Political Connections and Judicial Bias: Evidence from Chinese Corporate Litigations*

Haitian Lu
The Polytechnic University
Hong Kong

Hongbo Pan
Wuhan University
China

Chenying Zhang†
The Wharton School
University of Pennsylvania

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Abstract

Using hand-collected data from 3,323 court rulings on Chinese listed firms during 1998-2010, we investigate how political connections affect litigation outcomes. Connected firms have a win rate that is 8.6% higher than unconnected firms have. The higher win rate is most significant in cases with straightforward facts, in provinces where the local legal institutions are weak, and in cases tried in politically-connected firms' home provinces. The empirical evidence is consistent with the hypothesis that the difference in the win rates is caused by judicial bias, but not caused by information asymmetry about case merits. We show that trial outcomes have real wealth impacts on firms' shareholder values. Winning firms earn 5-day cumulative abnormal returns around the verdict announcements that are 50 basis points higher than losing firms earn.

Keywords: politically connected firms, corporate litigation, judicial bias, shareholder wealth, China

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†Corresponding author: Finance Department, the Wharton School of Finance, University of Pennsylvania, 3620 Locust Walk, Philadelphia, PA 19104. Phone: 617-412-0955, E-mail: chezhang@wharton.upenn.edu.

I. Introduction

There has been increasing economic interest in the significance of political connections in corporations, particularly in the context of emerging markets (Fisman 2001, Faccio 2006, Khwaja and Mian 2005, Fan et al. 2007). However, little of the work has paid attention to the relationships between a firm's political ties, its decision to seek protection from the judiciary, and litigation outcomes. There are a number of reasons why trial outcomes matter for corporations. In a market economy, courts serve as an important protective mechanism for entrepreneurs to secure property rights and enforce contracts (McMillan and Woodruff 1999, Frye and Zhuravskaia 2000). Litigation also has direct impacts on firms' shareholder wealth. Both Bhagat et al. (1993) and Firth et al. (2010) concluded that defendant firms suffer losses upon litigation announcements due to the potential of financial distress. Litigation is thus a direct yet undocumented channel in the literature through which political connections may affect firm values.

In this paper, we investigate how political connections affect trial outcomes based on hand-collected data from 3,323 court rulings that include all litigations involving Chinese listed companies during 1998-2010. More than 50% of our cases are loan related, making our findings directly relevant to firms' financing decisions. Taking state ownership as a natural form of political connection, we find that listed state-owned enterprises (SOEs)[1] (either as plaintiffs or as defendants) win 8.6% more often at trial than non-SOEs. Using the personal ties of the top managers in the non-SOEs as a second proxy for political connections, we show that connected non-SOEs fare better than the unconnected ones in court rulings by 8.9%. However, personal political ties do not serve as a perfect substitute for state ownership. We find that the connected non-SOEs are still at a disadvantage compared to the SOEs.

[1] As will be explained later, here we define SOE as a firm with the government as its ultimate shareholder. These SOEs are publically listed companies whose stocks can be traded .

If the politically connected firms have a higher win rate, then a potentially more important question is through what channel do the political connections take effect. There are two possible explanations: (1) connected firms are better able to acquire information about the intrinsic merits of the case, which enables them to bring stronger cases to trial or (2) connected parties play a direct role in setting the decision standard of the court. In the latter case, the judge may overlook the case facts to rule in favor of the connected party, resulting in what we define as judicial bias. The term *judicial bias* is used loosely here to refer to the judge exerting varying levels of discretion over a case verdict that is not solely based on merits, and does not necessarily indicate any unlawful activity. Nevertheless, political connections undermine the base of the judicial dispute resolution in this situation, for the judge is no longer impartial.

Empirically, it is hard to disentangle these two explanations since we cannot observe all of the characteristics of a case. This paper represents a first attempt in the literature to distinguish between those two possibilities. First, we argue that if the connected firms win more often due to better information about case merits, their advantages should diminish among cases with straightforward merits because it is equally easy for both the connected and unconnected firms to discover the facts in those cases. However, using the types of suits[2] as a proxy for the straightforwardness of case facts, we show that the connected firms win most often in cases with simple facts, suggesting the influence of judicial bias but not information asymmetry about merits.

Next, we find that better legal environments in a province lead to a lower win rate of connected firms. We use whether a Chinese province was opened as a leased territory or treaty port to foreign countries in the late Qing dynasty as an exogenous proxy for better local legal environments to address the reverse causality concern. Since the leased territories and treaty ports were set up more than a hundred years ago, their establishment should not affect a judge's ruling

[2]I.e., loan suits, sales and purchase contract suits, tort suits, and others.

today. However, the establishment of the leased territories and treaty ports is likely to have a long-term positive impact on the local legal institution development by introducing the Western-style laws at an early stage. Similarly, using the exogenous local governor turnovers caused by circumstances such as sudden death as a proxy for periods of weakened local political connections, we show that weaker connections also lower the win rate for connected firms. Moreover, the win rate of locally connected firms is higher when the case is tried in their home province. These findings suggest that the higher win rate of connected firms can be attributed to biased courts.

The higher win rate of connected firms has a real impact on shareholder wealth. Using an event study, we find that a winning firm has a five-day average market-adjusted cumulative abnormal return that is 50 basis points higher than that of a losing firm. Because an adverse verdict is often associated with future financial losses, the markets react upon receiving the news.

We see three contributions of this research. First, our work belongs to an increasing volume of literature on the impact of political connections on firm performance. It has been documented that corporations enjoy various benefits associated with political connections, including favorable regulatory conditions (Agrawal and Knoeber 2001, Morck et al. 2005) and access to resources such as bank loans (Khwaja and Mian 2005, Faccio 2006), which ultimately increase firm values (Roberts 1990, Fisman 2001, Claessens et al. 2008, Johnson and Mitton 2003). On the other hand, Fan et al. (2007), Yuan (2008) and Boubakri et al. (2008) found that political connectedness may destroy firm values. However, to the best of our knowledge, no prior study has demonstrated direct evidence of how political connections play a role in court decisions; nor have we seen a connection between litigation outcomes and shareholder wealth. This paper adds to the literature by offering a missing channel through which political connections can increase firm values.

Second, our study adds new evidence on formal and informal institutions that secure

property and contractual rights. It draws from the emerging law and finance literature on the role of political connections in a transitional economy (La Porta et al. 1997, Allen et al. 2005, Fan et al. 2007). In countries with fewer constraints on politicians and elites, the government is more likely to violate the property rights of private producers and seek benefits for the interest groups (Acemoglu et al. 2005). Political ties then become necessary for companies to run businesses when they cannot rely on the legal system to secure property rights (Li et al. 2008). This paper provides evidence that though SOEs receive favorable rulings in court, the judicial bias against non-SOEs can be partially corrected by the personal political ties of their top managers.

Our work also extends the large body of literature on the economic analysis of litigation behavior by incorporating the often-neglected judicial bias factor to the well-cited Priest/Klein framework (Priest and Klein 1984), which assumes that the decision between settlement and litigation is solely based on information asymmetry about case merits. In the Priest/Klein model, it is suggested that two parties take a case to court because they have divergent information on case merits. Where parties are symmetrically informed about merits, they tend to settle instead of litigate. Built on this hypothesis, Hylton (1993, 2002) argued that when parties are not symmetrically informed about the case merits, the party with informational advantage will have a more precise estimate about the likelihood of success at trial. Consequently a higher-than-50% win rate should be observed for the party with an informational advantage if the dispute finally goes to trial[3]. Our paper builds on this literature by analyzing litigation outcomes in a large, emerging market, proposing that the determinants of court outcomes should not be confined only to the parties' respective perceptions of the case merits, but also incorporate at least their prediction on the direction and extent of judicial bias. We present empirical evidence that judicial bias alone leads to a higher win rate of the favored party.

[3]Empirical evidence on this is mixed. Kessler et al. (1996) gave a review of the findings from the U.S. courts. Evidence outside the U.S. has been limited (Ramseyer and Nakazato 1989).

Though we use China as a case study in this paper, our findings are relevant to other countries, especially those with a socialist and civil law origin. The proposed tests to distinguish judicial bias from information asymmetry about case merits can be readily applied under other legal systems. Finally, the judicial bias we document imposes an additional litigation risk on the multinational companies participating in the Chinese market, which are of increasing importance as globalization accelerates.

The rest of the paper is organized as follows: Section II provides the institutional background of the legal reforms in China. Section III presents the data. Section IV outlines the empirical methodology and displays the results. Section V further supports our results with robustness checks, and Section VI concludes.

II. Legal Reform, Political Ties and Judicial Bias: A Review of China

The Chinese legal reforms have been the subject of intense scholarly interest in the West. Existing legal studies have mainly covered the administrative cases (Pei 1997) and economics cases (He 2007), most of which focused on historical reviews of the evolution of the related law and its implementation. Quantitative evidence remains scarce. The reforms started in 1978 when Deng Xiaoping emerged as the *de facto* political leader of China following the death of Mao Zedong in 1976. The role of the legal system at first was to bring order and stability to political and social life after the chaos of the Cultural Revolution. Since then, China's phenomenal economic development and corresponding rapid social changes have dramatically increased pressures on courts to cope with the problems that other government agencies have failed to resolve. Legal reform became a government priority in the 1990s as a result of the increasing global exposure. To provide a trust worthy legal environment for the incoming foreign

investments, the government has devoted enormous resources to revamp its legal institutions, putting major efforts in the rationalization and strengthening of the legal structure.

After the Chinese Communist Party (CCP) decided at the Fifteenth Party Congress to "promote judicial reform" in 1999, the Supreme People's Court (SPC) announced a five-year reform plan to build a "fair, open, highly effective, honest, and well-functioning" judicial system. "Fairness" was highlighted as the "essence" of judicial reform and has been the central theme since then. The SPC completed the second five-year plan between 2004 and 2008. During that time, documents were issued by the SPC demonstrating a cautious awareness of the importance of bringing greater professionalism, independence, and integrity to the judiciary.

Improvements resulting from the legal reform are obvious. New Western-style laws were introduced, and existing laws were amended for more comprehensive and fair coverage. For example, the 1994 Administrative Procedure Law was introduced to allow citizens to sue officials for abuse of authority or malfeasance. The trademark law has been modified and used more extensively as a result of increasing concerns over violations of intellectual property rights of foreign corporations in the early 1990s. In late 2005 a largely rewritten Company Law was adopted, radically increasing the role of courts. A new Enterprise Bankruptcy Law was promulgated in 2006, which in many aspects resembles the modern bankruptcy law in developed countries. As of 2008, China has roughly 200,000 judges, 160,000 procurators (prosecutors), and 150,000 lawyers. Over 600 law departments and law schools send out several hundred thousand graduates[4] every year. There is a development of a legal services market as well. Foreign lawyers have accompanied foreign capital and their clients to China, which has had an immense influence on the promulgation of new Chinese laws, especially in regard to intellectual property, and corporate and securities laws.

[4] Thirty Years of Chinese Legal Reform, *The Wall Street Journal, Dec.* 4th, 2008.

The reform also has awakened citizen's inherent demand for court services. This change in China can be described as a transformation from a former "acquaintance society" (Fei 1948) to an arm's length one. In an acquaintance society, the courts play a less important role as networks and reputations play a dominant role in directing economic activities. However, the use of courts as a forum for dispute settlement increases as a result of the prevalence of impersonal and contractual relations (Vago 2006). Figure 1 shows the number of civil, criminal and administrative cases filed in China has been increasing from 1990 to 2008 on a per million population basis.

Despite the growing demand for court services, court impartiality is still a primary concern of the public, especially when citizens are acting against the government or its affiliated enterprises (Chen 1995). Lubman (1999) indicated that the laws and court systems in China still serve more as a top-down instrument of Party control than as a framework to facilitate private transactions. Howson (2010) reviewed more than 1000 Company Law-related disputes between 1992 and 2008 in Shanghai and concluded that there is significant momentum toward the competence and autonomy of the People's Courts. However, the path toward autonomy is inconsistent; sometimes a development is followed by setbacks. As of today, litigation is still hampered by local governments and judicial corruption[5]. It is not clear whether the legal system has achieved its goal of fairness at the completion of the second five-year program.

III Data Description

III.1 Variable of Interests

The obligation of Chinese listed companies to disclose their involvements in the lawsuits and arbitrations is stipulated in Chapter 11.1 of the Listing Rule of the Shanghai and Shenzhen

[5] In March 2004, the Procurator-General Jia Chunwang admitted, "the procurators at all levels had not done enough to check the problems of unfairness in the implementation of laws " (Firth et al. 2010).

Stock Exchange, respectively[6]. The WIND database, a leading Bloomberg-style data provider in China, collects information on all Chinese listed firms that have reported their involvements in the lawsuits, either as plaintiffs or as defendants, by reproducing the original unprocessed texts from the companies' disclosure reports. We read through all of the case reports and hand-code useful information such as the nature of the disputes (type of suit), the parties in question, the claimed stake, the trial outcomes, the level of the courts, and others. Given that a large proportion of the appealed cases do not have information on final rulings, We only consider the verdicts from the first rulings[7]. Our final sample consists of 4,089 cases filed by listed firms between 1998 and 2010[8].

Another variable of major interest in this study is the political connection status of a company. Previous literature has proposed different measures for connections, including the chief executive officer (CEO)'s contribution in an election (Khwaja and Mian 2005, Claessens et al. 2008), firms' affiliations to large business groups (Fisman 2001), and whether the board has current/past politicians as members (Faccio 2006, Fan et al. 2007, Boubakri et al. 2008, Li et al. 2008). In China's case, one analogous aspect to consider is whether the firm has the government as its controlling shareholder. State ownership creates a natural connection with the government for the company and provides benefits such as immunity from bankruptcy. The heads of the SOEs are often important members in the communist party, which characterizes them as politicians. Though the privilege of SOEs may have been restricted due to a series of financial and legal

[6] According to the Listing Rule, a company must disclose its involvement in litigation/arbitration if the litigation/arbitration stake (of a single case or accumulative cases within 12 month) is over RMB 10 million ($1.54 million) and over 10% of the company's net assets, based on the company's last audited report. For litigations/arbitrations whose stake amount below the above threshold, the Board should also disclose if in their opinion such case would have a significant impact on the company's securities. See Chapter 11.1 of Listing Rule of the Shanghai Stock Exchange (1998) and Listing Rule of the Shenzhen Stock Exchange (1998).

[7] In China, the success rate of appeal is extremely low. The lower level of courts tend to report and "seek opinion" from their upper courts in making decisions in the first instance, especially when the stake of a case is significant. Therefore, even if the case is appealed, the upper court will generally not alter the decision of the lower court.

[8] We choose 1998 as the starting year because this is when the listing rules requiring mandatory disclosure of litigations were promulgated, both on the Shanghai and Shenzhen stock exchanges.

system reforms in China, anecdotal evidence suggests that SOEs enjoy advantages over non-SOEs when dealing with the government[9]. For our purposes, we define a listed company to be an SOE if the ultimate holder of the company is the local (at least at a city level) or central government as recorded in CSMAR, another leading Chinese data provider.

For non-SOEs, closer bonds to the authority may be established by hiring CEOs or directors who formerly held positions in the local or central governments (See Calomiris, Fisman, and Wang 2009 and Fan et al. 2007 for documentations on politically connected CEOs). We thus argue that for non-SOEs, CEO or directors' personal ties with the government can serve as an alternative measure for the firm's political connection.

To test this, we collect data on CEO or directors' previous employment histories of the non-SOEs. We consider a non-SOE as politically connected if the company's CEO or director is or was a government official (at least a leading official of a division, i.e., Ke Zhang) or a leader of the People's Congress, or the People's Political Consultative at either the national or regional level. We first use the firm's annual reports to identify its top managers, and then we refer to the WIND database, which has some records on whether the top manager of a listed firm has held positions in the government or in the communist party. For those CEOs/directors whose information is missing, we search on internet. If there is no evidence suggesting that the CEO/director was previously connected to the government, we then conclude that the CEO/director is not politically connected. Sometimes, especially for CEOs/directors who are recently appointed, the information is harder to trace because they tend to hide their previous relationship with the government to avoid undesired publicity. Under other circumstances, government officials may not sit on the board, but instead would have someone act on their behalf.

[9] For instance, in 2011, China started a reform in the steel industry with the target to "increase the global competitiveness of the steel industry." The reform plans to shutdown less efficient steel productions to solve the long standing problem of excess production capacity in China. In reality, however, the reform simply leads to massive acquisitions of non-state-owned steel companies by large SOEs such as Bao Gang and An Gang. The small scale non-SOEs, which may not necessarily be less efficient, have virtually no other choice but being acquired by a large SOE.

We are aware of the potential selection bias here: it is possible that some CEOs/directors are actually connected but successfully hid the information from the public. However this bias makes it harder for us to detect whether the politically connected CEOs have a positive impact on the firm's win rate. If we can correct for the bias, our results will only be stronger.

Finally we collect financial and stock data for each company from CSMAR and WIND, and match the financial data at the end of the last year to the cases that are tried in this year. Since a majority of the counterparties in the suits are not listed, their financial information cannot be retrieved. We do, however, include a variable for the ownership statuses of the counterparties wherever available, which we obtain from the internet. Since the status of political connection of a firm is the core of our paper, not controlling the counterparty's state ownership status would be an important miss. We further exclude the following five types of cases from our sample: (1) cases which were not tried in the Chinese courts, including cases heard by foreign courts and arbitration, (2) non-civil cases, including criminal and administrative cases, (3) cases which were withdrawn by the plaintiffs in the first trial, (4) cases which were settled during the first trial, and (5) cases for which court judgments were not disclosed.

Matching the litigations, political connections and financial data reduces our final sample size to a total of 3,323 cases, including 2,004, or 60% cases involving SOEs. Our sample has 714 distinct firms, with 502, or 70% SOEs. Many firms are repeated players in court, generally for similar reasons, such as loan disputes. Banks, in particular, may repeatedly sue other firms for over-due loans. We control for this factor in our later regression. In terms of the geographical distribution, the cases are widespread across the regions. Guangdong and Shanghai are the two provinces with the largest total number of cases between 1998 and 2010, while Shanghai and Hainan have the highest litigation rate on a per million person basis.

Panel A of Table 1 gives a summary of our final sample on the number of litigations each year, classified by suit types. We divide the suits into four types: (1) bank loans, (2) non-bank loans (3) sale/purchase and other contracts, (4) right infringement and other tort cases[10]. Cases related to loan and debt payment account for the majority of the litigations, but we see a variety of types of suits.

The number of tried cases reached its peak in 2005, and then dropped to a low level in 2010. This can be attributed to the banking reform propelled by the Chinese government in 2004, in which the big state banks launched their IPOs. The banks must write off the non-performing loans on their balance sheets to meet the listing criteria, leading to an increased number of the loan suits. Since 2007, the government started implementing several reforms on the financial market, including the stock reform that completes the conversion of all the non-floating shares to floating ones, and a new accounting standard that is enforced on listed firms. The number of litigations drops during the transition period. Moreover, the Chinese government has been actively advocating the idea of building a "harmonious society" since 2005 under the Hu Jintao administration. The ideology pursues a society with balance and harmony, resulting in a significant drop in the number of litigations after 2005.

Panel B of Table 1 shows the distribution of cases, classified by state ownership status. The SOEs tend to be plaintiffs more often, and non-SOEs are more likely to be involved in the loan suits as defendants. We see good presences of both SOEs and non-SOEs in each suit type.

III.2 Control variables

[10]A loan case here does not necessarily involve only the lender and the borrower. Disputes between a loan guarantor company and the borrower are also categorized under type 1 or 2. Type 4 includes torts such as civil tort on false statements in the securities market, disputes over trust management contract, and assets transfer and product liability.

Our choice of control variables follows the literature convention. The control variables include the firm size, leverage ratio, cash-to-asset ratio, profitability as measured by operating profit (EBIT), whether the disclosing firm was the plaintiff, whether the disclosing firm was involved in more than four other litigations in our sample, whether the case was tried at a higher level court, and the disputable amount.

Larger firms may have more abundant resources, such as better legal staff to help them win the case. Leverage ratio and cash ratio are used as proxies for the firms' solvency. Cutler and Summer (1988) and Bhagat (1994) both concluded that the risk of financial distress may be exacerbated around the time of litigation. Profitability is controlled because the court may favor firms that make pivotal contributions to the regional economy. A plaintiff dummy is included because previous literature (Klein and Priest 1994, Hylton 1993, 2002) indicated that the plaintiff usually has an information advantage in case merits, which leads to a higher probability of winning. We control for whether the firms are repeated players in court because we want to make sure that our result is not driven by the firms' familiarity with the legal procedure. The choice of four repetitions is somewhat arbitrary. Using ten as the threshold does not change our results.

A variable for the higher level court is included because the court level is associated with unobserved case characteristics. For a similar reason, the disputable amount of a case is included. Under the Chinese law, cases involving high monetary damages, or cases deemed as influential or complicated are stipulated to be tried at a higher level court. A case can be considered "complicated" for many reasons, such as the involvement of a sensitive industry or firms located in multiple cities. We also include the ownership status of the counterparty in the litigation. If there is judicial bias, then the disclosing firms are more likely to win if they face a non-SOE. To control for the regional development, we include the fixed effect for provinces where the trial takes place. Finally, we control for the fixed effects for industry, year, and suit types.

Table 2 shows summary statistics of SOEs and non-SOEs in the first two columns. Our sample consists of more SOEs than non-SOEs. Consistent with the conventional belief, SOEs are of slightly larger size, but the difference in size is not significant. The average firm size as measured by book asset is 152 million USD in our sample, which is also the average size of listed firms in China during the sample period. Not surprisingly, SOEs have a higher win rate, and are more likely to be plaintiffs. They are also more likely to be repeated players in courts, probably due to their comfort with the legal system. Non-SOEs have higher leverage, lower cash-to-asset ratios, and higher profits. Finally, SOEs are more likely to face a non-SOE counterparty in the suit, and are slightly more likely to have their cases tried at a higher level court with larger disputable amount. Neither of the above two discrepancies is statistically significant.

The last two columns of Table 3 divide the non-SOE subsample to firms with and without politically connected CEOs/directors. Less than 20% of non-SOEs do not have a connected CEO/director. The proportion of connected CEOs/directors in our sample is higher than that reported in Fan et al. (2007). The discrepancy can be explained by a different sample period and different set of firms covered by the study. Moreover, Fan et al. (2007) only considered the political connection of the CEOs, while our data include the directors as well. In our sample, the unconnected non-SOEs are comparable to the connected ones in most financial measures, except that they have higher leverage ratios. Firms with politically connected CEOs/directors have a higher win rate, and are more likely to be repeated players. However the average win rate of non-SOEs with politically connected CEOs/directors is still lower than the average win rate of SOEs.

IV. Empirical Results

In this section, we first use the state ownership as a proxy for political connections to show that connected firms have higher win rates than unconnected ones. We apply several tests to draw

the conclusion that the difference in the win rates is driven by court's political bias rather than parties' information asymmetry about case merits. We find that the advantage of the connected firms diminishes if the case is tried in provinces with better local legal environments. The local SOEs owned by the provincial governments receive additional benefits in court if they have the cases tried locally, and suffer a drop in the win rate if the cases are tried during periods of weak local connections. Using the subsample of non-SOEs, we then illustrate that our results hold when the personal political ties of CEOs/top directors are used as an alternative measure of political connections. However, the personal connections of CEO/director cannot serve as a perfect substitute for state ownership. Finally, we demonstrate that winning firms enjoy higher cumulative abnormal stock returns than losing firms when the verdict is announced.

IV.1 Judicial Bias and Case Merits

This subsection uses state ownership as a proxy for political connection. We first examine whether political connections are associated with higher win rates, using state ownership as a proxy for political connections. We regress the trial outcome on the ownership status of the firm. Though the dependent variable is binomial, we choose a linear model over a logit model, because the linear model is unbiased and imposes much fewer restrictions on the data structure. More importantly, a linear model enables us to get a clear interpretation of the coefficients of interaction terms, while a logit model would not allow us to measure the average marginal effect of a variable in the interaction term (Norton et al. 2004).

Our base line regression is:

$$Win_i = \alpha + \beta_1 SOE_i + \beta' Controls_i + \varepsilon_i \qquad (1)$$

where *i* is the unique case id number. ε is the noise term estimated using clustered standard error at a province level. *Win* is a dummy variable that equals one when the disclosing firm wins. We

define plaintiff winning as the plaintiff firm getting the full or partial amount of the compensation it requests[11]. *SOE* is a dummy variable that equals one when the firm's ultimate owner is the government. The control variables have been discussed in the previous section. The coefficient β_1 measures the average difference in the win rates between the SOEs and the non-SOEs.

Table 3, Panel A presents the regression result in the first column under Model 1. The main finding is that SOEs have a win rate that is 8.6% higher than non-SOEs, confirming our conjecture that SOEs enjoy a higher probability of winning in courts than non-SOEs.

The results on control variables are mostly in line with our expectations. Larger firms and plaintiff firms are more likely to win. Leverage ratio enters insignificantly. Firms with a high cash-to-asset ratio or profitability have a higher winning probability, since the court may want to favor the firms which make significant contributions to the regional economy. Interestingly, repeated players have a lower probability of winning, for they may bring weaker case to court due to their comfort with the legal system or their over-confidence in favorable trial results. Whether a case was appealed, the court level, and the disputable amount have no impact on the win rate. A final important observation is that the disclosing firm is more likely to win if the counterparty is a non-SOE, further confirming the claim that non-SOEs are at disadvantage in court.

Having established that SOEs win more often, we need further evidence that the higher win rate of SOEs are pursuant to the political preference of courts. The major challenge is to distinguish the claim of judicial bias from the alternative explanation that the SOEs bring stronger cases to court. When a firm is faced with a potential dispute as a plaintiff or as a defendant, it has the choice to settle. If the SOEs can choose the best cases based on case merits to take to court and settle the rest while the non-SOEs cannot, then the SOEs will have a higher win rate in the absence of the judicial bias. Indeed, Panel B of Table 1 shows that the distribution of suit types is

[11] We define defendants winning when the plaintiff loses the case. There are very few partial compensation cases.

different across SOEs and non-SOEs, implying that the choice of tried cases is not random. An ideal way to deal with that is controlling for every aspect of case characteristics, which are often unobservable. Hence, we propose an alternative test to distinguish the story of judicial bias from the explanation that the SOEs are more capable of spotting the stronger cases.

Specifically, we investigate judicial bias on cases with different levels of potential information asymmetry about merits. A case is only taken to trial if the two conflicting parties have a big enough divergence in the expectations on the trial outcome. Without judicial bias, the divergence in expectation stems from information asymmetry about the case merits between the two parties. Namely, the two parties possess different information or different interpretations of the information on case facts, which leads to their divergent expectations over a ruling. Some firms may have a superior ability to collect and process information to others, which enables them to predict the trial outcomes more precisely. On the other hand, firms will only agree to go to trial if they think there is a reasonable chance of winning. If the SOEs have better information on case merits in general, they can present a higher proportion of favorable cases to the court, resulting a higher win rate in the absence of judicial bias. Moreover, this difference in the win rates caused by an information advantage should be the greatest on cases whose facts are complicated and hard to retrieve, for a superior ability to acquire information would make the greatest difference in those cases. On the other hand, the difference in the win rates should diminish when the case merit is straight-forward, which does not require either party to devote resources in information collection. In fact, in the absence of judicial bias, if parties have little information asymmetry on the case merit, they would settle instead of litigate, as in the Priest/Klein model. Cases with clear-cut facts tend to be settled before they reach the court.

Judicial bias, however, has drastically different implications. When judicial bias is present, cases may be taken to court due to different information over case merits, or different expectations

over a judge's bias. If the two parties are not symmetrically informed on the direction and degree of judicial bias, the party with informational advantage on judicial bias will have a more rational estimate about the likelihood of success at trial, and consequently has a higher chance of winning than the opposite party does. Especially for those cases with clear-cut facts, the only reason to bring such a case to court instead of settling is that one party is relying on the judge's bias to get a ruling in its favor, while the other party does not fully realize the existence or the extent of the judicial bias. On the other hand, even when there is no judicial bias, a complicated case may still be brought to court purely because of the divergent information on intrinsic case merits. Empirically, this means that among all of the cases that are taken to trial, we should observe judicial bias to be more prevalent among cases with more straightforward case merits.

The existing law and economics literature has attributed the types of suit (e.g., property rights, contract, tort, etc.) to the extent of information asymmetry between parties on case merits (Waldfogel 1995, Shavell 1996, Siegelman and Waldfogel 1999). Parties may systematically have different information about facts of a dispute, in ways that vary across suit types. For example, it is commonly argued that information asymmetry on infringements case is large because defendants know their own actions, while plaintiffs do not. This type of information asymmetry makes less sense in a contracts case, since the relevant actions by the defendant are typically observed by both parties. Following this strand of thought, we propose to use the types of suits as a proxy for the levels of potential information asymmetry about intrinsic case merits. For the empirical test, we first eliminate the 67 cases in the sample that involve countersuing, because those cases may have specific complications that are independent of the suit type. This leaves us with a sample of 3,256 suits.

We then categorize the four types of suits into three case levels, according to how straightforward the case facts are or, in other words, according to the level of potential

information asymmetry about case merits. The contract-based cases (suit types 1-3) in general have less information asymmetry about merits than the tort cases (suit type 4). Unlike a tort case, in a contract case, the two parties involved in contracts must have had previous interactions with each other before the trial. There is also more hard information available for inspection, such as the content of the contract, the balance sheets of the firms, and product certificates. Among the contract cases, we define suit type 1 and 2, the loan cases as Case Level 1, which are the cases with the most straightforward case facts. In the loan cases, the obligation of repayment only falls on one party. The performance of repayment is clearly defined and easy to prove. Both parties know exactly what happened, and there is little room for unknown information.

We define suit type 3, the purchase/sale and other contract cases, as Case Level 2. The potential level of information asymmetry of this category falls between the loan cases and right infringement cases. Other types of contracts are usually less complete than a loan contract. They may involve agreements on different aspects of the product quality, or the maintenances of an office building, which cannot be specified comprehensively. Moreover, in those cases obligations fall on both parties. One party's fulfillment of obligation is dependent on the other party's performance of the contract. There is usually more hidden information compared with a loan case.

Finally, we define right infringement and other tort cases (suit type 4) to be Case Level 3 with the largest potential information asymmetry about case merits. The tort cases involve a breach of civil duties, but not contract duties. It requires the proof that the existence of duty is reasonable, and that the causation between the duty and the damage is direct. Without explicit contracts, the implicitly assumed duties are hard to prove and open to interpretation. Moreover, a major proportion of tort cases in our sample are right infringement cases. Those cases are often the so-called "stranger" cases in the sense that the plaintiffs usually do not have any interaction with the defendant until the dispute arises. It is hard for the plaintiffs to retrieve information on

what the defendant did, or for the defendant to retrieve information on what the plaintiff is able to prove, especially given the fact that most of the information is internal. Without judicial bias, information advantage on intrinsic case merit would make a significant difference in predicting the trial outcomes for Case Level 3. We also control for whether the case was appealed, since the decision to appeal for a case is related with the potential complication of the case facts.

Applying our previous argument, we expect to observe that cases with clear-cut facts to exhibit higher judicial bias, which is positively correlated with the favored party's win rate. On the other hand, if the difference in the win rates is caused by information asymmetry on case merits, we should see the information advantage to be magnified on cases with more complicated facts. To test the hypothesis we run the following regression:

$$\begin{aligned} Win_i = \alpha + \beta_1 SOE_i + \beta_2 \text{Case}\_level\_2_i \times SOE_i + \beta_3 \text{Case}\_level\_1_i \times SOE_i \\ + \beta_4 Case\_level\_2_i + \beta_5 Case\_level\_1_i + \beta' Controls_i + \varepsilon_i \end{aligned} \quad (2)$$

where *Case_level_1* is a dummy variable that equals one if the case is of Case Level 1. *Case_level_2* is defined likewise and *Case_level_3* is omitted. β_1 measures the average difference in the probability of winning between the SOEs and the non-SOEs for the cases with the most potential information asymmetry on case merits. β_2 and β_3 measure how the difference in the win rates is affected when we switch from Case Level 3 to Case Level 2 and Case Level 1, respectively. If the story of judicial bias is true, both β_2 and β_3 are expected to be greater than 0.

In Table 3, Panel A, Model 2, we present the result corresponding to Equation 2. Both *Case_level_2* and *Case_level_1* have positive coefficients when they are interacted with *SOE*. By switching from tort cases to contract cases, the difference in the win rates between the SOEs and the non-SOEs has an additional increase of 2.4%. The additional bias associated with switching from tort cases to loan cases is even larger at 5.1%. A t-test on the coefficients of the interaction

terms finds that their difference of 2.7% is statistically significant at a 5% level. The SOEs enjoy larger advantages on cases with less potential information asymmetry about merits, which supports the story of judicial bias and goes against the alternative explanation that SOEs are better at identifying strong cases based on case merits. The control variables keep the same signs as in Table 3 Panel A, Model 1.

There is the legitimate concern that the above result is driven by the lender characteristics in the loan suits, since the loan suits account for the majority of the sample. If the lenders tend to win regardless of the judicial bias, and our sample consists of mostly state owned banks which are lenders, then we would observe a higher win rate of SOEs for Case Level 1. To rule out this possibility, we run the same regression with the subsample of only defendant firms, and include a bank dummy that equals one if a bank is involved in the suit. Since lenders are almost always on the plaintiff side, using the defendant subsample ensures that our result is not driven by state-owned lenders winning the case.

Columns 3 and 4 in Table 3, Panel A present the results. Again, the SOEs have a higher win rate than the non-SOEs. The bias is more prominent on cases with less potential information asymmetry about merits. The difference between the coefficients of *Case_level_1*SOE* and *Case_level_2*SOE* is positive and statistically significant.

Another related concern is that bank loans may have special characteristics. For example, if some of the bank loans are policy loans made to support certain SOEs, those SOEs may get preferential treatments in courts. To deal with the problem, we eliminate all the bank loans and repeat the same test. Columns 5 and 6 in Table 3, Panel A present the result. Our main findings from the full sample stay unchanged. The SOEs have an average win rate that is 8% higher than the non SOEs. The advantage of the SOEs is the largest on the loan cases, creating a difference in the win rates of more than 10% (calculated as 0.062+0.042).

Finally, we refine the case categories to get clearer contrasts on the level of potential information asymmetry about case merits. Based on our four suit types, we further divide the loan cases into cases that only involve banks either as lenders or borrowers (very rarely), and cases involving guarantor companies or loan cases between two non-bank companies. The lender-borrower cases involve the simplest type of obligations, and bank loans have well-defined repayment schedules. A case with guarantor companies may be more complicated because it involves a third party other than the lender or the borrower.

We then exclude all of the tort cases that are not infringement cases from Case Level 3, because the infringement cases are more likely to be "stranger" cases in which there is no previous interaction between the plaintiff and the defendant. On the other hand, in a tort case such as a trade secret leakage case by a former employee, the two parties have some past relationships, and it is less clear whether those cases are exposed to more information asymmetry on case merits than the contract cases.

Using refined Case Level 1 to include only bank loan cases and refined Case Level 3 to include only infringement cases and leaving the Case Level 2 intact, we run the regression specified in Equation 2 again with a bank dummy. The results are presented in Table 3, Panel B. The *SOE* still has a positive and significant coefficient. The two interaction terms between the case categories and *SOE* are of larger magnitudes as compared to the coefficients in Panel A. In a bank loan case, an SOE can have a win rate which is 17% higher than that of a non-SOE. One factor that might have contributed to such a significant discrepancy is that banks are more reluctant to fight an SOE due to its connection to the government. As we refine the case categories, the message from the previous regressions stay the same.

IV.2 Judicial Bias and Legal Environments

In the following sections we go beyond each case idiosyncrasy to present more empirical evidence in support of our claim of judicial bias. The first set of tests we run are concerned with the development of provincial-level legal institutions. We conjecture that the local legal institutions affect the extent of judicial bias against unconnected firms, because poor property rights protection is the fundamental reason that the SOEs enjoy unjustified benefits. If the difference in the winning probability is caused by a bias against non-SOEs, then it should be less prominent when the case is tried in a region with a better legal environment. Moreover, the alleviation of the judicial bias should be more significant on cases with more straightforward case facts, as those are the cases with the largest potential for judicial bias.

To our advantage, China's economic reform in the past 30 years has necessitated the establishment of an almost entirely new set of economics institutions. These institutions have been developed at a varying pace across different regions of China (Xu 2009, Ayyagari et al. 2010), partly due to divergent regional economic policies and the significant autonomous power of the local governments. Such heterogeneities in the legal institution developments across time and regions in China have offered unique opportunities for us to examine the connection between legal institutions and judicial bias in a panel-like setting[12].

The variable we use to measure the development of the local legal institutions is the Producer Property Rights Protection Index at the provincial level taken from the Marketization Index for China's Provinces. It is a widely used index that measures province-level market and legal developments and is jointly published by the National Economic Research Institute and China Reform Foundation annually. The Producer Property Rights Protection Index is constructed based on three components: the number of economics cases filed every year normalized by the

[12] Technically speaking, we do not have a panel data set here; we only have observations of firms which are involved in litigations. There are multiple observations in each province each year, but a particular firm may only appear once.

regional GDP, the extent to which the local regulations emphasize the protection of non-SOEs, and some firm level survey evidences. A high score in the index indicates a better regional legal environment. We choose this specific measure instead of the more widely used overall marketization measure because our study puts an emphasis on the court's discrimination based on the ownership status of the firm. In regions of better property rights protection, the non-SOEs face less government exploitation, which might translate to a more fair court system.

The most updated Marketization Index covers all of the provinces from 1997 to 2007. We match the cases after 2008 with the index value of 2007. Using the average index value between 1997 and 2007 instead does not have a significant impact on our results. Figure 2 gives a summary of the average Producer Property Rights Protection Index across the provinces. There is regional heterogeneity even within the more developed regions. Beijing, Shanghai and Guangdong have high Index scores while places like Chongqing have a low score, consistent with the anecdotal evidence that the Chongqing autonomous city has suffered from abuse of administrative power.

We match the Producer Rights Protection Index from the previous year to the year when the case was tried and the province where the case was tried [13], and run the following regressions:

$$Win_i = \alpha + \beta_1 SOE_i + \beta_2 Lag\_legal_i + \beta_3 SOE_i \times Lag\_legal_i + \beta' \ Controls_i + \varepsilon_i \qquad (3.1)$$

$$\begin{aligned} Win_i = {} & \alpha + \beta_1 SOE_i + \beta_2 Lag\_legal_i + \sum_{n=1}^{2} \beta_{n+2} SOE_i \times Lag\_legal_i \times Case\_level\_n_i \\ & + \sum_{n=1}^{2} \beta_{n+4} SOE_i \times Case\_level\_n_i + \sum_{n=1}^{2} \beta_{n+6} Lag\_legal_i \times Case\_level\_n_i \\ & + \beta_9 SOE_i \times Lag\_legal_i + \sum_{n=1}^{2} \beta_{n+9} Case\_level\_n_i + \beta' \ Controls_i + \varepsilon_i \qquad (3.2) \end{aligned}$$

[13] Under most circumstances it's the location of the defendant. Sometimes the plaintiff may be able to have the case tried in its home province.

where *lag_legal* is the lagged Producer Rights Protection Index from the province where the case was tried. In Regression 3.2, our variable of main interest is the triple interaction term of SOE, lagged legal index, and case category, which allows us to test whether the alleviation of bias varies with different levels of potential information asymmetry on case merits.

We use the lagged index to mitigate the concern of reverse causality. Even though our measure of the legal index does not explicitly take into account judicial bias at the court level, it is possible that the behaviors of the courts may have an impact on the regional legal index. For instance, if the court becomes unbiased, the SOEs may be more reluctant to bring up a suit, because they are less confident of winning. The number of economics cases would drop as a result, which affects the legal index. We do not claim to completely solve the issue of reverse causality by using the lagged index. We argue that, as the main purpose of this paper is to prove that the high win rate by the SOEs is caused by the bias, the reverse causality here is not our major concern. The fact that a smaller win rate of the SOEs may have translated into a better legal index but not the other way around still lends support to our claim that the high win rate of the SOEs is associated with court bias. Nevertheless, we will tackle the problem of reverse causality directly later in the section.

The first two columns in Table 4 present the regression results. Model 1 corresponds to Equation 3.1. As we expected, the SOEs have less chance of winning if the case is tried in provinces with higher Producer Property Rights Index scores. The interaction term has a significant coefficient of -1.9%, indicating that the difference in the winning probabilities between the SOEs and the non-SOEs decreases by 1.9% if the trial province's legal environment index increases by 1, which is the difference between Guangdong province and Heilongjiang province, and is slightly smaller than one standard deviation of the legal environment index across provinces. As the legal environment improves, the court becomes more independent in decision

making, which in turn alleviates the discrimination against unconnected firms. The negative bias on the non-SOEs is less prominent in the regions with better legal environment, though it is not fully corrected. Moreover, Model 2 shows that the drop in the win rate is more prominent for cases with straightforward case merits, further distinguishing our story of judicial bias from the competing explanation of information asymmetry about case merits.

To formally address the problem of reverse causality, we employ an exogenous proxy for the local legal environments inspired by Fan, Wang, Zhang (2010): whether a province was forced to open to foreigners as a treaty port or a leased territory after the first Opium War in the Qing dynasty. After the first Opium War in 1842, China was forced to sign several treaties with foreign countries to establish treaty ports or setup leased territories in some of its provinces[14]. The setup of the treaty ports and leased territories increased China's openness and promoted business contact with the rest of the world. Foreign courts were set up in those areas to handle disputes involving foreigners, and the local court's jurisdiction was restricted. Since these treaty ports and leased territories were opened over 100 years ago, how a court rules an individual case now cannot have had any direct relation to their creation. However, as Fan, Wang, and Zhang (2010) argued, the establishment of these ports and territories is likely to have long-term impacts on the local legal institution development.

We create a dummy variable *port_lease* that equals one if the province was forced to open as a port or became a leased territory, and use the *port_lease* dummy directly in place of the legal index by running the regression:

[14]The treaty ports are located in Anhui, Chongqing, Fujian, Guangdong, Guangxi, Hainan, Hubei, Jiangsu, Liaoning, Shandong, Shanghai, Tianjin, and Xinjiang, Zhejiang. The locations of the leased territories include Tianjin (1860), Shanghai (1845), Jiangsu (1863), Zhejiang (1896), Anhui (1877), Jiangxi (1861), Fujian (1861), Shandong (1889), Guangdong (1857), Chongqing (1901) and Hubei (1861). (Taken from Fan, Wang, and Zhang 2010).

$$
\begin{aligned}
Win_i = \alpha + \beta_1 SOE_i + \beta_2 port\_lease_i + \sum_{n=1}^{2} \beta_{n+2} SOE_i \times port\_lease_i \times Case\_level\_n_i \\
+ \sum_{n=1}^{2} \beta_{n+4} SOE_i \times Case\_level\_n_i + \sum_{n=1}^{2} \beta_{n+6} port\_lease_i \times Case\_level\_n_i \\
+ \beta_9 SOE_i \times port\_lease_i + \sum_{n=1}^{2} \beta_{n+9} Case\_level\_n_i + \beta' \ Controls_i + \varepsilon_i \qquad (4)
\end{aligned}
$$

The underling theory is that the opening of the treaty ports and leased territories had a positive impact on the local legal environment by introducing the Western-style laws at an early stage. The *port_lea*se dummy has a positive correlation of 0.539 with the legal index. We use the opening of ports and leased territories as an exogenous positive shock to the regional legal environments. It is a noisy proxy in the sense that though these provinces on average have higher legal indices, some of them (such as Xinjiang province) may have relatively poorer legal environments today due to other historical reasons.

The results are presented in Table 4, Model 3 and Model 4. Here we take out the province fixed effects because the *port_lease* dummy is a province-level variable that is not time-varying. Provincial gross domestic product (GDP) per capita is included as a control of regional economic development. The win rate of the SOEs drops by 4.3% in the provinces that were forced to open as treaty ports and leased territories. The decrease is larger on cases with more straightforward case facts, as demonstrated in Model 4. Both of the triple interaction terms between SOE, case category, and the port/leased territory dummy have the right negative signs. The interaction term *Case_level_2*port_lease*SOE* is significant, but *Case_level_1*port_lease*SOE* is insignificant due to the noise. The regression results support our previous argument that the judicial bias is reduced in regions with better local legal environments.

As a further robustness check, we also employ a two stage least square (2SLS) method. In the first stage, the Producer Rights Index of a province is regressed on two instruments: the *port_lease* dummy, and the latitude of the province. We interact the two instruments with the SOE

dummy and use the variables to instrument for the interaction term of the Producer Rights Index and SOE. Latitude of a province (measured at the center of the province's capital city) is included as an instrumental variable to capture the geographic feature of a region, since a region's latitude has a great effect on its climate and weather. It has been argued that natural environment puts restrictions on the institution development (Acemoglu 2005). The 2SLS leaves our findings largely unchanged[15].

IV.3 Judicial Bias and Local Connections

The second set of tests makes use of the distinction between national SOEs and local SOEs. A national SOE is owned by the central government, while a local SOE is owned by a provincial or city level government. There are 1,089 cases involving local SOEs in our sample. Compared with the national SOEs, the local SOEs' political connections are constrained by their geographic locations. A local SOE in one province is likely to be favored by the local court, but may not necessarily enjoy the same benefit if the trial takes place elsewhere. Thus, we should observe more bias favoring a local SOE if the case is tried in its home province. The related regression is:

$$\begin{aligned} Win_i = \alpha + \beta_1 SOE_i + \beta_2 Home\_province_i + \beta_3 LSOE_i \\ + \beta_4 LSOE_i \times Home\_province_i + \beta' \ Controls_i + \varepsilon_i \end{aligned} \qquad (5.1)$$

where *LSOE* is a dummy variable for local SOE and *Home_province* is a dummy which equals one if the case is tried in the home province of the disclosing firm.

Furthermore, the strength of local political connections is likely to be affected when there is a turnover of the provincial governor. A change in the provincial governor is usually followed by turnovers of other provincial and city level officials, since new governors would want to promote people closer to them. The governor turnover thus significantly weakens, if not destroys,

[15] Results are not reported here for conciseness. Tables are available upon request.

the existing connection a local SOE has with the current local government. During this period, the control of the old political power has dissolved while the influence of the new political power has yet to be established. Even if the governor turnover is expected, there are only limited things a local SOE can do to secure a new connection in advance, since there is uncertainty over who will be the successor. The national SOEs are less exposed to this problem, because they can rely on the central government.

Under normal circumstances, such turnovers of governors would again expose us to a reverse causality problem. The decision of the reappointment of a governor can depend on various political and economic factors during the governor's tenure. However there are a few exceptions; sudden death is an obvious one. Moreover, according to the regulation on the appointment and selection of party leaders in China, provincial governors have a term of 5 years and can be reappointed only once. By the end of the 10th year in office, governors have to be transferred to a different position. Another regulation is that government leaders have to step down once reaching the age of 65. These are the three exceptional circumstances where the turnovers can be considered to be exogenous. In particular, we denote that a province has an exogenous regime shift if its governor or its provincial party secretary leaves the office for the following reasons: sudden death, reaching the 10th year of tenure, and surpassing the age of 65. Among the 173 total governor turnovers we document across the provinces between 1997 and 2010, 46 of them are defined as exogenous. There are 255 cases in 14 provinces which happened during the exogenous regime shifts.

Exogenous reappointment of new provincial leaders represents a shock to the local political environment. This is a period when the local government has the least interference over court decisions, and when the local SOEs benefit the least by having the trial in their home provinces. In fact, the average number of locally tried cases involving local SOEs drops from the

sample average of 16 per year per province to 9 per year per province during the exogenous leader changes, indicating that the local SOEs are indeed more reluctant to participate in litigations during the governor turnovers. Eliminating the cases which are tried during endogenous leader changes, we are left with a sample of 2,038 cases to run the following regressions:

$$Win_i = \alpha + \beta_1 SOE_i + \beta_2 LSOE_i + \beta_3 Leader\_change_i + \beta_4 LSOE_i \times Leader\_change_i + \beta' Controls_i + \varepsilon_i \quad (5.2)$$

$$Win_i = \alpha + \beta_1 SOE_i + \beta_2 LSOE_i + \beta_3 Leader\_change_i + \beta_4 LSOE_i \times Leader\_change_i + \beta_5 Home\_province_i \times Leader\_change_i + \beta_6 LSOE_i \times Home\_province_i + \beta_7 LSOE_i \times Home\_province_i \times Leader\_change_i + \beta' Controls_i + \varepsilon_i \quad (5.3)$$

where *LSOE* and *Home_province* are defined as before. The variable *Leader_change* is a dummy variable that equals one whenever there is an exogenous province leader change. In Equation 5.3, our main interest is in the triple interaction term of *Home_province*, *LSOE*, and *Leader_change*. We test whether the leader switch has a larger detrimental impact on the local SOEs when the case is tried in the local SOE's home province.

Regression results are presented in Table 5. Model 1 shows that besides the average favor a SOE receives, a local SOE enjoys a 2.8% increase in the win rate when the case is tried in its home province. Model 2 demonstrates that the judicial favor on the local SOEs diminishes by 4.8% as a result of local political regime switches. Model 3 includes the triple difference term with a negative coefficient, implying that the local SOEs having their cases tried at the local provinces suffer higher than average drops in the win rates during the provincial leader turnovers, which is what we would expect if the advantage of the SOE is caused by judicial bias. The interaction term between *LSOE* and *Home_province* is still positive in Model 3. The interaction term between local SOE and leader change remains negative, but becomes insignificant. The impact of leader changes on the local SOEs is concentrated on the cases tried in their home provinces.

IV.4 Self-Established Political Connections by Non-SOE and Judicial Bias

Up to this point in our research, we have used only the state ownership as a proxy for political connections and have shown that the non-SOEs suffer discrimination in court decisions. Facing such a disadvantage, the non-SOEs seek other means to compete with the SOEs. One of the most widely used methods is to rely on the personal networks of their top managers. To be specific, the non-SOEs can hire CEOs or directors who have previously held positions as leading government officials. This kind of personal tie helps firms establish some insider connections with the government and gain political advantages, and is commonly observed in emerging markets. Faccio (2006) studied listed firms in 47 countries and found that political connections are prevalent among listed firms. Both Cull and Xu (2004) and Li et al. (2008) did work specifically on China and found that in regions with a less developed market and weaker legal system, firms are more likely to have connected CEOs/directors.

Based on our previous observations, we conjecture that the non-SOEs with CEO/director connections have an advantage in court compared with those non-SOEs without connections. The CEO/director connection here is only defined within the subsample of non-SOEs because the SOEs are connected by default through their ownership statuses. Tests in this section only involve the subsample of non-SOEs, proposing an alternative measure of political connections and at the same time mitigating the potential concern that the difference in the win rates between SOEs and non-SOEs is caused by some unobserved dissimilarities, but not by political connections.

We first re-estimate the regressions as in Table 3 using the subsample of all the non-SOEs, replacing the SOE dummy with a dummy of CEO/director connection, which equals one if the firm's top official (CEO or director) is previously affiliated with the government.

Table 6 presents the test results, using the first definition of Case Levels. Non-SOEs with connected CEOs/directors win with higher probabilities. The bias is more significant on cases

with less potential information asymmetry, which is demonstrated by the positive coefficients of the terms *Case_level_2*CEO/Dir_Connection* and *Case_level_1 *CEO/Dir_Connection* in Model 2. Model 3 and Model 4 use subsamples of defendant firms. As seen previously, the results in the first two columns are not driven by the lenders winning the cases. A t-test confirms that *Case_level_2*CEO/Dir_Connection* has a coefficient that is smaller than the coefficient of *Case_level_1 *CEO/Dir_Connection* in both Model 2 and Model 4. We also perform the test under the refined definition of Case levels, and the results still hold[16]. The control variables keep the original signs, though some of them become insignificant.

Next we test the implications of local legal environments. As before, we expect the judicial bias to be alleviated in regions with more developed legal institutions. In the first column of Table 7, improved legal environment exerts a correcting force on the bias and makes the connected firms less advantageous, though the magnitude of correction is not as big as in the full sample case. In Model 2, the decrease in the win rate of the connected firm is the greatest on cases with the most straightforward case facts (Case Level 1). In Model 3 and Model 4 we directly add the dummy for the opening of ports or leased territories in place of the legal index to confirm the results. The difference in the win rates drops by 5.8% in the provinces that were forced to open as treaty ports or leased territories. We also perform a 2SLS and the results are largely unchanged. Results are not presented here for conciseness.

We further divide the sample to firms whose CEOs or directors are locally connected and whose CEOs or directors have political connections outside their local provinces. There are 625 cases involving locally connected non-SOEs. We expect the firms with local connections to enjoy extra benefits if their cases are tried locally. We also expect the exogenous local governor turnovers to have a negative impact on the win rate of locally connected firms. Within the non-

[16] Results are available upon request.

SOE subsample, we have 167 cases tried during exogenous provincial leader changes.

Table 8 presents the results. Model 1 shows that the locally connected firms receive extra favors from the courts when the cases are tried locally. Model 2 demonstrates that the change in the local governor has a negative impact on the win rate of the locally connected non-SOEs. During the regime switch, the difference in the probabilities of winning between locally connected and unconnected non-SOEs drops by 7.1%. Column 3 shows that the additional advantage enjoyed by the locally connected firm in the local courts diminishes during the time of leader change, consistent with our full sample results.

The impacts of other control variables are of the same direction and comparable magnitude with the full sample case. One thing worth mentioning is that the *Leader_change* dummy has a significant negative coefficient now, while it has a positive coefficient in the test with the full sample. During the regime switch, an average non-SOE is less likely to win in court. This can be explained by an overall uncertainty caused by the governor turnover, which affects non-SOEs more severely than SOEs.

IV.5 Self Established Political Connections vs. State Ownership

The question asked next is whether the CEO/director connections of non-SOEs can completely eliminate their disadvantages against the SOEs. If this is the case, then the non-SOEs are able to level the playing field without any formal policy interference. Since the majority of the non-SOEs have some form of political connections, we may conclude that only a small fraction of the privately owned firms suffer the judicial bias. To test this, we use the subsample of all of the SOEs and politically connected non-SOEs to re-run the main regression Equations 1, 2, and 3.1. We find that the connected non-SOEs are still more likely to lose compared to the SOEs, though the coefficient is of a smaller magnitude (6.8%) compared to the full sample case. The non-SOEs'

disadvantages still diminish as potential information asymmetry on case merits gets smaller, but the local legal index no longer has a significant impact on the win rate of the SOEs. We do not present the full table here for conciseness. The overall message is that our main findings hold in the subsample only including connected non-SOEs, but the impact of political connections is weaker due to the self-established political ties.

IV.6 Effect of Litigation Outcomes on Stock Performances

Previous literature has shown that litigation announcements have negative impacts on listed firms' stock prices (Bhagat et al.1994, Firth et al. 2010) due to the potential financial distress. Among others, Jarrell and Peltzman (1985) and Garber and Adams (1998) analyzed the impacts of product liability verdicts on firm values in the United States. However, no existing literature has looked at the wealth impact of the court rulings across all suit types. Like product liability cases, most inter-corporation lawsuits involve considerable monetary compensations. If the market reacts to the potential financial distress brought by litigation announcements, it should also react to the realized losses of the losing firms once the uncertainty in the verdict is resolved.

In this section, we provide a succinct test to examine the effect of trial outcomes on the firm's stock prices. We show that market responds differently to favorable and adverse rulings. The judicial bias against unconnected firms has a real wealth impact on the firms.

To examine the market impact of court rulings, we employ an event study method and collect the dates on which the disclosing firms announces the trial outcome and treat it as the event date. The announcement date is usually within a couple months after the verdict date. Though the verdict is already made, the court makes no effort to make the information publically available. Given that many lower level courts do not have well maintained websites, the best most courts can do is to post the verdicts on the bulletin boards outside, which makes it essentially

impossible for the non-local investors to get timely information. Moreover, under certain circumstances,[17] listed firms are allowed to postpone revealing their involvements in pending litigations until the verdicts come out. Some firms choose to do so. Not able to know that firms are involved in litigation, the investors are unlikely to pay attention to particular courts' bulletin boards. As a result, while insiders may hear about the ruling right after (or even before) the formal verdicts are released, most people learn about rulings from the disclosing firms' announcements.

Using a market adjusted model, we calculate the cumulative abnormal returns (CAR) over an event window of (1,5), which means that the CARs are measured from the day after the announcement to 5 trading days afterward. The market beta of the stocks is calculated using daily returns from the fiscal year just prior to the year in which the event occurs.

Table 9 reports the summary statistics of the CAR on different subgroups. We divide our sample to winning and losing firms. There are more losing firms in the sample. The winning firms have an average CAR(1,5) of 0.12%, and the losing firms have an average CAR(1,5) of -0.47%. Though as in Column 2,the t-test cannot reject the hypothesis that the winning/losing firm has a higher/lower-than-0 CAR, a one tail t-test rejects the null hypothesis that the winning firm's CAR is smaller or equal to that of a losing firm at a 5% level (Column 5). The test confirms our conjecture that the winning firm enjoys a better return. To take into consideration the possible information leakage before the verdict announcement date, we also try an alternative event window of (-2,2) (results not reported here). The difference between the mean CARs of winning and losing firms is of the same sign and similar magnitude. However the standard error is larger, and the one tail test is only significant at a 10% level.

To formally test the market impact of the trial outcomes, we employ an ordinary least squares (OLS) regression:

[17] E.g., when the compensation amount is below a threshold.

$$CAR_i = \alpha + \beta_1 win_i + \beta_2 Pol\_Conn_i + \beta' Controls_i + \varepsilon_i \quad (6)$$

where *Pol_Conn* is a dummy for politically connected firms, measured by the state ownership status of a firm or the CEO/director's personal connections in a non-SOE. The dependent variable is CAR(1,5) around the event date. We run the test for both measures of political connection. We also run separate tests for loan cases because those are the cases with the most direct impact on firms' financing decisions.

The first column (all suit types) of Table 10 presents the results for the full sample. We include the trial outcomes and other firm level control variables in the regression, while controlling for province, suit type, industry, and year fixed effect as before. Consistent with our t-test result from the summary statistic table, the *win* variable has a positive coefficient of 0.0053, which is significant at a 10% level. A 0.53% difference in stock returns translates to a 5.2 million RMB (0.66 million USD) difference in the shareholder wealth if multiplied by the average total market value of all the firms in our sample. The ownership status, size, leverage, cash ratio and profitability do not have significant impacts on the CAR, which should be expected as the financial situation of a firm is observable before the trial outcome gets revealed. In the second column we run the regression with only loan cases. The coefficient of *win* is again positive and of comparable magnitude. A win on the loan cases generates a 5-day CAR that is 45 basis points higher.

Column 3 and 4 of Table 10 present the result within the non-SOE subsample. Again, the winning firms have a 5 day CAR that is on average 0.5% higher than the losing firms have. The result holds for the loan cases as well.

In conclusion, the trial outcome has a real impact on firms' shareholder value. The unconnected firms endure economic losses as a direct result of their lower chances of winning. We report a new channel through which the unconnected firms could suffer a financial loss.

V. Robustness Check

To alleviate possible omitted-variable bias, we add other control variables such as sales growth, receivable/asset ratio, and whether the two parties involved in a suit were from the same province. Our findings remain unchanged: sales growth enters insignificantly; receivable ratio has a negative impact on the probability of winning for both SOEs and non-SOEs; and having the two parties come from the same province does not have significant impact.

Additionally, we try different measures for the local legal environment. In particular, we use an index of financial intermediary development and legal institution (Marketization Index No. 7) instead of the Property Producer Rights Index. The results remain quantitatively unchanged, and we will not report the results here for conciseness.

We also use leverage from bank loans as a proxy for firms' political influence, following Calomiris, Fisman, and Wang (2010). Generally, higher leverage ratios imply that the SOEs are more subject to soft budget problems, which signals firms' close relationships with banks. Only when SOEs have strong ties with banks or the government can they get loans with ease. However, as in Calomiris, Fisman, and Wang (2010), our regression with leverage yields mixed results. High leverage ratios, which signal firms' political strength, are also an indicator for high bankruptcy risk. It is hard to separate these two effects. Using short leverage encounters the same problem.

The last step taken is to use a logit model instead of a linear model to run the main regressions in the paper. The means of the coefficients of the interaction terms are of the right signs and significant, but of different magnitude.

VI. Conclusion

In this paper, we document firm-level empirical evidence on judicial bias against politically unconnected firms in China. Using state ownership as a natural form of political connection, we find the SOEs have a winning probability that is 8.6% higher than the non-SOEs, based on a hand-collected sample of 3,323 corporate litigations during 1998-2010. Since winning firms are shown to receive higher cumulative abnormal returns around the verdict announcement, the judicial bias against non-SOEs has a real wealth impact on firms. The effect of political connection in predicting the litigation outcome is more pronounced when the case merit is more straightforward, which distinguishes our story of judicial bias from the alternative explanation that SOEs win more often in an unbiased court due to their superior information on the case merits. We further find that the biases against the unconnected firms in trial are alleviated in regions with improved local legal institutions, during times of provincial leader switches, and when the case is not tried in the home province of the SOE.

Moreover, the non-SOEs can partially correct the judicial biases by establishing political ties through top managers. Using the personal ties of the top managers in the non-SOEs as a second proxy for political connections, we find that the connected non-SOEs fare better than the unconnected ones in court rulings. The difference between their win rates is similarly influenced by the local legal institution development, provincial leader switches, and whether the case is tried locally. However, the connected non-SOEs still will less often compared to the SOEs. The overall evidence is consistent with the judicial bias against unconnected firms in China, which has a negative effect on the firms' shareholder wealth.

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Table 1 Distribution of suit types

This table presents the distribution of different type of cases. Cases are divided into four types: (1) bank loans, (2) non-bank loans, (3) sales/purchase and other contracts, and (4) right infringement and other torts.

Panel A: Number of cases by suit types

The panel presents distribution of different type of cases across between 1998 and 2010. The numbers in bold are the numbers of the cases of a particular suit type as percentage of the total cases in a year.

Suit type	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	Total
1	16	36	106	76	92	90	184	275	228	167	56	58	24	1408
	38.1	**32.4**	**45.7**	**35.8**	**41.3**	**39.3**	**46.6**	**55.9**	**54.4**	**39.7**	**25.9**	**23.6**	**28.2**	**42.4**
2	6	22	44	47	38	47	73	44	65	70	31	34	16	537
	14.3	**19.8**	**19.0**	**22.2**	**17.0**	**20.5**	**18.5**	**8.9**	**15.5**	**16.6**	**14.4**	**13.8**	**18.8**	**16.2**
3	12	40	79	87	83	85	128	160	111	158	101	118	35	1197
	28.6	**36.0**	**34.1**	**41.0**	**37.2**	**37.1**	**32.4**	**32.5**	**26.5**	**37.5**	**46.8**	**48.0**	**41.2**	**36.0**
4	8	13	3	2	10	7	10	13	15	26	28	36	10	181
	19.0	**11.7**	**1.3**	**0.9**	**4.5**	**3.1**	**2.5**	**2.6**	**3.6**	**6.2**	**13.0**	**14.6**	**11.8**	**5.4**
Total	**42**	**111**	**232**	**212**	**223**	**229**	**395**	**492**	**419**	**421**	**216**	**246**	**85**	**3323**

Panel B: Distribution of cases across ownership

The panel presents the distribution of different types of cases across SOEs and non-SOEs. P/D ratio is the ratio of the number of plaintiffs over the number of defendants..

Suit type	SOE			non-SOE			Total
	Plaintiff	Defendant	P/D ratio	Plaintiff	Defendant	Combined	
1	2	673	0.30%	3	730	0.41%	1408
2	119	201	59.20%	46	171	26.90%	537
3	452	428	105.61%	80	237	33.76%	1197
4	66	63	104.76%	**30**	22	136.36%	181
Total	639	1365	46.81%	159	1160	13.71%	3323

Table 2 Summary statistics

This table gives summary statistic of the main variables in the paper. *Win* is a dummy which equals 1 if the disclosing firm wins the case. *Plaintiff* is a dummy that equals 1 if the disclosing firm is the plaintiff. *Ln(asset)* is the natural log of the firm's total book asset as measured in RMB. *Leverage* is the leverage ratio calculated by total leverage/total asset. *Cash ratio* and *operating profit* are measured likewise. *Otherparty_nonSOE* is a dummy that equals 1 if the counterparty is a non-SOE. *Repeated_player i*s a dummy that equals 1 if the disclosing firm is involved in more than 4 other litigations. *High court* is a dummy that equals 1 if the case is tried at a higher level court. *Amount* is the disputed amount measured in 10,000 RMB.

*The maximum and minimum of the dummy variables are not presented here since it is always 1 and 0.

		Full Sample		Non-SOE subsample	
		SOE	non-SOE	connected CEO/Director	Unconnected CEO/Director
number of observation		2004	1319	1040	279
Win	Mean	0.37	0.18	0.19	0.17
	Stdev	0.48	0.18	0.39	0.38
Plaintiff	Mean	0.32	0.14	0.10	0.30
	Stdev	0.46	0.35	0.36	0.35
Ln(asset)	Mean	20.79	20.15	20.19	20.14
	Max	24.87	22.80	22.41	22.80
	Min	14.94	12.31	17.36	12.31
	Stdev	1.00	0.99	0.82	1.04
Leverage	Mean	0.93	2.54	1.77	2.82
	Max	8.50	82.55	43.08	82.55
	Min	0.02	0.05	0.05	0.07
	Stdev	0.96	2.63	3.46	28.82
Cash Over Asset Ratio	Mean	0.10	0.07	0.07	0.07
	Max	0.64	0.59	0.54	0.59
	Min	0.00	0.00	0.00	0.00
	Stdev	0.09	0.82	0.08	0.08
Operating Profit Over Asset	Mean	0.002	0.01	0.006	0.006
	Max	0.75	1.06	0.45	0.64
	Min	-0.22	-2.51	-0.54	-0.25
	Stdev	0.61	0.16	0.10	0.17
Otherparty_nonSOE	Mean	0.40	0.35	0.40	0.34
	Stdev	0.49	0.48	0.49	0.47
Repeated_Player	Mean	0.69	0.62	0.64	0.53
	Stdev	0.50	0.48	0.48	0.50
High Court	Mean	0.15	0.12	0.09	0.12
	Stdev	0.33	0.32	0.28	0.33
Disputable Amount	Mean	3635.71	3095.56	3059.02	3291.94
	Max	1197464	152000	152000	150000
	Min	0.06	1.00	2.58	1.00
	Stdev	30702.42	8111.62	7344.23	11407

Table 3 Judicial bias and information asymmetry on case merit

The two panels report how the state ownership affects the trial outcomes. The dependent variable is the trial outcome, which equals 1 if the disclosing firm wins. *SOE* is a dummy that equals 1 if the firm is state owned. *Case_level_n* (where n=1, 2, or 3) is a measure for the potential information asymmetry on case merits. *Case_level_1* consists of loan cases. *Case_level_3* consists of tort cases which has the highest level of potential information asymmetry. *Case_level_2* consists of other contract cases. *Case_level_3* is omitted.

The control variables include the firm size (*ln(asset)*, unit: RMB), leverage ratio, cash-to-asset ratio, profit ratio, whether the disclosing firm is the plaintiff, whether the firm is involved in more than 4 other litigations (*Repeated_player*), whether the case is tried at a higher level court, and the disputable amount. We also include dummies for appeal, whether the counter-party is a non-SOE (*Otherparty_nonSOE*), the fixed effects of province, industry, year, and suit types. We estimate the robust standard errors clustered by the provinces.

***, **, * are significant at the 1%, 5%, and 10% levels. The numbers in parentheses are clustered standard errors.

Panel A

	Full Sample		Defendant subsample		Non-bank loan subsample	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Win	Win	Win	Win	Win	Win
SOE	0.086***	0.050***	0.052***	0.039***	0.084***	0.062**
	(0.014)	(0. 016)	(0.014)	(0.019)	(0.032)	(0.018)
Case_level_2*SOE		0.024***		0.063***		0.027*
		(0.007)		(0.014)		(0.013)
Case_level_2		0.016		-0.296*		-0.034
		(0.029)		(0.140)		(0.047)
Case_level_1*SOE		0.051***		0.083***		0.042***
		(0.016)		(0.014)		(0.006)
Case_level_1		-0.054*		-0.221*		0.004
		(0.029)		(0.117)		(0.043)
Bank Dummy	--	--	-0.163	-0.060	--	--
	--	--	(0.169)	(0.149)	--	--
Ln(asset)	0.036**	0.018***	0.005	0.005	0.015**	0.019*
	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)	(0.011)
Leverage	0.0001	0.0000	0.0001	-0.0001	-0.0001	-0.0002
	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0002)
Cash ratio	0.575***	0.594***	0.418***	0.355**	1.022***	0.669***
	(0.054)	(0.078)	(0.085)	(0.084)	(0.076)	(0.189)
Operation profit	0.109***	0.098***	0.025	0.056	0.034	0.021
	(0.041)	(0.041)	(0.038)	(0.039)	(0.036)	(0.093)
Plaintiff Dummy	0.447***	0.331***	--	--	0.575***	0.390***
	(0.014)	(0.013)	--	--	(0.021)	(0.020)
Repeated_player	-0.073	-0.049***	-0.073	0.017	-0.041***	-0.042***
	(0.013)	(0.014)	(0.013)	(0.014)	(0.015)	(0.017)
Appeal	0.039	0.004	0.049**	0.024	-0.038	0.019
	(0.027)	(0.807)	(0.024)	(0.016)	(0.027)	(0.016)
Otherparty_nonSOE	0.195***	0.152***	0.094***	0.045***	0.117***	0.113***
	(0.013)	(0.016)	(0.017)	(0.017)	(0.018)	(0.031)
Suit type	Yes	No	Yes	No	Yes	No
Other controls*	X	X	X	X	X	X
Observation	3323	3256	2528	2496	1897	1863
R-square	0.38	0.42	0.18	0.19	0.44	0.38

Panel B Subsample with refined case categories

This panel refines the three case categories presented in panel A. *Case_level_1_R* consists of loan cases only involving banks as either the lenders or the borrowers. All the cases that involve guarantor companies are deleted. *Case_level_2_R* consists of other contract cases. *Case_level__3_R* consists of infringement cases only. *Case_level_3_R* is omitted. The rest of the variables are defined as in Panel A.

	Model 1	Model 2
	Win	Win
SOE	0.061***	0.058**
	(0.017)	(0.029)
Case_level_2_R*SOE		0.091*
		(0.049)
Case_level_2_R		-0.083*
		(0.045)
Case_level_1_R *SOE		0.110*
		(0.061)
Case_level_1_R		-0.061
		(0.047)
Bank Dummy	0.073	0.036
	(0.121)	(0.113)
Ln(asset)	0.016**	0.019**
	(0.008)	(0.008)
Leverage	0.0002	-0.0001
	(0.0003)	(0.0003)
Cash ratio	0.870***	0.828***
	(0.073)	(0.074)
Operation profit	0.101***	0.101***
	(0.041)	(0.041)
Plaintiff Dummy	0.458***	0.291***
	(0.021)	(0.016)
Repeated_player	-0.042***	-0.039***
	(0.015)	(0.015)
Appeal	0.027	0.037
	(0.018)	(0.028)
Otherparty_nonSOE	0.136***	0.148***
	(0.019)	(0.019)
Suit type	Yes	No
Other controls*	X	X
Observation	2768	2768
R-square	0.36	0.37

Table 4 Judicial bias and legal institutions

This table reports how the local legal institutions affect the win rate of the SOEs. The dependent variable is the trial outcome. *SOE* is a dummy that equals 1 if the firm is state owned. *Case_level_n* (where n = 1, 2, or 3) is a measure for the potential information asymmetry on case merit. *Case_level_1* consists of cases with the lowest level of potential information asymmetry. *Case_level_3* consists of cases that have the highest level of potential information asymmetry. We use two proxies for local legal environments (the *legal* variable). In the first two columns, we use *Lag_legal*, which is the lagged producer rights protection index. In the last two columns, we use *port_lease*, which is a dummy that equals 1 if a province was forced to open to foreigners as a treaty port or leased territory.

The control variables include the firm size (*ln(asset)*, unit: RMB), leverage ratio, cash-to-asset ratio, profit ratio, whether the disclosing firm is the plaintiff, whether the firm is involved in more than 4 other litigations (*Repeated_player*), whether the case is tried at a higher level court, and the disputable amount. We also include dummies for appeal, whether the counterparty is a non-SOE (*Otherparty_nonSOE*), and the fixed effects of industry, year, suit types, and province (or regional GDP). We estimate the robust standard errors clustered by the provinces. ***, **,* are significant at 1%, 5%, and 10% level. The numbers in parenthesis are clustered standard errors.

	legal proxy 1: Lag_legal		legal proxy 2: port_lease	
	Model 1	Model 2	Model 3	Model 4
	Win	Win	Win	Win
SOE	0.073***	0.063**	0.057**	0.024*
	(0.014)	(0.026)	(0.024)	(0.013)
legal	0.042	0.061	0.011	-0.021
	(0.045)	(0.040)	(0.039)	(0.025)
SOE* legal	-0.019*	-0.021	-0.043*	-0.055**
	(0.008)	(0.018)	(0.024)	(0.025)
Case_level_2*SOE		0.034**		0.006
		(0.014)		(0.041)
Case_level_2		-0.077		0.093
		(0.132)		(0.078)
Case_level_1*SOE		0.041**		0.013
		(0.017)		(0.013)
Case_level_1		-0.041		-0.048
		(0.168)		(0.050)
Case_level_2* legal		0.015		-0.011
		(0.024)		(0.105)
Case_level_1* legal		-0.016		-0.058
		(0.029)		(0.104)
Case_level_2* Legal*SOE		-0.006*		-0.021*
		(0.003)		(0.011)
Case_level_1* Legal*SOE		-0.009**		-0.043
		(0.003)		(0.031)
Ln(asset)	0.021***	0.019*	0.033***	0.027***
	(0.007)	(0.010)	(0.008)	(0.010)
Cash ratio	0.795***	0.631***	0.317***	0.660***
	(0.065)	(0.187)	(0.058)	(0.179)
Operating profit	0.118***	0.036	0.123***	0.013
	(0.046)	(0.092)	(0.046)	(0.009)
Plaintiff Dummy	0.488***	0.396***	0.471***	0.394***
	(0.017)	(0.018)	(0.017)	(0.019)
Repeated_player	-0.062***	-0.045***	-0.052***	-0.039**
	(0.014)	(0.019)	(0.014)	(0.018)
Otherparty_nonSOE	0.177***	0.106***	0.136***	0.111***
	(0.015)	(0.031)	(0.016)	(0.030)
Suit type	Yes	No	Yes	No
Province fixed effects	Yes	Yes	No	No
Other Controls*	X	X	X	X
Observation	3323	3256	3323	3256
R-square	0.41	0.40	0.41	0.42

Table 5 Judicial bias and local connections

This table reports the regression results of how the local state-owned enterprises enjoy additional benefits when the cases are tried in their home provinces. The dependent variable is the trial outcome. The independent variables include the following: a *SOE* dummy for state owned firms, a *LSOE* dummy for firms owned by the provincial or lower level government. *Home_province* is a dummy that equals 1 if the case is tried in the disclosing firm's home province. *Leader_change* is a dummy that equals 1 whenever there is an exogenous provincial governor turnover.
The control variables include the firm size, leverage ratio, cash-to-asset ratio, operating profit, whether the disclosing firm is the plaintiff, and whether the disclosing firm is involved in more than 4 other litigations (*Repeated_player*). We also include the ownership status of the counterparty (*Otherparty_nonSOE*) and the appeal status of a case. We estimate the robust standard errors clustered by the provinces.
Other control variables include: high court dummy, disputable amount, type of suits, province dummy, industry, and year controls. None of these are significant.
***, **, * are significant at the 1%, 5%, and 10% levels. The numbers in parentheses are clustered standard errors.

	Model 1	Model 2	Model 3
	Win	Win	Win
SOE	0.064**	0.038**	0.043**
	(0.029)	(0.019)	(0.020)
LSOE	0.005	0.011	0.031
	(0.029)	(0.028)	(0.030)
LSOE*Home Province	0.028**		0.030
	(0.014)		(0.026)
Home Province	-0.016		0.026
	(0.020)		(0.017)
Leader Change		0.017	-0.018
		(0.036)	(0.051)
LSOE* Leader Change		-0.048**	-0.053
		(0.023)	(0.050)
Leader Change*Home Province			0.067
			(0.053)
LSOE*Home Province* Leader Change			-0.047*
			(0.025)
Ln(asset)	0.035***	0.023**	0.025**
	(0.006)	(0.006)	(0.006)
Leverage	0.0003	0.0000	0.0001
	(0.0004)	(0.0004)	(0.0004)
Cash ratio	0.669***	0.512***	0.107***
	(0.063)	(0.066)	(0.047)
Operating profit	0.124**	0.098**	0.097**
	(0.042)	(0.042)	(0.042)
Plaintiff Dummy	0.396***	0.349***	0.342***
	(0.012)	(0.012)	(0.012)
Repeated_player	-0.067***	-0.054***	-0.050***
	(0.013)	(0.013)	(0.013)
Appeal	-0.007	-0.006	-0.003
	(0.016)	(0.016)	(0.016)
Otherparty_nonSOE	0.195***	0.196***	0.196***
	(0.014)	(0.017)	(0.014)
Other controls	X	X	X
Observation	3323	2038	2038
R-square	0.36	0.37	0.38

Table 6 Non-SOE subsample: Judicial bias and information asymmetry on case merit

This table reports the regression results of how the personal connection in a non-SOE affects the trial outcomes. The dependent variable is the trial outcome. The independent variables include the following: *CEO/DIR connection* is a dummy variable that equals 1 if the non-SOE has a CEO/director who was previously connected to the government. *Case_level_n* (where n = 1, 2, or 3) is a measure for the potential information asymmetry on case merit. *Case_level_1* consists of loan cases, which has the lowest level of potential information asymmetry. *Case_level_3* consists of tort cases, which has the highest level of potential information asymmetry. *Case_level_2* consists of purchase/sales contract cases which lie in between. *Case_level_3* is omitted. We also include the interaction terms of *Case_level_n* with *CEO/DIR connection*. The control variables include the firm size (*ln(asset)* with asset measured in RMB), leverage ratio, cash-to-asset ratio, profitability, whether the disclosing firm is the plaintiff, whether the firm is involved in more than 4 other litigations in our sample (*Repeated_player*), and whether the case was appealed. We also include the ownership status of the counterparty (*Otherparty_nonSOE*).
Other control variables include: high court dummy, disputable amount, type of suits, province dummy, industry and year controls. None of those are significant.
***, **,* are significant at 1%, 5%, and 10% level. The numbers in parentheses are clustered standard errors.

	Full sample		Only Defendant	
	Model 1	Model 2	Model 3	Model 4
	Win	Win	Win	Win
CEO/DIR connection	0.089**	0.026**	0.063**	0.039*
	(0.041)	(0.001)	(0.028)	(0.020)
Case_level_2		0.065***		0.019**
*CEO/DIR connection		(0.026)		(0.008)
Case_level_2		0.039		0.004
		(0.091)		(0.010)
Case_level_1		0.137***		0.081***
*CEO/DIR connection		(0.024)		(0.024)
Case_level_1		-0.067**		-0.035
		(0.024)		(0.054)
Bank Dummy			-0.130	-0.140
			(0.151)	(0.171)
Ln(asset)	0.017*	0.035	0.007	0.008
	(0.010)	(0.024)	(0.009)	(0.009)
Leverage	0.0002	0.0002	0.000	-0.0000
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Cash ratio	0.590***	0.731***	0.033***	0.112**
	(0.135)	(0.295)	(0.019)	(0.050)
Operating profit	0.105*	0.066	-0.027	-0.030
	(0.064)	(0.131)	(0.051)	(0.050)
Plaintiff Dummy	0.489***	0.366***	--	--
	(0.027)	(0.041)	--	--
Repeated_player	0.190	0.200	-0.008	-0.001
	(0.142)	(0.141)	(0.011)	(0.017)
Appeal	0.057***	0.058***	0.063***	0.065***
	(0.024)	(0.024)	(0.019)	(0.020)
Otherparty_nonSOE	0.077***	0.076*	0.075***	0.041**
	(0.021)	(0.045)	(0.025)	(0.021)
Suit type	Yes	No	Yes	No
Other Controls	X	X	X	X
Observation	1319	1304	954	947
R-square	0.32	0.33	0.07	0.08

Table 7 Non-SOE subsample: Judicial bias and legal institutions

This table reports how the local legal environments affect the connected non-SOEs' win rate. The dependent variable is the trial outcome. *CEO/DIR connection* is a dummy variable that equals 1 if the non-SOE has a CEO or a director who was previously connected to the government. *Case_level_n* (where n=1,2,or 3) is a measure for the potential information asymmetry on case merit. *Case_level_3* consists of cases with the highest level of information asymmetry. We use two proxies for local legal environments (*legal* variable). In the first two columns, we use *Lag_legal*, which is the lagged producer rights protection index. In the last two columns, we use *port_lease*, which is a dummy that equals 1 if a province was forced to open to foreigners as a treaty port or leased territory.

The control variables include the firm size (*ln(asset)*, unit: RMB), leverage ratio, cash to asset ratio, profit ratio, whether the disclosing firm is the plaintiff, whether the firm is involved in more than 4 other litigations (*Repeated_player*), whether the case is tried at a higher level court, and the disputable amount. We also include dummies for appeal, whether the counter-party is a non-SOE(*Otherparty_nonSOE*), the fixed effects of industry, year, suit types and province (or regional GDP). We estimate the robust standard errors clustered by the provinces.

***, **, * are significant at the 1%, 5%, and 10% levels. The numbers in parentheses are clustered standard errors.

	legal proxy 1: Lag_legal		legal proxy 2: port_lease	
	Model 1	Model 2	Model 3	Model 4
	Win	Win	Win	Win
CEO/DIR conn.	0.011*	0.039*	0.072**	0.056**
	(0.007)	(0.016)	(0.026)	(0.027)
legal	0.008***	0.009***	0.052***	0.027
	(0.002)	(0.002)	(0.020)	(0.065)
CEO/DIR conn.* legal	-0.006***	-0.007**	-0.058*	-0.057
	(0.002)	(0.002）	(0.031)	(0.069)
Case_level_2* CEO/DIR conn.		0.015		0.034**
		(0.016)		(0.015)
Case_level_2		0.191		-0.096
		(0.134)		(0.184)
Case_level_1* CEO/DIR conn.		0.023**		0.094
		(0.010)		(0.064)
Case_level_1		0.042		0.211
		(0.090)		(0.158)
Case_level_2* legal		-0.047		0.076
		(0.041)		(0.054)
Case_level_1* legal		0.021		-0.024
		(0.041)		(0.089)
Case_level_2* Legal* CEO/DIR conn.		-0.015***		-0.013
		(0.002)		(0.017)
Case_level_1* Legal* CEO/DIR conn.		-0.069***		-0.029***
		(0.023)		(0.008)
Ln(asset)	0.012	0.023**	0.019***	0.024**
	(0.015)	(0.008)	(0.007)	(0.008)
Cash ratio	-0.003	0.004	0.736***	0.477***
	(0.005)	(0.003)	(0.062)	(0.121)
Operating Profit	0.104*	0.088*	0.075*	0.102*
	(0.057)	(0.029)	(0.040)	(0.058)
Plaintiff Dummy	0.335***	0.276***	0.487***	0.287***
	(0.020)	(0.027)	(0.016)	(0.027)
Repeated_player	-0.062***	-0.037**	-0.068***	-0.029
	(0.014)	(0.019)	(0.014)	(0.020)
Otherparty_nonSOE	0.067***	0.121***	0.077*	0.132***
	(0.028)	(0.035)	(0.061)	(0.018)
Province fixed effects	Yes	Yes	No	No
Other Controls:	X	X	X	X
Observation	1319	1304	1319	1304
R-square	0.37	0.38	0.35	0.36

Table 8 Non-SOE subsample: Judicial bias and local connections

This table reports the regression results of how the local state-owned enterprises enjoy additional benefits when the cases are tried in their home provinces. The dependent variable is the trial outcome. The independent variables include the following: a *CEO/DIR connection* dummy for connected firms, a *LConnection* dummy local connection. *Home_province* is a dummy that equals 1if the case is tried in the disclosing firm's home province. *Leader_change* is a dummy that equals 1 whenever there is an exogenous provincial governor turnover.

The control variables include the firm size, leverage ratio, cash-to-asset ratio, operating profit, whether the disclosing firm is the plaintiff, and whether the disclosing firm is involved in more than 4 other litigation (*Repeated_player*). We also include the ownership status of the counterparty (*Otherparty_nonSOE*) and the appeal status of a case.

Other control variables include: high court dummy, disputable amount, province dummy, industry, and year controls. None of those are significant.

***, **, * are significant at the 1%, 5%, and 10% levels. The numbers in parentheses are clustered standard errors.

	Model 1	Model 2	Model 3
	Win	Win	Win
CEO/DIR connection	0.071*	0.028*	0.013***
	(0.033)	(0.011)	(0.004)
LConnection	0.022	0.032	0.043
	(0.042)	(0.022)	(0.032)
LConnection * home province	0.045**		0.032**
	(0.021)		(0.015)
home province	-0.023		0.038
	(0.026)		(0.023)
Leader Change		-0.087***	-0.084
		(0.031)	(0.063)
LConnection* Leader Change		-0.071**	0.071
		(0.031)	(0.051)
Leader Change* home province			-0.078
			(0.058)
LConn. * Leader Change* home province			-0.025**
			(0.012)
Ln(asset)	0.021**	0.013	0.022**
	(0.012)	(0.012)	(0.013)
Leverage	0.0003	0.0002	-0.0005
	(0.000)	(0.0003)	(0.002)
Cash ratio	-0.961***	-0.412***	-1.153***
	(0.102)	(0.004)	(0.117)
Operating profit	0.111**	0.111**	0.106**
	(0.048)	(0.049)	(0.048)
Plaintiff Dummy	0.478***	0.386***	0.575***
	(0.027)	(0.028)	(0.031)
Repeated_player	-0.022	0.011	-0.019
	(0.019)	(0.018)	(0.023)
Appeal	0.015	0.022	0.022
	(0.020)	(0.020)	(0.020)
Otherparty_nonSOE	0.072***	0.087***	0.074***
	(0.023)	(0.025)	(0.028)
Other Controls	X	X	X
Observation	1319	919	919
R-square	0.38	0.41	0.64

Table 9 t-test on CAR(1,5)

This table compares the five-day CARs of the winning and losing firms after the firms' verdict announcement date. Column 1 is the number of observations. Column 2 is the equal weighted CAR. Columns 3 and 4 are the minimum and maximum CARs. Column 5 is the t-test result of whether the CAR is statistically different between the two groups.
***, **, * are significant at the 1%, 5%, and 10% levels. The numbers in parentheses are standard errors.

	1	2	3	4	5
	Obs.	Mean	Min	Max	diff. in mean (winning-losing)
Winning	939	0.12%	-17%	26%	0.59%**
(Win=1)		(0.04)			(0.10)
Losing	2384	-0.47%	-23%	38%	
(Win=0)		(0.08)			

Table 10 regression analysis on CAR(1.5)

The dependent variable *CAR* is the market adjusted cumulative abnormal returns of 5 days after the verdict announcement. *Win* is the trial outcome, which equals 1 if the disclosing firm wins. *SOE* is a dummy variable, which equals 1 if the firm is state owned. *CEO/DIR connection* is a dummy variable that equals 1 if the non-SOE has a CEO/director who is connected to the government. The control variables include the firm size, leverage ratio, operating profit, and cash-to-asset ratio. We also control for the fixed effects of province, suit types, industry, and year.
***, **, * are significant at the 1%, 5%, and 10% levels. The numbers in parentheses are clustered standard errors.

	Full Sample		Non-SOE only	
	All suit type	Loan Cases	All suit type	Loan Cases
	CAR(1,5)	CAR(1,5)	CAR(1,5)	CAR(1,5)
Win	0.0053*	0.0045**	0.0049*	0.0046***
	(0.0022)	(0.002)	(0.004)	(0.0011)
CEO/Dir Connection			0.0011	0.0018
			(0.070)	(0.0019)
SOE	-0.0011	-0.0017		
	(0.002)	(0.009)		
lnasset	-0.003	-0.003	-0.001	-0.004
	(0.003)	(0.003)	(0.004)	(0.007)
leverage	-0.0004	-0.002	-0.0003	-0.001
	(0.002)	(0.002)	(0.001)	(0.003)
cash ratio	0.002	0.0064	-0.0005	0.0083
	(0.035)	(0.0052)	(0.002)	(0.009)
Operating Income	0.011	0.007	0.001	-0.005
	(0.022)	(0.024)	(0.027)	(0.005)
suit type	Yes	No	Yes	No
Other controls	X	X	X	X
Observation	3323	1954	1319	784
R squared	0.005	0.008	0.004	0.003

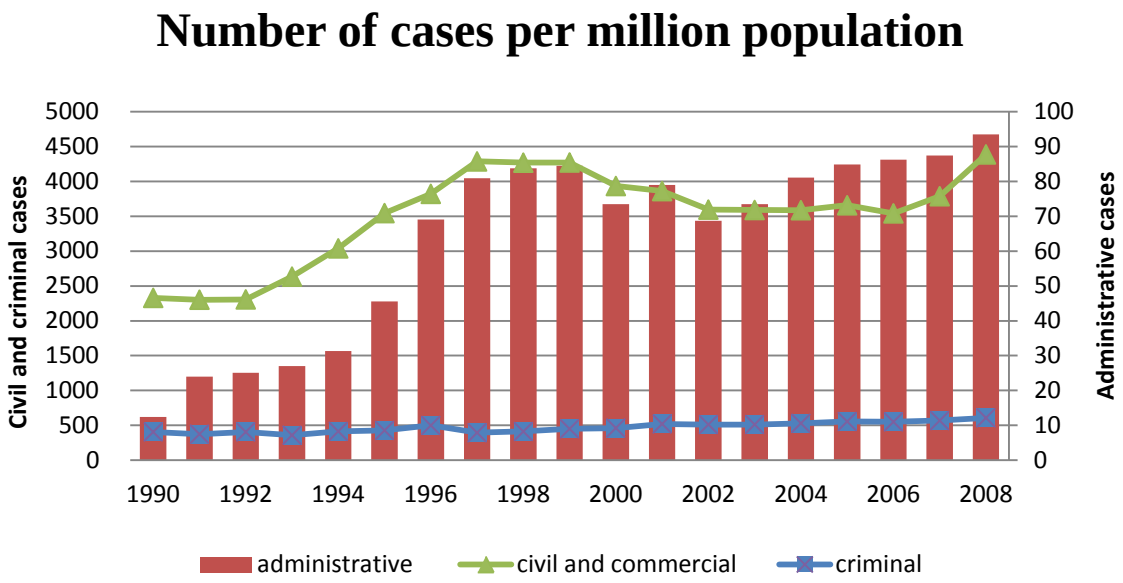

Figure 1 Number of cases per million population, national average

This figure shows how the number of cases per million population changes across the years. The left hand side y-axis is for the number of civil and commercial cases, and criminal cases. The right hand side y-axis is for the number of administrative and criminal cases.

Source: The Law Yearbook of China (1990-2009), published by China Law Society

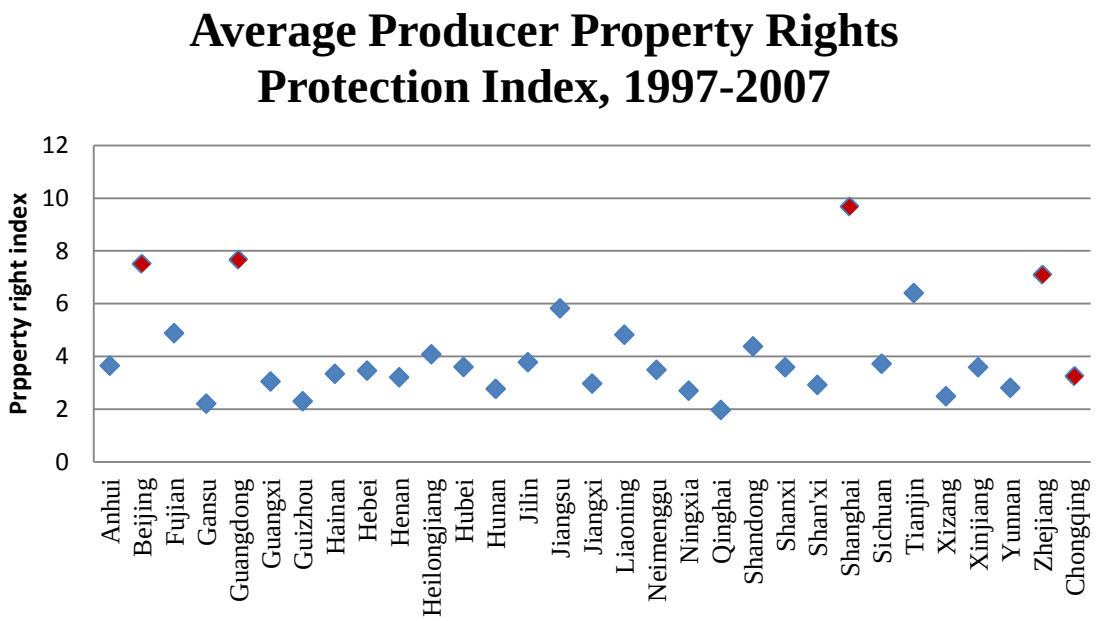

Figure 2 Average Producer Property Rights Protection Index, 1997-2007

This figure gives a summary of the Producer Property Rights Protection Index across the provinces. A higher index score means the province has better property rights protection.

Source: Marketization Index for China's Provinces (1997-2008), published by National Economic Research Institute