Political Connections and Corporate Litigation: Evidence from a Quasi-Natural Experiment

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Abstract

This paper studies the causal impacts of political connections on corporate litigation. Specifically, by exploiting an unanticipated depoliticization reform in China which forces all politically connected directors to resign from listed firms, we investigate how political connections impair the effectiveness and fairness of the judicial system. We show that firms' government connections deter disadvantaged groups from taking legal action to resolve disputes, and that the weakening of the political ties results in greater litigation risk for connected firms in forms of higher likelihood of and larger monetary amounts involved in litigation as defendants. The effects are stronger for non-state-owned firms, financially distressed firms, and firms in regions with weak legal institutions. We also find that plaintiffs fare better in litigation against those previously connected firms after the reform. Overall, our analysis highlights the social costs of judicial corruption, and demonstrates that curbing firms' abusive political power in judicial affairs can effectively mitigate the biases rooted in the judiciary.

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Keywords: Political Connections, Corporate Litigation, Litigation Risk, Judicial Biases.

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1 Introduction

A fair and effective judicial system is essential for the security of property and the enforcement of contracts (Djankov et al., 2003). Moreover, judicial independence is regularly portrayed as vital to rule of law, good governance, economic growth, human rights and well-being, and political stability (Peerenboom, 2009). Under these premises, firms and individuals can rely on the formal legal system to resolve legal disputes. However, judicial independence is often undermined by political interference and corruption, especially in emerging market economies (e.g., Djankov et al., 2003; Allen et al., 2005; Wang, 2015). In countries such as China where judicial institutions are less independent and accountable, judicial officials surrender legal standards and professional integrity to external political or economic pressure (Gong, 2004), and courts can be exploited to achieve wealth transfers from the unconnected to politically connected parties (Ginsburg and Moustafa, 2008). Once the judiciary is "captured" by political parties or interest groups, public trust and confidence in the legal system may decline, and courts may no longer serve as an attractive venue for resolving disputes. In the commercial realm, a lack of an independent judiciary will put legitimate businesses at a great disadvantage and impede sustainable development and long-term prosperity of the economy (North, 1990). Therefore, it is crucial to understand what regulatory incentives may be useful in promoting an independent legal system with fair and impartial courts.

In this situation corporations have every reason to minimize the risk and mitigate the consequences of litigation by utilizing their political connections. Ample evidence has pointed to the fact that litigation incurs huge costs to firms. Once a firm is sued, the market will always interpret it as negative news and impose significant penalties on the defendant, regardless of the litigation outcome (Bhagat et al., 1998; Fich et al., 2007; Murphy et al., 2009; Gande et al., 2009; Firth et al., 2011; Haslem et al., 2017). Furthermore, defendant firms suffer substantial reputational damage and experience declines in cash flows due to reductions in sales and increases in financing and

contracting costs (Karpoff and Lott, 1993; Karpoff et al, 2005; Karpoff et al., 2008). Nevertheless, despite the apparent merits having good political connection can bring in legal disputes, only a few case- and correlation-based studies have attempted to investigate the association between political connections and potential protection and favoritism connected firms may enjoy in the judicial process. For example, Firth et al. (2011) document that upon the announcements of cases filed against them, firms with personal political ties suffer fewer losses of shareholder wealth than is the case in unconnected firms, while state-owned enterprises (SOEs) do not appear to have such pattern. In contrast, Lu et al. (2015) find that politically connected firms, especially the SOEs, are more likely to obtain favorable litigation outcomes.

In this paper, we provide novel firm-level evidence on the impacts of disruption to political connection to uncover the value of political ties in corporate litigation. The causal relationship between political connection and corporate litigation is ambiguous. On the one hand, the loss of political connection may discipline a connected firms' behavior, and the resultant reduction in business corruption and misconduct may lead to fewer lawsuits being filed and penalties being imposed. On the other hand, a defendant's political power may distort the standard of court decisions, resulting in lower expected benefits and higher expected costs for plaintiffs to litigate the disputes, thus reducing plaintiffs' incentives to file a lawsuit in the first place. Accordingly, only a less corrupt and more independent judiciary will encourage people to use the court more often and lead to correct decisions and fair judgments. Furthermore, firms' decisions to build up political connections are highly endogenous. In the absence of any restriction on the abuse of political power, firms anticipating a potentially high risk of litigation can strategically foster connections with government agencies to preempt the judicial system, giving rise to a reverse causality problem.

To test these competing predictions while overcome the endogeneity problem, we exploit an unanticipated regulation in China which removed all politically connected personnel from indepen-

dent directorships of listed firms. Specifically, on October 19, 2013, the Chinese Communist Party (CCP) issued "Rule 18", which forced the incumbent party and government officials as well as the former officials retired within the previous three years to resign from public firms as independent directors immediately. Following the wave of the large-scale resignations triggered by Rule 18, political connections through these directors have been disrupted and significantly weakened. China serves as an ideal laboratory to test our hypotheses in that judicial fairness is often respected in the commercial realm but not in the political realm in China, rendering the rule of law vulnerable to political influence (Wang, 2015). In this quasi-experimental setting, we first examine how the depoliticization reform affects firm-level litigation risk and quantify the relevant magnitudes. We construct a 10-year panel dataset spanning the years from 2009-2018 to estimate the effects of political connections on corporate litigation. We first show that treatment firms that are affected by Rule 18 tend to be larger, more levered, riskier, and underperforming compared with other listed firms. These patterns are indicative of the fact that these firms were inefficiently managed and should have had a higher litigation risk (Strahan, 1998; Gande et al., 2009). Nevertheless, the risk somehow does not fully materialize because of their political power. Employing a difference-in-differences (DID) strategy based on a propensity score matched sample, we compare firms with forced government official resignations as independent directors due to Rule 18 (treatment group) with firms without politically connected independent directors (control group) before and after the issuance of Rule 18. We find strong evidence that firms which lose official directors are no longer protected as before and experience higher litigation risk. Following the enforcement of Rule 18, the disadvantaged groups are more likely to go to court to resolve disputes with the connected firms. On average, the probability of a connected firm being sued goes up by a stark 10.4%; the number of cases against a connected firm per year rises by 0.23; and the total monetary amount involved in cases against a connected firm per year also increases by almost 0.2% of its total assets. These

findings manifest the social costs associated with judicial corruption, and reflect an ex ante inefficient equilibrium where connected firms accused of wrongdoings enjoy protection and favoritism from their political power in the judicial system while less politically empowered counterparties suffer from their inability to seek justice and to receive fair and impartial treatment in court.

We next investigate the cross-sectional effect heterogeneity of the depoliticization regulation. We start by exploring whether state ownership matters, given that SOEs often receive preferential treatments from the government (see, e.g., Kornai et al., 2003) and judges as government employees favor SOEs in court (Lu et al., 2015). We find that the treatment effects are stronger for non-SOEs firms than for SOEs, suggesting that government-related directors are more important for non-SOEs. We then examine whether the impacts of loss of political connections are stronger for firms that are in financial distress or close to it, as they are more vulnerable to litigation (Cutler and Summers, 1988; Bhagat et al., 1994; Firth et al., 2011). We show that litigation risk indeed becomes more salient for connected firms that are more financially distressed. We further our investigation by considering the strength of regional legal protection, and find that the impacts are more pronounced for firms in provinces with weak legal institutions, which implies that the judicial system is more susceptible to political interference in these areas.

Finally, by focusing on the other side of the litigation, we observe more plaintiff victories in cases against connected firms following the disruption to political connections. The total monetary amount of damages awarded per year in winning cases against a connected firm also becomes larger. These findings are consistent with the idea that the court advantage of the politically connected firms is reduced after the depoliticization reform.

Our findings survive a battery of robustness checks, including placebo tests with pseudo treatment years or pseudo treatment firms, tests that control for potential confounding events of the anti-corruption campaign, tests with alternative estimation windows, and tests with other matching

procedures.

Our paper contributes to the literature on the analyses of the costs and benefits of political rent-seeking by firms, and corruption more broadly. Prior studies have established that political connections can help firms to be well-positioned in the allocation of economic resources and extraction of rents, which enhance firm value and performance (e.g., Fisman, 2001; Faccio, 2006; Li et al., 2008; Goldman et al., 2013). We extend the extant literature by investigating adversary corporate litigation, a relatively underexplored topic in the political connection literature. By exploiting the unanticipated disruption to political ties of board members, we estimate the value of firms' political connections in the judicial process. Meanwhile, another strand of the literature emphasizes the negative consequences of political connections as they often result in resource misallocation (e.g., Khwaja and Mian, 2005; Fan et al., 2007; Claessens et al., 2008), negative externalities and inefficiencies (Giannetti et al., 2021), and regulatory non-compliance (Fisman and Wang, 2015). Importantly, by focusing on the salience of litigation risk experienced by previously connected firms, our findings highlight the social costs associated with judicial corruption due to political connections. The deeply-rooted biases in the judiciary deter the the less politically endowed parties from using courts to resolve disputes, which may ultimately result in a loss of confidence in the judicial system, reduction in business activities, and slowdown of economic growth (Murphy et al., 1993; Mauro, 1995).

Our study also contributes to the literature on the real effects of government regulation. Much of the research in this area has been concentrating on the negative impacts of onerous regulations (e.g., Djankov et al., 2003 on entry regulation; Vig, 2013 on creditor protection; Fisman and Wang, 2017 on workplace "Death Ceiling" regulation; Bai et al., 2019 on employment protection). Nevertheless, Shleifer (2011) regards government regulation as an efficient remedy for the failure of courts to solve legal disputes cheaply, predictably, and impartially. By focusing on litigation risk

of politically connected firms, our paper confirms Shleifer's view and unravels the positive welfare implications of the Rule 18 depoliticization reform which levels the playing field in the judicial process for less politically empowered litigants.

Our work relates to a growing body of literature which examines the consequences of departure of politicians from a firm. In the seminal paper by Fisman (2001), he shows that rumors of Indonesian President Suharto's worsening health substantially reduce the value of those well-connected firms. Kim (2018) documents that the sudden losses of firms' political capital due to unexpected exits of legislators from the U.S. Congress result in more investment yet negative announcement returns, reduced profitability, and reduced sales to the government. Two related studies consider the same regulatory change as ours, namely Rule 18, and investigate its impacts on firm value and performance as well as financial and accounting policies. Berkowitz et al. (2021) illustrate that connected firms initially lost value but then bounced back by adjusting their operations in a more efficient manner. Hope et al. (2020) find that connected firms increase their financial reporting quality after the reform. In contrast, our paper examines an unexplored area of corporate litigation. To our best knowledge, we are among the first to quantify the aggravated litigation risk of firms after the weakening of their political connections.

Finally, our research adds to an emerging empirical literature on the economic analysis of litigation. It is well documented that political ideology has important real effects on corporate litigation and the judiciary at large (Hutton et al., 2015; Cassella and Rizzo, 2020; Gormley et al., 2020). Moreover, several types of judicial biases, such as racial bias (Arnold et al., 2018), home bias (Bhattacharya et al., 2007) and pro-debtor bias (Chang and Schoar, 2013), have been found to be deeply rooted in the legal systems. Our paper focuses on a particular form of bias due to corporate political connections and extends the extant literature that documents the associations of corporate political connections with less negative market reactions to litigation (Firth et al., 2011)

and more favorable court outcomes (Lu et al., 2015). We illustrate the relationship between political connections and judicial biases with a causal interpretation. More importantly, our work uncovers an overlooked negative externality of political ties which can deter stakeholders from using courts to resolve disputes with the connected firms. Further, Lu et al. (2015) argue that state ownership has a first-order effect in corporate lawsuits while the impact of management's personal political connections tends to be mild. In contrast, our findings demonstrate that connections through hiring government officials as independent directors also play a significant role in the judicial process and have large and economically meaningful impacts on the litigation risk faced by connected firms. Moreover, our paper differs from those two papers in that, instead of solely relying on case-level data, we provide firm-level causal evidence on the changes in the magnitudes of litigation risk experienced by firms that suffer from the weakening of their political connections.

This paper proceeds as follows. Section 2 describes the institutional background. Section 3 discusses the potential relationship between political connections and corporate litigation. Section 4 illustrates the identification strategy and the construction of the sample. Section 5 discusses the empirical results. Section 6 concludes.

2 Institutional Background

2.1 China's Legal System

China's legal system is largely a civil law system with elements of socialist law tradition, reflecting the influence of the legal systems of both Continental European in the 19th and early 20th centuries and Soviet Union in the 1950s. After the turmoil of the Cultural Revolution in the late 1970s, the wrecked Chinese legal system was reconstructed with an aim to structure and maintain law and order in people's daily life. Nowadays, the Chinese judicial system has evolved into a unique

legal system with Chinese characteristics, which is characterized by both a strong commitment to protecting the single-party state's core interests, and an increasingly significant role as the primary institution for adjudicating social, economic, and political conflicts (Xu, 2020). In the modern Chinese economy, the post-reform economic laws, such as the Contract Law (1994), the Property Law (2007), and the Company Law (2005), are designed to protect contract and property rights, safeguard the operations of both state-owned and private firms, and promote trade, investments, and economic growth. Nevertheless, in China, judicial fairness is often respected in the commercial realm but not in the political realm, rendering the rule of law vulnerable to political influence (Wang, 2015).

The design of the Chinese courts has been inspired by the civil law tradition in continental Europe, where courts are more inquisitorial than adversarial in nature (Wang et al., 2017). Court hierarchy prescribed in the Organic Law of the People's Courts is outlined as follows: (1) The Supreme People's Court is the highest judicial organ in mainland China and the court of final appeal. According to China's Constitution, the National People's Congress, which is structured as a unicameral legislature, has the power to legislate and oversee the operations of the Supreme Court. (2) The High People's Courts are the highest-level courts at the provincial level. (3) The Intermediate People's Courts are established at the prefecture level. (4) The Basic People's Courts are established at the county or district level. All courts are funded by the governments at the corresponding levels. Whether a case is heard at the court of a specific level mainly depends on the monetary value of the matter, which varies according to the degree of economic development of each province. For instance, in wealthy provinces such as Guangdong, Jiangsu, and Shanghai, cases with monetary stakes over RMB 500 million are heard in the High Courts, while the threshold drops to RMB 100 million in less wealthy provinces like Gansu, Guizhou, and Tibet. In addition, a ruling from a court of first instance can be appealed only to the next-highest court. If, however,

the appellate judgment is rendered with error, as determined by a higher-level court, a review or a retrial may be requested through the trial supervision proceedings. In practice, the courts receive internal political supervision from the Communist Party Committee (*dangwei*) and external supervision from the Political and Legal Affairs Committee (*zhengfawei*). Every court in China is institutionalized with a unique committee called the Adjudication Committee (*shenpan weiyuanhui*). The committee, which is composed of the Party secretary, the president, the vice presidents, and the division heads of the court concerned, virtually governs the decision-making mechanism in litigation, and often decides the outcomes of significant cases behind closed doors rather than in courtrooms (He and Su, 2013). At the same time, judges are government employees appointed by the political leaders of the courts, and they may receive instructions from a higher power in the hierarchy of the court system when giving judgments. In general, most cases are tried by a collegiate panel (*heyiting*) of three judges which issue verdicts collectively. Besides being subject to political influence, the judicial work is also individualized because laws and regulations are often vaguely defined and the collegiate panel possess a great deal of flexibility in their interpretation and implementation. It is worth noting that the courts themselves do not have the power of judicial review, and the resolution of conflicts of laws is set forth in the Legislation Law where an interpretation is requested from the legislative body, thereby further reducing the independence of the judiciary (Firth et al., 2011).

Evidently, the most fundamental deficiency of the Chinese legal system is manifested in the equivocal relationship between the CCP and the judicial authority, and judicial independence is often impaired by the personal power of the party. In particular, local CCP officials are notorious for interfering with judicial work. They do so when they find a case to be socially or politically important in their localities, or, worse, to have personal implications for themselves (Gong, 2004). Therefore, such a deficient judiciary often provides politically connected special interest groups

with well-structured opportunities for manipulation and exploitation in judicial affairs.

2.2 Rule 18

Since President Xi Jinping took over as the paramount leader of China in 2012, the government under Xi's regime has vowed to cut the politics-business nexus and to combat corruption. On October 19, 2013, the Central Organization Department of the CCP unexpectedly issued "Rule 18" which requires current government and party officials above certain ranks as well as former officials retired within three years to resign from listed firms as independent directors with immediate effect. Officials who still wish to remain in the listed firms must seek special approval and receive no compensation. The practice of hiring officials as independent directors by Chinese listed firms used to be quite commonplace, because it can not only help to meet the requirement by the China Securities Regulatory Commission that at least one-third of the board be composed of independent directors but also allow firms to enjoy preferential access to various resources through their political connections. Nevertheless, officials often lack sufficient professional knowledge and business experience. As a result, such political connections may also bring about rent extraction by politicians, deterioration in corporate governance quality, and loss of value for firms. Before the depoliticization reform in 2013, about 25% of the Chinese listed firms had at least one official sitting on their boards as an independent director. Rule 18 was implemented with an unusually strong force, which triggered an unprecedented tide of director resignations within a short period. Because the sudden mandate of forced director resignations had not been anticipated at all, the unexpected large-scale departures of officials resulted in substantial exogenous disruption to the political connections of the listed firms.

3 Relationship between Political Connections and Corporate Litigation

China's judiciary agency is generally considered as an administrative unit within the political system (see, e.g., Lubman, 1999; Potter, 1999; Gong, 2004; Li, 2012). The Chinese government *de jure* and *de facto* controls the legal system. Accordingly, politically connected firms can monopolize their privileges and prevent the equalization of the playing field in the judicial process, which may potentially discourage the disadvantaged groups from utilizing the judiciary to sue them (Wang, 2015). Moreover, the political power they possess will assist the connected firms in multiple ways during litigation (Abdulmanova and Ferris, 2018). Government officials can introduce defendant firms to relevant interest groups which may influence the plaintiff to settle. Because laws in China are commonly drafted in broad and general terms and are subject to individualized judicial interpretations (Wang et al., 2017), officials can also connect the defendant firms with local politicians of higher ranks, powerful law firms, and court rulers, which may help to shape the verdicts to the defendants' advantage. In addition, officials may indirectly affect the lawsuit resolution and monetary penalties by using their implicit and explicit authority to help to excuse the defendant firms' wrongdoings and question the penalties.

Nevertheless, the causal impacts of disruption to firms' political connections on corporate litigation are less clear-cut and have not been explored in the literature so far. On the one hand, the weakening of their political ties may discipline connected firms' behavior, and the resultant reduction in misconduct and wrongdoing may lead to fewer lawsuits being filed and fewer penalties being imposed. On the other hand, defendant firms' political power may distort the decision standard in court, resulting in higher expected costs and lower expected benefits for plaintiffs to litigate the disputes, thus reducing plaintiffs' incentives to file a lawsuit in the first place (Priest and Klein,

1984). Conversely, a less captured and more independent judiciary will lead to higher expected net payoff to a plaintiff by initiating litigation. As such, a stronger legal system will encourage a firm's stakeholders to use the court more often and lead to correct decisions and fair judgments when the firm is sued. Additionally, firms' decisions to build up political connections are highly endogenous. It has been well documented that litigation can be anticipated by firms based on their industry membership (Francis et al., 1994; Johnson et al., 2000; Field et al., 2005) or certain financial and governance characteristics (Johnson et al., 2000; Gande et al., 2009). Hence, firms anticipating a high risk of litigation are very likely to strategically foster connections with political agents to preempt the judicial system and minimize potential losses in future litigation, which gives rise to a reverse causality problem for studies which only simply look at the correlations. To test these competing predictions while overcome the endogeneity issue, we exploit the unexpected depoliticization reform of Rule 18 in China, which removed all politically connected independent directors from the listed firms, as a quasi-natural experiment.

4 Identification Strategy and Sample Construction

4.1 Identification Strategy

By exploiting government officials' resignations imposed by Rule 18 as an exogenous source of variation in listed firms' political connections, we employ a DID strategy to assess the causal effects of political connections on corporate litigation. This strategy compares the litigation risk of the treatment firms (firms that experienced at least one resignation of a government official director) with that of the control firms (firms without director resignations) in a ten-year window. Specifically, we estimate the following DID model:

$$y_{it} = \beta \cdot Rule\,18_i * Post_t + \delta' \cdot Controls_{it} + \gamma_i + \theta_{kt} + \mu_{pt} + \epsilon_{it}. \quad (1)$$

where y_{it} denotes the outcome of interest for firm i and year t. We construct four variables related to corporate lawsuits. $Sued_{it}$ is an indicator variable which is set to one if firm i is sued for a dispute as a defendant at least once in year t. The indicator variable helps to alleviate the concern that litigation filings could be frivolous and mitigate the effects of follow-on opportunist lawsuits. $Case\,Number_{it}$ is the logarithm of one plus the total number of cases filed against firm i in year t. $Litigation\,Stake_{it}$ is the logarithm of one plus the total amount of stakes of litigation filed against firm i in year t, which measures the absolute magnitude of the total monetary amount involved in litigation. $Litigation\,Stake\,(\%)_{it}$ is the ratio of total amount of stakes of litigation filed against firm i to its total assets in year t, which measures the relative magnitude of the total monetary amount involved in litigation and is expressed in percentage term. $Rule\,18_i$ is a treatment dummy variable which takes one for firms with resigned government official directors due to the reform and zero otherwise. $Post_t$ is a year dummy variable which equals one for periods after the announcement of Rule 18 (i.e. from 2014 to 2018) and zero otherwise (i.e. from 2009 to 2013).[1]

Following the prior literature (see, e.g., Strahan, 1998; Johnson et al., 2000; Field et al., 2005; Gande et al., 2009; Hutton et al., 2015), we control for financial characteristics, governance quality, and market circumstances which can affect corporate litigation risk (See Appendix A for a detailed list of variables with their definitions). The control variables are lagged by one year to mitigate concerns of reverse causality. γ_i denotes the firm fixed effects, absorbing all time-invariant differences across firms. θ_{kt} is the industry-by-year fixed effects, controlling for the time-varying heterogeneities across industries. μ_{pt} is the province-by-year fixed effects, controlling for the time-varying heterogeneities across regions. The inclusion of province-by-year and industry-by-year fixed

[1]We believe that our choices of the post treatment years are reasonable in that after the announcement of Rule 18 near the end of 2013, although it might take time for all official directors to fully separate from the listed firms, the firms' political power was already substantially weakened after 2013. Because Rule 18 is a crucial part of the anti-corruption campaign by the Chinese government, directors that were in the middle of departing from the firms after the announcement would fear the suspicion of corruption and thus no longer dare to exert their political influence over judicial affairs. In the unreported analysis, we derive qualitatively similar results by using an alternative staggered DID design where the treatment year is the year that an official director left the firm.

effects enable us to obtain credible estimates by alleviating the concern of omitted variables that could confound our analysis. Consequently, any unobserved/unincluded time-varying province-specific factors, such as local business cycles and local government policies that change around the time of the enforcement of Rule 18 and affect our outcomes of interests, do not bias our estimated effects. Further, industry-by-year fixed effects mitigate identification concerns by controlling for any potential different trends across industries over time. We cluster standard errors at firm level to adjust for the serial correlations of residuals.[2] For regressions with fixed effects of which the dependent variable is binary, we use the *linear* probability model rather than the non-linear ones such as the Logit or the Probit models because they only give consistent estimates under very strong and unrealistic assumptions (see, e.g., Wooldridge, 2010).[3]

To better estimate the treatment effects, we first adopt the propensity score matching (PSM) method to identify a group of control firms that are comparable to the treatment firms. We use observations in 2012 to estimate all listed firms' predicted probability of experiencing government official resignations due to Rule 18 in a Logit model. We include independent variables that can potentially affect corporate litigation in equation (1) to ensure that these observable dimensions are similar across treatment and control groups. To both avoid the problem associated with fixed effects in the Logit model and account for the time-varying industry and provincial characteristics, we control for industry sales growth and GDP per capita at the province level. By estimating the Logit model, we obtain the predicted propensity scores for all listed firms and then match each treatment firm with a control firm that has the closest propensity score within the same industry. We perform the 1:1 matching procedure without replacement.

[2]For robustness, we also cluster the standard errors at province level even though the total number of provinces in our sample is less than 50. We conclude with similar results.

[3]For more details, see Wooldridge (2010), Sections 13.9.1 and 15.8.2-3.

4.2 Sample Construction

We start our sample construction by including all publicly listed non-financial firms in China in a ten-year window from 2009 to 2018. We hand collect all public reports about director resignations after the announcement of Rule 18 from October 19, 2013 until December 31, 2016 when almost all official directors have already resigned from the firms. We collect the background information for each director from the China Stock Market and Accounting Research (CSMAR) database and conduct an extensive web search if the information is missing. To identify the treatment firms, we first exclude firms that went public after 2014 as these firms were already aware of Rule 18 when they were listed. Second, we keep firms in the treatment sample if their public announcements explicitly state that the resignations of the official directors are due to Rule 18. In this respect, as some firms are less transparent and tend to hide the true reasons for director resignations, we manually check the backgrounds of the departing directors and exclude them from the sample if they are not identified as government officials. We also exclude firms with directors who do not have civil ranks and who are reported to have resigned voluntarily for personal, health, or career reasons. Finally, we exclude firms with directors who are affiliated with universities, public organizations, and SOEs. Although these directors have civil ranks as officials, their political power is rather limited. We drop treatment firms that cannot be matched with any control firm. We require that all firms in our sample have financial, governance, and stock market data from the CSMAR and WIND databases. Continuous variables are winsorized at the 1st and 99th percentiles to mitigate the influence of outliers. Before performing the PSM procedure, we have 368 treatment firms and 2058 candidate control firms. After matching, we end up with a matched sample consisting of 336 treatment firms and 336 control firms. We summarize the construction of the treatment sample in Table 1. Appendix B reports the results of the Logit model and Appendix C compares the key characteristics of the treatment versus control firms in 2012. The treated and control firms

initially differ substantially in most dimensions. Specifically, the treatment firms that are affected by Rule 18 tend to be larger, less well-performing, riskier, and with a more concentrated ownership structure compared with other listed firms. These patterns are indicative of the fact that these firms were inefficiently managed and should have been expected to have a higher litigation risk before the reform (Strahan, 1998; Gande et al., 2009). Nevertheless, the risk somehow does not fully materialize because of their political power. In contrast, in the matched sample, the treated and control firms have similar summary statistics across all variables, indicating high quality of covariate balance.

[Insert Table 1 here.]

Next, we collect data of corporate litigation from the CSMAR database. The CSMAR database archives material court cases for all Chinese listed firms. As required by Shanghai and Shenzhen stock exchanges, listed firms must disclose their involvement in a case if the stake is over either RMB 10 million or 10% of a firm's net assets. Firms should also disclose any cases that would have a significant impact on a company's securities even when the stake is below the two prescribed thresholds. As a result, the mandatory disclosure requirement allows us to track all cases of materiality which are filed against listed firms. The CSMAR database keeps track of the development of each case and provide detailed information on involved firm's security ID, event date, stage of the litigation (first instance or second instance with the relevant judgment by the Supreme People's court), the relevant plaintiff or defendant and its relationship with the firm, types and reasons of cases, size of stake, level of the court the case is tried, case status, judgment, etc. Because the main focus of our paper is the firm-level litigation risk, we first merge the case data with the matched-sample firm data using security ID and year of the first filing date of a case, then aggregate case information of each firm within a year (e.g., whether the firm is sued, and if so, the total number of cases, total amount of stakes, etc.) at the firm-year level. In our empirical analysis throughout

this paper, we focus exclusively on listed firms that are sued as the *defendants* in legal disputes to assess the causal impacts of political connections on corporate litigation risk.

Panel A of Table 2 presents the summary statistics of the key variables of the study. There are 5,785 firm-year observations in total from 2009 to 2018. We report the raw DID estimates based on the matched sample in Panel B of Table 2. As we can see, before Rule 18, there is no significant difference across all the four measures of litigation risk among the treated and control firms. The results confirm that our matching procedure yields highly comparable treatment and control samples which have similar levels of litigation risk before Rule 18. In contrast, we notice that the four measures *increase* after the disruption to political connections and the changes are statistically significant at 1% level. These findings are already indicative of potential partiality and bias rooted in the judiciary due to corporate political connections. Moreover, the preliminary evidence supports the hypothesis that the reduction in judicial favoritism as a result of the depoliticization reform leads to more frequent use of courts by the disadvantaged groups to resolve disputes with the politically connected firms.

[Insert Table 2 here.]

5 Empirical Results

5.1 The Effects of Rule 18 on Corporate Litigation

We assess the causal impacts of the removal of politician-directors on corporate litigation by performing the DID tests on our matched sample. The results are presented in Table 3. In models (1)-(4) of Panel A, we adopt four different measures of litigation risk as the independent variables, and include firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects to control for unobserved heterogeneities. We find strong evidence that firms which lose their

government-related directors are no longer favored by the judicial system as before and thus experience higher litigation risk. Put differently, the depoliticization regulation has encouraged the disadvantaged groups to use the courts more often as a venue to resolve legal disputes with the politically connected firms. As shown in Column (1) of Panel A, after the enforcement of Rule 18, compared with the control firms, the treatment firms whose political power is substantially weakened are 10.4% more likely to be sued. Column (2) shows that, on average, the number of cases filed against a connected firm rises by 0.23 per year. Columns (3) and (4) suggest that the total monetary amount involved in cases against a treatment firm per year also becomes larger in terms of its absolute and relative magnitudes. From Column (4), such increase represents about 0.2% of the total assets of an average firm in our sample. Taken together, these findings manifest the *social costs* associated with judicial corruption and reflect an *ex ante* inefficient equilibrium where politically connected firms accused of wrongdoings enjoy protection and favoritism while less connected counterparties suffer from their inability to seek justice and receive fair and impartial treatment in the judicial process.

Next, we examine the dynamic effects of Rule 18 on corporate litigation which also helps us to test the parallel trends assumption of the DID strategy. We create eight indicator variables: *Year 2010*, *Year 2011*, *Year 2012*, *Year 2013*, *Year 2014*, *Year 2015*, *Year 2016*, and *Year 2017*$^{+}$, which are set to one in the relevant years.[4] We interact them with the treatment dummy *Rule*, 18_i to gauge the impacts of Rule 18 in each year. We show that the treatment effects map out in the post-treatment period instead of the pre-treatment period, while the magnitude of coefficients prior to the treatment is small and statistically insignificant. The results are presented in Panel B of Table 3. Indeed, the treatment and control firms share similar pre-treatment trends in all the four measures of litigation risk before 2014 in that the estimates are negligible and insignificant during

[4] $Year2017^{+}$ is set to one if an observation is in either 2017 or 2018 and zero otherwise.

the pre-treatment period. Further, the effects of Rule 18 kick in from 2014 which is consistent with our expectation because Rule 18 was announced near the end of 2013 and was entirely unanticipated before its enactment. The treatment effects start to grow and become significant from 2014. They have remained sizable and persistent since 2015, reflecting that the effects of depoliticization on treatment firms' litigation risk are not only strong but also long-lasting.

[Insert Table 3 here.]

5.2 Cross-sectional Analysis

In this subsection, we employ different cross-sectional tests to assess the heterogenous effects of the mandated resignations of government officials due to Rule 18. We exploit cross-sectional variation in different firm and regional characteristics to estimate the difference-in-difference-in-differences (DDD) regression models. These tests serve the following purposes. First, they provide evidence on which firms are more affected by the disruption of political connection in the judicial process, and hence provide support for the underlying mechanisms. Second, it is possible that unobserved trends in measures of litigation risk and/or other unobserved factors may affect firms that do and do not suffer from the weakening of political connections differently. By identifying firms in treatment group that are more likely to be affected by the enforcement of Rule 18 and comparing groups of firms sharing certain common features, the DDD estimator can help alleviate these concerns.

State ownership. It is well documented that the ownership structure of Chinese list firms is highly concentrated, and every public firm has one large (controlling) shareholder (e.g., Jiang and Kim, 2020). We investigate whether the identity of the controlling shareholders influence the intensity of the treatment effects. The government usually have ultimate control over SOEs. Compared with non-SOEs, SOEs often have easier access to government resources and are less regulated. As

government employees, judicial officials have the incentive to act favorably towards SOEs. On the contrary, non-SOEs must first establish political connections to mitigate any potential bias then seek assistance from the government in legal disputes. Accordingly, we hypothesize that the impacts of Rule 18 will be more pronounced for non-SOE firms once they lose political connections. In panel A of Table 4, we interact $Rule18_i * Post_t$ with the $Non\text{-}SOE_{it}$ indicator variable. As shown in the table, most of the aforementioned DDD estimates are statistically significant at conventional levels which support our hypothesis that politician-directors play a more important role in interfering in judicial affairs with their political power for non-SOEs. Interestingly, the coefficients on $Rule18_i * Post_t$ are also significant, implying that politically connected directors can also help SOEs to further reduce litigation risk.

Financial distress. Financial distress is crucial in understanding the cross-sectional relationship between defendant, plaintiff, and their combined wealth effects (Bhagat et al., 1994). Financially distressed firms are unable to cover current obligations such as unpaid expenses to suppliers and missed principal or interest payments under borrowing agreements (Wruck, 1990). Therefore, they can be damaged more severely by lawsuits against them (Firth et al., 2011). Firms with political ties are more likely to be bailed out by the government when they encounter financial difficulties (Faccio, 2006), which helps them to avoid lawsuits with stakeholders. In this vein, we anticipate that, once financially distressed firms are no longer covered by the government, their risk of being sued will go up dramatically. We use Altman's Z-score as a measure of financial distress (Altman, 1983) and define a firm to be financially distressed or close to it if the firm's pre-treatment average Z-score is below the median of the pre-treatment average Z-scores of all sample firms. As shown in Panel B of Table 4, we find that the litigation risk based on the four measures we have chosen is indeed more salient for connected firms which are in deeper financial distress compared with less financially distressed treatment firms, although the litigation risk of the latter also increases.

Legal protection. In regions with weak legal institutions where government agencies often do not strictly follow the procedural rules prescribed by the Administrative Litigation Law of China, the space for political power to manipulate the legal proceedings can be large. Thus, we expect that the treatment effects of depoliticization should be stronger for firms in regions with weak legal institutions. We employ the legality score from the widely used Fan Gang marketization index (MI) for all provinces in China as a measure of the quality of legal institutions (Fan et al., 2010). We define a firm to be operating in a province with weak legal institution if the score is below the sample median. The results are presented in Panel C of Table 4. Treatment firms operating in regions with weak legal institutions are more likely to be sued than treatment firms in regions with strong legal institutions. Also, they encounter larger increases in both the cases filed against them and the total litigation stakes per year than those of the treatment firms with better legal environments, although the latter group of firms also experience more severe litigation risk after the enactment of Rule 18. In general, the results show that while Rule 18 is effective in curbing judicial corruption across different parts of China, the judicial system is more susceptive to political interference in regions with weak legal institutions.

[Insert Table 4 here.]

5.3 Type of Cases

In this subsection, we categorize the cases in our case sample based on their types and merge the sample with the firm-year panel data. We re-estimate the DID model to assess the impacts of Rule 18 on changes in the probability and number of cases of each type. CSMAR provides detailed information about the reasons and classifications of the lawsuits. We categorize cases according to the type definitions in Regulation of Reasons for Civil Lawsuits, which was modified in 2011 based

on General Principles of Civil Law, Property Law, Contract Law, Tort Law, and Civil Procedure Law.[5] We end up with nine different types of cases: Contract Disputes, Company Disputes, Labor Disputes, Intellectual Properties Disputes, Unfair Competition Disputes, Guarantee Disputes, Bankruptcy Disputes, Securities Disputes, and Others which are cases not classified in CSMAR.[6] The distribution of the types of the cases are reported in Panel A of Table 5. We then run eight different regressions (excluding the type 'Others') to examine if the disruption of political connections affects the probability and number of cases for each type.[7]

[Insert Table 5 here.]

Panel B of Table 5 reports the results of the linear probability model. For each dependent variable, it is set to one if a firm encounters at least one case of the relevant type filed against it and zero otherwise. We find that the disruption of political connections results in higher probabilities of being sued for contract disputes, company disputes, labor disputes, intellectual property disputes, and unfair competition disputes. The results imply that firms were able to reduce the litigation risk associated with disputes of these types by utilizing their political power to create barriers to judicial justice before the reform. The enforcement of Rule 18 renders the legal system more independent and impartial, thus the less politically empowered parties are more likely to take legal action to resolve these disputes with previously connected firms. Nevertheless, the litigation risk remains unchanged for bankruptcy disputes, guarantee disputes, and securities disputes. The insignificant result concerning bankruptcy disputes may be due to the small number of bankruptcy cases in our sample period. Also, listed firms often create many employment opportunities, pay huge amounts of taxes, and contribute significantly to local GDP. Therefore, the local government has an incentive

[5] See http://www.court.gov.cn/fabu-xiangqing-3456.html for more details on case classifications.
[6] We categorize note disputes to Contract Disputes because bank notes are a specific form of debt contract.
[7] Like before, we use the linear probability model to assess the changes in the probability of cases of each type.

to bail out the firms and discourage them from declaring bankruptcy. Moreover, guarantee disputes usually involve other parties besides the connected firm as the guarantors and the relational liability is often clearly defined. Hence, the space for manipulation with the firm's political power is rather limited in these disputes. Finally, China lacks a mature legal procedure to deal with securities class actions as found in the U.S., thus making it relatively hard for investors to defend their legal rights in court for securities disputes despite the weakening of the listed firms' political connections. Panel C of Table 5 reports the results of the changes in the number of cases based on case types due to Rule 18. We find that the numbers of cases goes up for contract disputes, company disputes, labor disputes, and intellectual property disputes. In contrast, the case numbers are not significantly affected by the reform for unfair competition disputes, bankruptcy disputes, guarantee disputes, and securities disputes. The evidence on change in the numbers of cases of each type is broadly consistent with our previous findings on the risk of being sued for disputes of certain types.

5.4 Plaintiffs' Litigation Outcomes

We have already shown that the disruption of political connections leads to greater corporate litigation risk. We next examine whether the disruption of political connections leads to more plaintiffs winning the cases in this subsection. We exclude the following from our cases sample: cases that are not brought to court, cases that are filed but subsequently withdrawn, and cases without information about their outcomes. We go through all documents related to each case and use the first instance judgment provided by CSMAR, and define a plaintiff's success as being awarded monetary benefit in a court case (Lu et al. 2015). We then match the first instance judgments to our firm-year panel data and create four variables similar to our baseline regressions. $Plaintiff\ Win_{it}$ is an indicator variable which takes one if firm i encounters at least one lawsuit as a defendant in which the plaintiff wins the case at first instance in year t. $Case\ Number\ (Plaintiff\ Win)_{it}$ is the

total number of cases against firm i where the plaintiffs win at first instance in year t. $Damages_{it}$ is the logarithm of one plus the total amount of damages paid by firm i in cases where the plaintiffs win at first instance in year t. $Damages\ (\%)_{it}$ is the ratio of the total amount of stakes of cases against firm i where the plaintiffs win at first instance to a firm's total assets in year t. We estimate the same DID and dynamic effect models as in Section 5.1. The results are reported in Table 6. From Column (1)-(2) of Panel A, it becomes 7.6% more likely to observe incidences of a plaintiff winning a case against a politically connected firm, and on average there are 0.105 more plaintiff-winning cases against a politically connected firm following the enforcement of Rule 18. Moreover, from Column (3)-(4) of Panel A, the total monetary amount of damages paid in these winning cases also becomes larger, and the increase accounts for 0.092% of an average firm's total assets after the reform. As shown in Panel B of Table 6, the dynamic effects appear to have the same patterns as those in Panel B of Table 3 in that both the treatment and control firms share the same pre-treatment trends in the four measures of case outcomes while the effects of Rule 18 kick in and start to grow and become significant from 2014. These findings further corroborate the evidence on the existence of the judicial bias and show that curbing the abuse of political power not only materializes the potentially high litigation risk of the politically connected firms, but also leads to more favorable court outcomes for the plaintiffs.

[Insert Table 6 here.]

5.5 Robustness Tests

5.5.1 Placebo Tests

To further validate our finding that the enforcement of Rule 18 causes higher litigation risk for the politically connected firms, we perform two types of placebo tests. First, we set the year of 2011 as

the pseudo treatment year. We create a dummy variable $Post(2011)_t$ which takes one from 2011 to 2013, and zero from 2008 to 2010. We re-estimate the DID model for all the four measures of litigation risk and find that all the coefficients are close to zero and insignificant. The results are presented in Panel A of Table 7. Second, Rule 18 requires that both government and party officials who are serving on the boards of the listed firms must resign immediately. Our paper so far only has only considered government officials who are equipped with strong political power. Nevertheless, party officials who are affiliated with universities, public organizations, and SOEs and hired by listed companies as independent directors also have civil ranks, although their political power is rather limited. As a result, their resignations should not have any material impact on firms' political power and thus will not affect the litigation risk. We use firms with less relevant official directors as the pseudo-treatment firms and repeat the same PSM procedure to construct a matched sample. We create a dummy variable $Non\text{-}official_i$ that equals one if a firm experiences resignations of the official directors who are politically less important and zero otherwise. The results are presented in Panel B of Table 7. Again, we do not find any economically meaningful and statistically significant result, suggesting that the aggravation of the litigation risk is indeed driven by the disruption of political connections.

[Insert Table 7 here.]

5.5.2 Controlling for Potential Confounding Events

One potential concern could be that other policies in China's recent anti-corruption campaign launched in December 2012 by President Xi Jinping may drive our results. The campaign formulated an Eight-point Regulation document to combat corruption, which requires government officials to forego conspicuous perks and be grassroots-oriented. After the issuance of the docu-

ment, listed firms substantially cut their business entertainment expenses which used to be the main source of government officials' perks. These expenses are considered a key measure of the severity of corruption (Lin et al., 2017; Giannetti et al., 2021). To control for the potential impact of the anti-corruption campaign, we follow Hope et al. (2020) by including either business entertainment expenses (*BEE*) or cash payment for business entertainment expenses (*BECP*) as an additional control variable and perform the DID tests again. From Panels A and B of Table 8, controlling for business entertainment expenses does not change our conclusions.

In addition, the anti-corruption campaign could also mitigate the judicial biases and cut corruption within the entire legal system, which may drive our results. To alleviate such a concern, we include another control variable *Arrested Judges*, which is the number of judges arrested due to corruption-related reasons each year in the prefecture-level city where each firm is located. Corporate lawsuits are usually first tried at the Intermediate People's Courts in the city where the defendant is located, so controlling for the number of arrested judges can account for the effect of the anti-corruption campaign on the legal system of the city (Giannetti et al., 2021). As shown in Panel C of Table 8, the magnitudes and significance of the four DID estimates remain unchanged compared with our previous results. Therefore, we alleviate the concern that our results may be driven by other possible confounding events.

[Insert Table 8 here.]

5.5.3 Short Estimation Windows

In our previous analysis, the DID models employ a ten-year window, and one might suspect that there could be other contemporaneous shocks that drive our results within the same period. In this section, we estimate the same regression models with a four-year window from 2012 to 2015 and a

six-year window from 2011 to 2016. The results are presented in Table 9. In sum, no conclusions are altered in these tests with shorter windows.

[Insert Table 9 here.]

5.5.4 DID Tests with Different Matching Procedures

We re-estimate our DID models with four different matching procedures. First, we implement the PSM method with replacement: we allow the same control firm to be matched to more than one treatment firm. Second, we employ an alternative matching method based on the normalized Euclidean distance computed using the key firm characteristics considered in Appendix B with replacement. Third, we simply match control firms to treatment firms based on industry and size with replacement. Finally, we simply use all listed firms except those that are affected by Rule 18 as our matched control firms. We present the DID estimates of the four measures of litigation risk with four different matched samples in Table 10. In general, we conclude with strongly robust findings and our inferences hold again.

[Insert Table 10 here.]

6 Conclusions

Using a combination of firm-level and case-level datasets in the world's largest emerging economy, we provide comprehensive evidence that curbing the abuse of political power corrects an inefficient equilibrium where firms with politically connected directors minimize their supposedly high litigation risk while the disadvantaged counterparties suffer from their inability to receive fair and impartial treatment in the judicial process.

We document that politically connected firms are at greater risk of being sued and being involved in more cases with higher stakes against them after a negative shock to their political ties. The effects of Rule 18, while being significant on all firms that hire government officials as their independent directors, are more pronounced for non-SOEs, firms that are in financial distress or close to it, and firms in regions with weaker legal institutions. Overall, plaintiffs fare better in lawsuits against the politically connected firms after the reform.

Our findings highlight the importance of going beyond case- and correlation-based studies of the role of political connections in corporate litigation (e.g., Firth et al., 2011; Lu et al., 2015). In these papers, it is to be expected that politically connected firms receive preferential treatment in the courts. In contrast, our paper not only confirms their findings with a causal interpretation, but also uncovers a neglected negative externality of corporate government linkage in that it can deter the stakeholder, be it a supplier, customer, lender, employee, or competitor, from using the court to seek justice and resolve disputes with a politically connected firm. We show that, after the enactment of the depoliticization regulation, litigation is becoming an increasingly common method for the stakeholders to resolve conflicts with firms that used to possess strong political power. In general, besides contributing towards a better characterization and understanding of the evolving legal environment in China, our analysis also has important implications for policymakers in emerging economies with weak legal institutions.

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Table 1: Treatment Sample Construction Process

All independent director's resignations for all non-financial firms listed on Shanghai and Shenzhen Stock exchanges from Oct 2013 to Dec 2016	2,131
Less: Resignations in firms that went public after 2014	175
Less: Voluntary resignations (e.g., personal reasons, health reasons, career reasons) without political connection	949
Less: Resignations citing Rule 18 with directors from universities, public organizations and SOEs	467
Politically connected independent directors' mandatory resignations that are influenced by Rule 18	540
Firms with resigned official directors (For some firms, there were multiple resignations)	368
Less: firms with missing value of variables or no matched control firms	32
Treatment firms	336

This table describes the sample construction process of the treatment firms. We manually collect director resignation news from public reports from 19/10/2013 to 31/12/2016. We then collect the background information for each director in CSMAR and conduct an extensive web search if the information is missing.

Table 2: Descriptive Statistics

Panel A: Summary Statistics for Key Variables

Variables	Obs.	Mean	SD	Min	Median	Max
Sued	5,785	0.083	0.276	0.000	0.000	1.000
Case Number	5,785	0.161	0.666	0.000	0.000	5.000
Litigation Stake	5,785	0.240	0.947	0.000	0.000	5.431
Litigation Stake (%)	5,785	0.132	0.792	0.000	0.000	6.663
Plaintiff Win	5,785	0.049	0.216	0.000	0.000	1.000
Case Number (Plaintiff Win)	5,785	0.067	0.313	0.000	0.000	2.000
Damages	5,785	0.127	0.662	0.000	0.000	4.605
Damages (%)	5,785	0.053	0.387	0.000	0.000	3.498
Rule No.18	5,785	0.510	0.500	0.000	1.000	1.000
Post	5,785	0.527	0.499	0.000	1.000	1.000
Size	5,785	22.313	1.447	19.218	22.149	26.230
Leverage	5,785	0.509	0.214	0.060	0.520	1.009
ROA	5,785	0.033	0.061	-0.202	0.029	0.230
Sales Growth	5,785	0.204	0.582	-0.613	0.105	4.091
PPE	5,785	0.260	0.185	0.003	0.223	0.744
Sales Volatility	5,785	0.124	0.140	0.005	0.080	0.863
Cash Flow	5,785	0.043	0.075	-0.195	0.041	0.258
Tobin's Q	5,785	2.082	1.414	0.924	1.573	8.466
Annual Return	5,785	0.171	0.666	-0.706	-0.015	2.792
Listed Age	5,785	2.408	0.595	0.775	2.586	3.219
Insider Holdings	5,785	0.062	0.145	0.000	0.000	0.632
Board Size	5,785	2.192	0.209	1.609	2.197	2.708
Board Independence	5,785	0.374	0.056	0.300	0.357	0.571
Largest Shareholder	5,785	0.366	0.153	0.110	0.341	0.759
SOE	5,785	0.551	0.497	0.000	1.000	1.000

Panel B: Univariate Analysis

	Before			After			Diff-in-Diff
	Control Group	Treat-ment Group	Diff	Control Group	Treat-ment Group	Diff	
Sued	0.039	0.038	-0.002	0.071	0.173	0.102***	0.104***
Case Number	0.084	0.056	-0.029	0.135	0.345	0.209***	0.238***
Litigation Stake	0.110	0.089	-0.021	0.186	0.538	0.352***	0.373***
Litigation Stake(%)	0.088	0.049	-0.039	0.109	0.265	0.156***	0.195***

This table presents descriptive statistics of the matched sample. Panel A reports the summary statistics of the key variables used in the paper, based on a sample of 5,785 firm-year observations during 2009-2018. We winsorize all variables except indicator variables (*Sued*, *Rule 18*, *Post*, and *SOE*) at the 1st and 99th percentiles to mitigate the impacts of outliers. Panel B provides comparison between treatment firms and control firms and the raw DID estimates for the main litigation risk measures. See Appendix A for variable definitions. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3: Political Connections and Corporate Litigation
Panel A: Baseline Regressions

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule No.18*Post	0.104***	0.229***	0.372***	0.196***
	(0.016)	(0.038)	(0.054)	(0.046)
Size	-0.006	-0.063*	-0.020	-0.069
	(0.014)	(0.034)	(0.046)	(0.047)
Leverage	0.161***	0.487***	0.517***	0.446**
	(0.048)	(0.132)	(0.174)	(0.181)
ROA	-0.235**	-0.675**	-1.012**	-1.201**
	(0.106)	(0.268)	(0.410)	(0.470)
Sales Growth	0.002	0.011	0.019	0.036
	(0.008)	(0.022)	(0.027)	(0.026)
PPE	0.055	0.045	0.064	0.036
	(0.057)	(0.124)	(0.183)	(0.161)
Sales Volatility	0.093*	0.216*	0.319**	0.314
	(0.053)	(0.121)	(0.153)	(0.215)
Cash Flow	-0.075	-0.152	-0.215	-0.237
	(0.060)	(0.155)	(0.210)	(0.200)
Tobin's Q	0.005	-0.002	0.017	0.055**
	(0.006)	(0.013)	(0.020)	(0.025)
Annual Return	-0.009	0.007	-0.034	-0.049*
	(0.011)	(0.024)	(0.032)	(0.029)
Listed Age	0.018	0.047	-0.044	-0.088
	(0.030)	(0.072)	(0.099)	(0.079)
Insider Holdings	-0.147	-0.378	-0.182	-0.156
	(0.100)	(0.281)	(0.343)	(0.340)
Board Size	0.044	-0.000	0.161	-0.059
	(0.042)	(0.098)	(0.137)	(0.127)
Board Independence	-0.008	-0.121	-0.391	-0.517
	(0.131)	(0.314)	(0.425)	(0.385)
Largest Shareholder	-0.159**	-0.220	-0.388	-0.233
	(0.071)	(0.175)	(0.247)	(0.220)
SOE	-0.015	-0.021	-0.089	-0.094
	(0.029)	(0.071)	(0.101)	(0.098)
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.179	0.231	0.166	0.181

Panel B: Dynamic Effects

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Year2010	0.006	0.028	-0.000	-0.037
	(0.024)	(0.050)	(0.074)	(0.062)
Rule 18*Year2011	-0.025	-0.010	-0.012	-0.009
	(0.025)	(0.056)	(0.083)	(0.071)
Rule 18*Year2012	0.006	0.042	0.080	0.030
	(0.024)	(0.060)	(0.084)	(0.072)
Rule 18*Year2013	-0.001	0.010	0.006	0.029
	(0.027)	(0.059)	(0.092)	(0.083)
Rule 18*Year2014	0.027	0.098	0.197*	0.131
	(0.030)	(0.065)	(0.109)	(0.085)
Rule 18*Year2015	0.098***	0.236***	0.366***	0.197**
	(0.032)	(0.076)	(0.110)	(0.092)
Rule 18*Year2016	0.125***	0.276***	0.440***	0.250**
	(0.034)	(0.077)	(0.121)	(0.108)
Rule 18*Year2017^{+}	0.130***	0.309***	0.478***	0.221**
	(0.030)	(0.073)	(0.106)	(0.092)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.182	0.232	0.166	0.180

This table presents the results from the regression analysis of the relationship between political connections and litigation risk. Panel A reports the results of the DID tests. Panel B reports the estimates of the dynamic effects of Rule 18 on treatment firms' litigation risk. The dependent variables in Panel A and B are *Sued*, an indicator variable which takes one if at least one lawsuit is filed against the firm in a year, *Case Number*, which is the total number of lawsuits filed against the firm in a year, *Litigation Sake*, which is the logarithm of one plus the total monetary amount involved in litigations against the firm in a year, and *Litigation Stake (%)*, which is the ratio of total monetary amount involved in litigations against the firm to its total assets in a year, and is expressed in percentage term. *Rule 18* is a treatment dummy variable which takes one for firms with resigned government official directors due to Rule 18 and zero otherwise. In Panel B, *Year2010*, *Year2011*, *Year2012*, *Year2013*, *Year2014*, *Year2015*, and *Year2016* are dummy variables which are set to one in the relevant years and zero otherwise. *Year2017*$^{+}$ is set to one in 2017 and 2018 and zero otherwise. In our matched sample, for each treatment firm, we identify the control firm using nearest neighbor matching without replacement based on the estimated propensity score. All controls are lagged by one year. See Appendix A for variable definitions. All regressions include firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Cross-sectional Tests

Panel A: Ownership Structure

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Post*Non-SOE	0.055*	0.125*	0.149	0.239***
	(0.028)	(0.070)	(0.096)	(0.079)
Rule 18*Post	0.080***	0.174***	0.306***	0.091*
	(0.020)	(0.045)	(0.066)	(0.048)
Non-SOE	0.000	-0.014	0.047	0.027
	(0.030)	(0.071)	(0.101)	(0.099)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.180	0.232	0.166	0.183

Panel B: Financial Distress

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Post*FD	0.061**	0.184***	0.241***	0.215***
	(0.025)	(0.061)	(0.087)	(0.069)
Rule 18*Post	0.066***	0.115**	0.222***	0.062
	(0.019)	(0.045)	(0.062)	(0.046)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.181	0.233	0.167	0.183

Panel C: Legal Protection

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Post*Weak	0.094**	0.187**	0.350***	0.231**
	(0.037)	(0.093)	(0.125)	(0.105)
Rule 18*Post	0.080***	0.181***	0.282***	0.137***
	(0.019)	(0.044)	(0.061)	(0.050)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.181	0.232	0.167	0.182

This table presents the results of our cross-sectional tests. In our matched sample, for each treatment firm, we identify the control firm using nearest neighbor matching without replacement based on the estimated propensity score. Panel A presents the effect heterogeneity based on state ownership, panel B presents the effect heterogeneity based on degree of financial distress, and panel C presents the effect heterogeneity based on strength of legal institutions. The dependent variables in Panel A, B and C are *Sued*, an indicator variable which takes one if at least one lawsuit is filed against the firm in a year, *Case Number*, which is the total number of lawsuits filed against the firm in a year, *Litigation Sake*, which is the logarithm of one plus the total monetary amount involved in litigations against the firm in a year, and *Litigation Stake (%)*, which is the ratio of total monetary amount involved in litigations against the firm to its total assets in a year, and is expressed in percentage term. *Rule 18* is a treatment dummy variable which takes one for firms with resigned government official directors due to Rule 18 and zero otherwise. *Post* is a year dummy variable which equals one for post-treatment years from 2014 to 2018 and zero otherwise. In Panel A, *Non-SOE* is a dummy variable which equals one if the firm is not an SOE, and zero otherwise. In Panel B, *FD* is a dummy variable which equals one if a firm's pre-treatment average Z-score is below the median of the pre-treatment average Z-scores of all sample firms, and zero otherwise. In Panel C, *Weak* is a dummy variable which equals one if a firm is in a province whose legality score from Fan Gang Index is below the median of the average legality scores of all provinces in the pre-treatment period, and zero otherwise. All controls are lagged by one year. See Appendix A for variable definitions. All regressions include firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Types of Cases
Panel A. Distribution of Cases

First-level Case Types	Examples	Number
Unfair Competition Disputes	Disputes over counterfeiting, false advertising, bundling sales, and commercial secrets infringement.	84
Company Disputes	Disputes over company incorporation, transfer of shares, earnings distribution, related transaction etc.	82
Bankruptcy Disputes	Disputes over recognition of bankruptcy claims and recall rights etc.	16
Labor Disputes	Disputes over wages, pension, medical care, benefits etc.	46
Contract Disputes	Breaches of contract include commercial disputes over the delivery, pricing, quality, ownership of goods and services, real estate sales, loan, debt, bank cards, rental etc., and disputes about bank drafts, promissory note, and cheques.	1,177
Guarantee Disputes	Disputes over collateral and pledge.	45
Intellectual Properties Disputes	Disputes over copyrights, trademark, patent, technology transfer, franchise etc.	43
Securities Disputes	Disputes about stocks, corporate bonds, treasuries, mutual funds, derivatives, underwriting etc.	130
Others	Unclassified by CSMAR	144
Total	-	1,767

Panel B: Probability of Litigation based on Case Types

	(1) *Contract Disputes*	(2) *Company Disputes*	(3) *Labor Disputes*	(4) *Intellectual Properties Disputes*	(5) *Unfair Competition Disputes*	(6) *Guarantee Disputes*	(7) *Bankruptcy Disputes*	(8) *Securities Disputes*
Rule 18*Post	0.070***	0.011**	0.008**	0.015***	0.008*	0.002	0.001	0.004
	(0.012)	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785	5,785	5,785	5,785	5,785
Adjusted R^2	0.161	0.037	0.082	-0.003	0.023	0.043	-0.010	0.043

Panel C: Number of Cases based on Case Types

	(1) *Contract Disputes*	(2) *Company Disputes*	(3) *Labor Disputes*	(4) *Intellectual Properties Disputes*	(5) *Unfair Competition Disputes*	(6) *Guarantee Disputes*	(7) *Bankruptcy Disputes*	(8) *Securities Disputes*
Rule 18*Post	0.302***	0.016**	0.008**	0.017***	0.003	0.004	0.001	-0.038
	(0.082)	(0.007)	(0.004)	(0.004)	(0.008)	(0.003)	(0.003)	(0.045)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785	5,785	5,785	5,785	5,785
Adjusted R^2	0.126	0.030	0.030	-0.005	0.004	0.058	-0.010	0.194

This table presents the distribution of case types and the results of the regression analysis on the effects of political connection disruption on the probability of being sued and the number of cases for each case type. Panel A presents the distribution of case types. Panel B presents the results of the linear probability model for changes in probability of each case type. Panel C presents the results on changes of the number of cases for each case type. In Panel B, the independent variables are an indicator variable which takes one for each specific type of cases. In Panel C, the independent variables are the total number of cases which belong to a specific type of cases. *Rule 18* is a treatment dummy variable which takes one for firms with resigned government official directors due to Rule 18 and zero otherwise. *Post* is a year dummy variable which equals one for post-treatment years from 2014 to 2018 and zero otherwise. All controls are lagged by one year. See Appendix A for variable definitions. All regressions include firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Lawsuit Outcomes

Panel A: Disruption to Political Connections and Lawsuit Outcomes

Variables	(1) *Plaintiff Win*	(2) *Case Number (Plaintiff Win)*	(3) *Damages*	(4) *Damages (%)*
Rule 18*Post	0.076***	0.105***	0.240***	0.092***
	(0.012)	(0.017)	(0.037)	(0.023)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.169	0.188	0.121	0.125

Panel B: Dynamic Effects of Disruption to Political Connections on Lawsuit Outcomes

Variables	(1) *Plaintiff Win*	(2) *Case Number (Plaintiff Win)*	(3) *Damages*	(4) *Damages (%)*
Rule 18*Year2010	0.008	0.014	0.012	-0.043
	(0.021)	(0.028)	(0.058)	(0.039)
Rule 18*Year2011	-0.017	-0.004	-0.011	-0.009
	(0.020)	(0.027)	(0.052)	(0.034)
Rule 18*Year2012	-0.009	-0.004	0.039	0.003
	(0.019)	(0.028)	(0.055)	(0.040)
Rule 18*Year2013	-0.011	-0.013	-0.001	-0.008
	(0.021)	(0.029)	(0.063)	(0.042)
Rule 18*Year2014	0.016	0.037	0.151**	0.053
	(0.022)	(0.030)	(0.070)	(0.040)
Rule 18*Year2015	0.082***	0.122***	0.285***	0.114**
	(0.026)	(0.037)	(0.078)	(0.051)
Rule 18*Year2016	0.084***	0.114***	0.243***	0.080*
	(0.027)	(0.036)	(0.080)	(0.048)
Rule 18*Year2017^{+}	0.082***	0.120***	0.286***	0.082*
	(0.023)	(0.033)	(0.074)	(0.046)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.170	0.188	0.121	0.124

This table presents the results of regression analysis on the effects of political connection disruption on litigation outcomes. The dependent variables in Panel A and B are *Plaintiff Win*, a dummy variable which takes one if the firm experiences at least one litigation as the defendant in which the plaintiff wins at first instance in a year, and zero otherwise, *Case Number (Plaintiff Win)*, which is the number of litigations the firm experiences as a defendant in which the plaintiff wins at first instance in a year, *Damages*, which is the logarithm of one plus the total monetary amount of damages paid by the firm in cases where the plaintiffs win at first instance in a year, and *Damages (%)*, which is the ratio of the total monetary amount of damages paid by the firm in cases where the plaintiffs win at first instance to a firm's total assets in a year, and is expressed in percentage terms. *Rule 18* is a treatment dummy variable which takes one for firms with resigned government official directors due to Rule 18 and zero otherwise. In Panel A, *Post* is a year dummy variable which equals one for post-treatment years from 2014 to 2018 and zero otherwise. In Panel B, *Year2010, Year2011*, *Year2012*, *Year2013*, *Year2014*, *Year2015*, and *Year2016* are dummy variables which are set to one in the relevant years and zero otherwise. *Year2017*$^{+}$ is set to one in 2017 and 2018 and zero otherwise. All controls are lagged by one year. See Appendix A for variable definitions. All regressions include firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Robustness: Placebo Tests

Panel A: Pseudo Treatment Year

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Post(2011)	0.001	0.000	0.011	0.006
	(0.015)	(0.021)	(0.046)	(0.032)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	3,144	3,144	3,144	3,144
Adjusted R^2	0.168	0.197	0.135	0.222

Panel B: Pseudo Treatment Group

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Non-official*Post	-0.004	-0.000	0.005	0.005
	(0.013)	(0.019)	(0.034)	(0.009)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,419	5,419	5,419	5,419
Adjusted R^2	0.140	0.173	0.160	0.153

This table presents the results of the placebo tests. Panel A presents the results using 2011 as the pseudo treatment year for a sample period of 2008 to 2013. Panel B presents the results using a pseudo treatment group that do not possess strong political power together with a matched control group. The dependent variables in Panel A and B are *Sued*, an indicator variable which takes one if at least one lawsuit is filed against the firm in a year, *Case Number*, which is the total number of lawsuits filed against the firm in a year, *Litigation Sake*, which is the logarithm of one plus the total monetary amount involved in litigations against the firm in a year, and *Litigation Stake (%)*, which is the ratio of total monetary amount involved in litigations against the firm to its total assets in a year, and is expressed in percentage term. In panel A, *Rule 18* is a treatment dummy variable which takes one for firms with resigned government official directors due to Rule 18 and zero otherwise. *Post(2011)* is a dummy variable which takes one from 2011 to 2013, and zero from 2008 to 2010. In Panel B, *Non-official* is a dummy variable which takes one for firms with resigned party officials with limited political power, who are affiliated with universities, public organizations, and SOEs, due to Rule 18 and zero otherwise. *Post* is a year dummy variable which equals one for post-treatment years from 2014 to 2018 and zero otherwise. All controls are lagged by one year. See Appendix A for variable definitions. All regressions include firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Robustness: Controlling for Potential Confounding Events

Panel A: Entertainment Expenses

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Post	0.104***	0.232***	0.375***	0.201***
	(0.016)	(0.038)	(0.054)	(0.046)
BEE	3.315	13.205**	15.235**	20.654**
	(2.204)	(6.062)	(7.129)	(8.897)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.180	0.233	0.167	0.184

Panel B: Entertainment Cash Payments

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Post	0.104***	0.231***	0.374***	0.199***
	(0.016)	(0.038)	(0.054)	(0.045)
BECP	0.066	0.293**	0.329**	0.394***
	(0.040)	(0.128)	(0.134)	(0.145)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.181	0.235	0.168	0.187

Panel C: Number of Judges Arrested

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Post	0.103***	0.228***	0.371***	0.196***
	(0.016)	(0.038)	(0.054)	(0.045)
Arrested Judges	-0.023	-0.068	-0.030	-0.020
	(0.031)	(0.074)	(0.106)	(0.080)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	5,785	5,785	5,785	5,785
Adjusted R^2	0.179	0.231	0.166	0.181

This table presents the results of the robustness tests which control for potential confounding events in the anti-corruption campaign. Panel A presents the results with the inclusion of entertainment expenses. Panel B presents the results with the inclusion of entertainment cash payments. Panel C presents the results with the inclusion of the number of arrested judges each year in a prefecture-level city. See Appendix A for variable definitions. The dependent variables in Panel A and B are *Sued*, an indicator variable which takes one if at least one lawsuit is filed against the firm in a year, *Case Number*, which is the total number of lawsuits filed against the firm in a year, *Litigation Sake*, which is the logarithm of one plus the total monetary amount involved in litigations against the firm in a year, and *Litigation Stake (%)*, which is the ratio of total monetary amount involved in litigations against the firm to its total assets in a year, and is expressed in percentage term. *Rule 18* is a treatment dummy variable which takes one for firms with resigned government official directors due to Rule 18 and zero otherwise. *Post* is a year dummy variable which equals one for post-treatment years from 2014 to 2018 and zero otherwise. In panel A, *BEE* is measured as a firm's business entertainment expenditures scaled by sales per year. In Panel B, *BECP* is measured as a firm's cash payment related to business entertainment expenses scaled by sale per year. In Panel C, *Arrested Judges* is the number of judges arrested due to corruption-related reasons each year in the prefecture-level city where each firm is located. All controls are lagged by one year. See Appendix A for variable definitions. All regressions include firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Robustness: Short Window Estimation

Panel A: Four-year Window

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Post	0.057***	0.135***	0.239***	0.146***
	(0.020)	(0.045)	(0.066)	(0.049)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	2,460	2,460	2,460	2,460
Adjusted R^2	0.207	0.341	0.201	0.261

Panel B: Six-year Window

Variables	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
Rule 18*Post	0.090***	0.195***	0.319***	0.188***
	(0.018)	(0.042)	(0.061)	(0.048)
Controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry*Year FE	Yes	Yes	Yes	Yes
Province*Year FE	Yes	Yes	Yes	Yes
Observations	3,587	3,587	3,587	3,587
Adjusted R^2	0.197	0.303	0.168	0.219

This table presents the results of regression analysis using shorter estimation windows. Panel A employs a four-year window from 2012 to 2015 and Panel B employs a six-year window from 2011 to 2016. The dependent variables in Panel A and B are *Sued*, an indicator variable which takes one if at least one lawsuit is filed against the firm in a year, *Case Number*, which is the total number of lawsuits filed against the firm in a year, *Litigation Sake*, which is the logarithm of one plus the total monetary amount involved in litigations against the firm in a year, and *Litigation Stake (%)*, which is the ratio of total monetary amount involved in litigations against the firm to its total assets in a year, and is expressed in percentage term. *Rule 18* is a treatment dummy variable which takes one for firms with resigned government official directors due to Rule 18 and zero otherwise. *Post* is a year dummy variable which equals one for post-treatment years from 2014 onwards and zero otherwise. All controls are lagged by one year. See Appendix A for variable definitions. All controls are lagged by one year. See Appendix A for variable definitions. All regressions include firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 10: Robustness: Difference-in-Differences Estimations with Different Matching Procedures

Matching Procedure	(1) *Sued*	(2) *Case Number*	(3) *Litigation Stake*	(4) *Litigation Stake (%)*
PSM 1:1 with replacement	0.117***	0.258***	0.415***	0.252***
	(0.017)	(0.039)	(0.056)	(0.058)
Normalized Euclidean Distance 1:1 matching with replacement	0.121***	0.229***	0.391***	0.194***
	(0.016)	(0.034)	(0.052)	(0.038)
Industry + Size 1:1 matching with replacement	0.107***	0.235***	0.394***	0.234***
	(0.016)	(0.034)	(0.056)	(0.041)
All listed firms unaffected by Rule 18 as a control sample	0.113***	0.208***	0.383***	0.191***
	(0.013)	(0.025)	(0.044)	(0.030)

This table presents the results of the robustness tests which re-estimate the DID models with different matching procedures. All regressions include a set of covariates, firm fixed effects, industry-by-year fixed effects, and province-by-year fixed effects as in our baseline regressions. Standard errors clustered at the firm level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix A: Key Variable Definition

Variables	Definition
Dependent Variables	
Sued	A dummy variable which takes one if at least one lawsuit is filed against the firm in a year, and zero otherwise.
Case Number	The total number of lawsuits filed against the firm in a year.
Litigation Stake	Logarithm of one plus the total monetary amount involved in litigations against the firm in a year.
Litigation Stake (%)	Ratio of total monetary amount involved in litigations against the firm to its total assets in a year (expressed in percentage terms).
Plaintiff Win	A dummy variable which takes one if the firm experiences at least one litigation as the defendant in which the plaintiff wins at first instance in a year, and zero otherwise.
Case Number (Plaintiff Win)	The number of litigations the firm experiences as a defendant in which the plaintiff wins at first instance in a year.
Damages	The logarithm of one plus the total monetary amount of damages paid by the firm in cases where the plaintiffs win at first instance in a year.
Damages (%)	The ratio of the total monetary amount of damages paid by the firm in cases where the plaintiffs win at first instance to a firm's total assets in a year, in percentage terms.
Control Variables	
Size	Logarithm of one plus the total book assets of a firm in a year.
Leverage	Ratio of current and long-term debt to total assets in a year.
ROA	Ratio of net income to total assets in a year.
Sales Growth	The annual growth rate of the total sales in a year.
PPE	Ratio of property, plant, and equipment to total assets in a year.
Sales Volatility	Ratio of standard deviation of sales in past three years to total assets in a year.
Cash Flow	Ratio of operating cash flow to total assets in a year.
Tobin's Q	Sum of market value of A shares, the market value of B shares, (total number of outstanding shares – A shares – B shares)*(Owner's equity/Paid-in capital), and total liabilities, scaled by total assets in a year, as provided in CSMAR.
Annual Return	Daily returns compounded over a year.
Listed Age	Logarithm of one plus the cumulative listing years.
Insider Holdings	Share proportion of insiders in a year.
Board Size	Logarithm of one plus the total number of board directors in a year.
Board Independence	Proportion of independent directors in the board in a year.
Largest Shareholder	Share proportion of the largest shareholder in a year.
SOE	Dummy variable which equals one if the firm is an SOE in a year, and zero otherwise.
Additional Independent Variables	
Financial Distress	Dummy variable which equals one if a firm's pre-treatment average Z-score is below the median of the pre-treatment average Z-scores of all sample firms, and zero otherwise.
Weak	Dummy variable which equals one if a firm is in a province whose legality score from Fan Gang Index is below the median of the average legality scores of all provinces in the pre-

	treatment period, and zero otherwise. (China Market Index Database)
BEE	Business entertainment expenditures scaled by sales in a year.
BECP	Cash payment related to business entertainment expenses scaled by sales in a year.
GDP per capita	GDP per capita of the province where the firm locates in a year.
Industry Sales Growth	The annual growth rate of the total sales for the industry which the firm belongs to in a year.

Appendix B: Logit Model for Estimating the Propensity Scores

	(1) *Forced Resignation*
Size	0.074
	(0.066)
Leverage	0.815**
	(0.374)
ROA	-0.732
	(1.331)
Sales Growth	0.040
	(0.100)
PPE	0.431
	(1.13)
Sales Volatility	0.503
	(0.453)
Cash Flow	-0.762
	(-0.941)
Tobin's Q	0.084
	(0.063)
Annual Return	-0.478**
	(0.235)
Listed Age	0.040
	(0.125)
Insider Holdings	-0.002
	(1.000)
Board Size	1.241***
	(0.363)
Board Independence	3.778***
	(1.215)
Largest Shareholder	0.502
	(0.422)
SOE	0.075
	(0.150)
GDP per capita	-0.048
	(0.031)
Industry Sales Growth	0.026
	(0.021)
Observations	2,230
Pseudo R^2	0.039
LR Chi^2	73.830

This table presents the results from the logit model. Robust standard errors are reported in parentheses. The independent variable *Forced Resignation* is set to one if a firm has at least one government official resigned due to Rule 18 and zero otherwise. All the independent variables use observations in 2012, which is one year prior to the announcement of Rule 18. See Appendix A for variable definitions. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix C: Covariates Balance

Variables	Before Matching			After Matching		
	Treatment (N=368)	Control (N=2058)	Diff.	Treatment (N=336)	Control (N=336)	Diff.
Size	22.068	21.739	0.329***	22.173	22.107	0.066
Leverage	0.496	0.423	0.073***	0.507	0.499	0.008
ROA	0.034	0.045	-0.010***	0.031	0.030	0.001
Sales Growth	0.164	0.166	-0.002	0.168	0.141	0.026
PPE	0.245	0.219	0.026***	0.253	0.248	0.005
Sales Volatility	0.133	0.112	0.021***	0.126	0.130	-0.004
Cash Flow	0.034	0.041	-0.007*	0.038	0.038	-0.001
Tobin's Q	1.703	1.696	0.008	1.680	1.765	-0.085
Annual Return	0.008	0.031	-0.023	0.005	0.000	0.004
Listed Age	2.150	1.994	0.155***	2.243	2.241	0.001
Insider Holdings	0.097	0.139	-0.042***	0.086	0.086	0.000
Board Size	2.203	2.158	0.045***	2.205	2.200	0.005
Board Independence	0.375	0.370	0.005*	0.374	0.374	0.000
Largest Shareholder	0.379	0.361	0.017**	0.378	0.381	-0.004
SOE	0.511	0.389	0.122***	0.530	0.500	0.030

The table reports the before- and after-matching mean differences in firm characteristics of the control and treatment firms in 2012 (one year before the issuance of Rule 18). See Appendix A for variable definitions. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.