the Chinese bureaucracy creates a “hierarchy” effect in the judicial system, and 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 threshold for cases to enter higher-level courts, which created exogenous variation in cases’ exposure to various levels of courts, this study uses an IV-DID 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 correction of selection bias using genetic matching, considering firms’ political connections, and excluding centrally controlled SOEs.

This study makes a couple of notable contributions. First, it provides the first quasi-experimental evidence that Chinese judges do in fact rule in a biased fashion over business disputes, protecting firms that local party authorities want to protect. This is qualified or nuanced, however, by the finding that local authorities protect firms not solely for being local, but only if these firms also contributed revenues to the local coffers.

Second, it also confirms and quantifies the widespread sense that central control over local governments has important limits. I echo a recent study by [Lorentzen, Landry and Yasuda \(2014\)](#) on environmental transparency, which also finds that local governments try to protect large local businesses based in their jurisdictions.

These points lead to an important policy implication. Recently, many authoritarian regimes have carried out judicial reforms to promote the “rule of law.” An important component of these reforms is to centralize the judiciary to avoid “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.²⁴ In the same vein, the Chinese Communist Party announced a “rule-of-law” reform package during the Fourth Plenary Session of the 18th Party Congress in 2014 to centralize the judicial

²⁴Please see <https://www.bhba.org/index.php/component/content/article/251> (Accessed February 23, 2015).

system.²⁵ These reforms have received financial support from international donors, such as the U.S. Agency for International Development and the Ford Foundation.²⁶ However, as this study shows, higher-level courts do not necessarily bring fairer judicial outcomes; without fundamental changes to the political structure, centralizing the judicial management in higher-level courts would only transform “local bias” into “higher-level bias” that favors litigants with connections to higher-level governments.

²⁵Please see <http://thediplomat.com/2014/10/4th-plenum-rule-of-law-with-chinese-characteri> (Accessed February 23, 2015).

²⁶Please see <https://www.bhba.org/index.php/component/content/article/251> (Accessed February 23, 2015).

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Table 1: Means of Court Level and Win Rate 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 Census 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 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) and control (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.

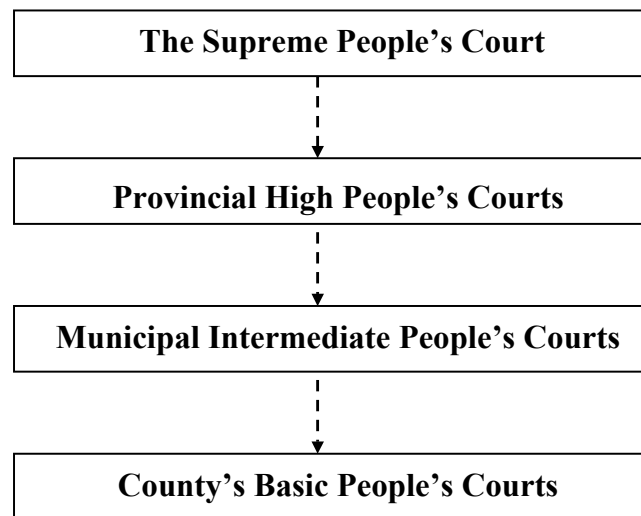


Figure 1: Hierarchy of China's Judiciary System

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

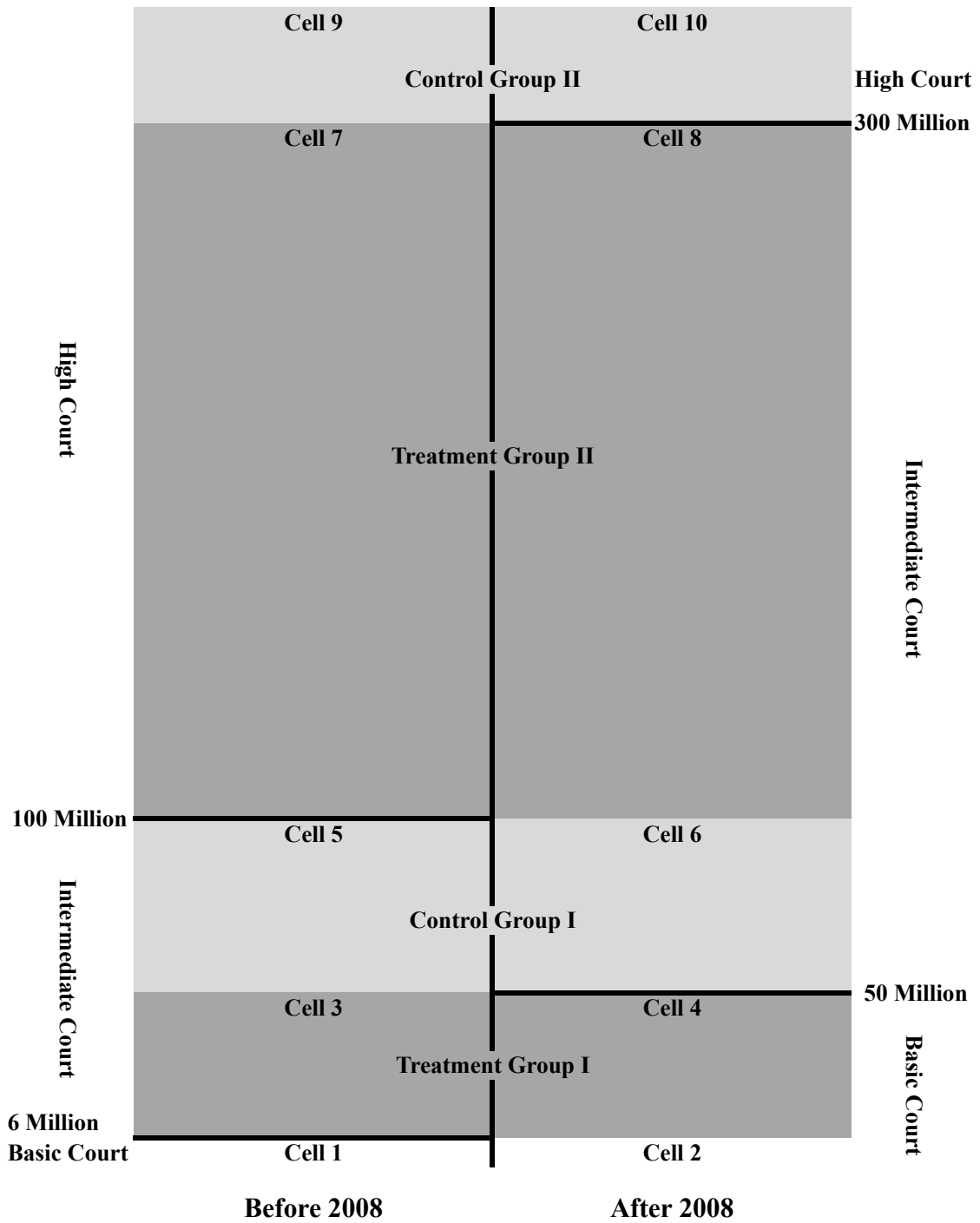


Figure 2: Court’s Jurisdictional Changes in Guangzhou City in Guangdong Province

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.

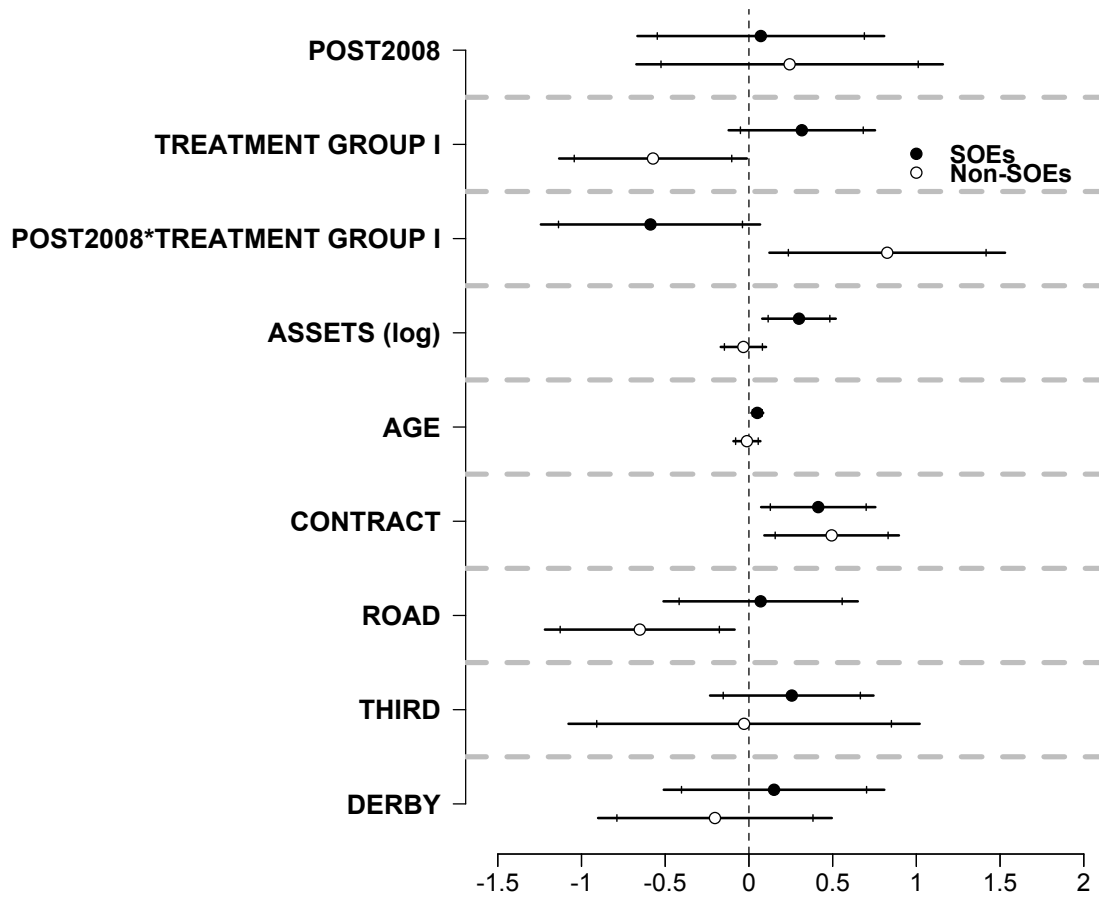


Figure 3: Effect of Exposure to Basic People's Courts on Win Rate: DID Estimates

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 shares 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.

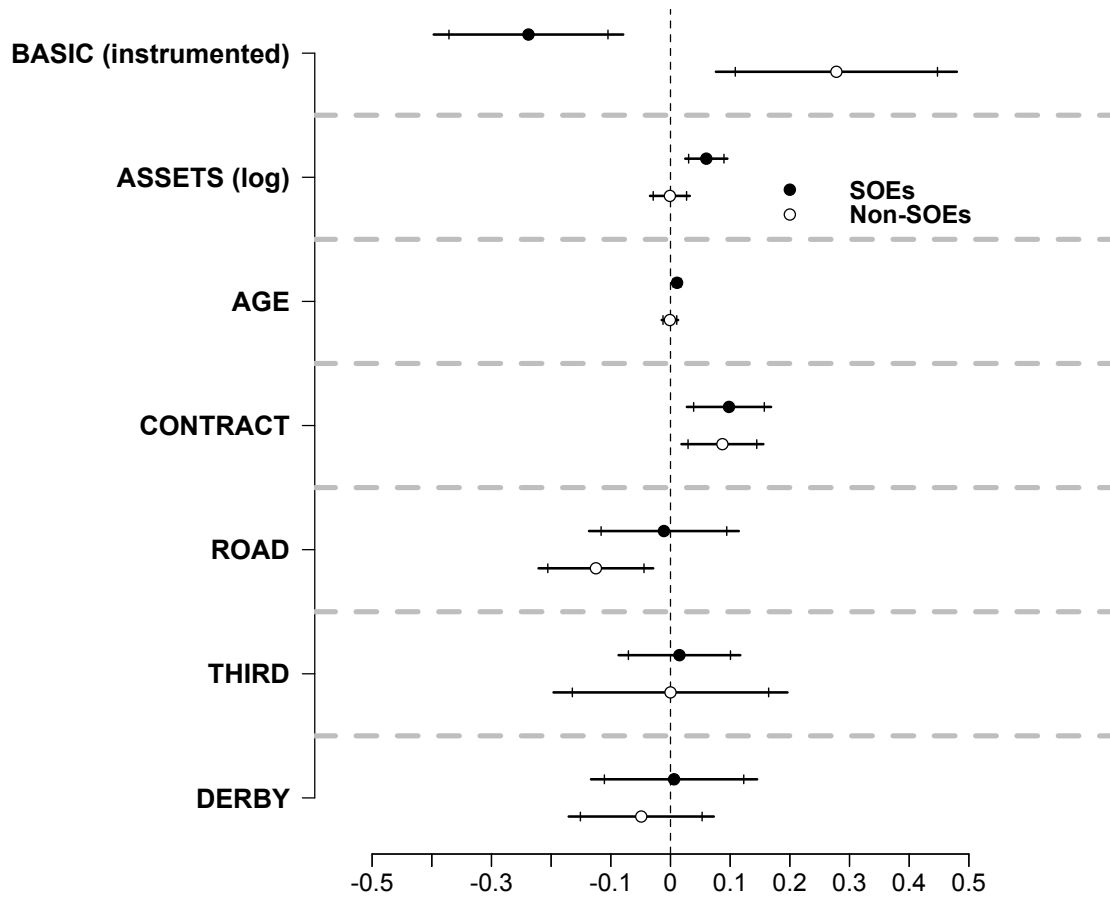


Figure 4: Effect of Exposure to Basic People's Courts on Win Rate: 2SLS Estimates

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. *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 shares 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.