Punish One, Teach A Hundred: The Sobering Effect of Peer Punishment on the Unpunished*

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Abstract

Direct experience of a peer's punishment might have a sobering effect above and beyond deterrence (information about punishments). We test this mechanism in China studying the reactions to listed state-owned enterprises' (SOEs) punishments for fraudulent loan guarantees by firms in the same location or industry (peers) and non-peer firms, across SOEs and non-SOEs. After experiencing SOEs' punishments, peer SOEs cut their loan guarantees by more than non-peer SOEs and peer non-SOEs, even if information is common to all firms. The reaction is stronger for peer SOEs whose CEOs have higher career concerns or face lower costs of cutting guarantees. Managers' overreaction to the salience of a peer's punishment could be an important channel of transmission of the spillover effects of punishments on firms' outcomes.

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I Introduction

From Ancient Rome to Mao's China, philosophers have argued that the salience of a peer's punishment might have a sobering effect on the behavior of non-punished peers,[1] above and beyond the mere deterrence effect that information about punishment has on both peer and non-peer agents. According to this mechanism, peers update their beliefs about the probability of being punished and the effects of punishment more than non-peers, who have the same information about the punishment, because peers experience both dimensions saliently through their stronger relationship with those punished (e.g., Gao, Kleiner, and Pacelli (2020); Goldman and Zeume (2021); Agarwal, Qian, and Zhou (2021); Zeume and Slutzky (2023)).

In this paper, we propose a setting to test if expert decision makers — managers of Chinese listed firms, which comply with regulation more than private firms (e.g., Slutzky (2020)) — behave in line with this governance mechanism. We focus on punishments of fraudulent loan guarantees rather than other wrongdoings that are more common in the US, such as accounting fraud, not only because corporate misconduct in loan guarantees has become one the most prominent types of minority-shareholder expropriation in China, but also because it allows us to measure peer's reaction to punishment in a homogeneous way.[2]

In China, public firms that head business groups often use intra-group transfers and loan guarantees to alleviate the financial constraints of private related parties, who can barely access credit from banks.[3] On top of relaxing financial constraints, loan guarantees can also be used to tunnel resources from the minority shareholders of public companies to private related parties. In this mechanism, a private related party takes a loan from a bank despite a high risk of default. In case of default, minority shareholders' wealth is dissipated, whereas controlling shareholders gain from the private party's operations through their stake in it. Over the last two decades, Chinese regulators have tackled the punishment of fraudulent related-party transactions (Jiang, Lee, and Yue (2011)), and the

[1] In Latin, "Unum castigabis centum emendabis." A similar prescription stating "Punish One, Teach a Hundred" is often attributed to Mao Zedong.

[2] Every Chinese publicly listed firm is mandated to disclose all related party transactions in financial statements. These disclosure requirements were put in place on July 1, 1997 by a Ministry of Finance directive, and the enforcement of such disclosure was further strengthened in 2004.

[3] For anecdotal evidence, see "Illegal loan guarantees hurt Chinese publicly listed firms, regulators should pay attention to 'transactions under the table'" (*Securities Daily*, February 27, 2022).

tests in this paper exploit such punishment events to study the reaction of non-punished peers of the punished firms.[4]

Testing for our proposed mechanism while controlling for local shocks that correlate with punishments is challenging. Documenting the reactions of all non-punished firms before and after a peer's punishment would confound any shocks that correlate with the peer's punishment. We therefore need an empirical strategy that does not rely on the implausible assumption that punishments are randomly allocated across space and over time. Ideally, we would compare two firms that operate in the same location, at the same time, and hence are exposed to the same local time-varying shocks, but one of which is more prone than the other to update its beliefs after observing the punishment event because it has a stronger peer relationship with the punished firm.

We argue the Chinese setting allows for such a research design. Chinese listed firms include both state-owned enterprises (SOEs) — whose controlling shareholder is the central or local governments — and non-SOEs. Government ownership insulates SOEs more than non-SOEs from traditional governance mechanisms in both developed and developing economies (e.g., Huang, Pagano, and Panizza (2020); Ding, Levine, Lin, and Xie (2021)). We thus compare the reactions to peer punishments by peer SOEs relative to peer non-SOEs, which face the same local time-varying shocks that potentially triggered the punishments, as well as to non-peer SOEs, which face the same governance implications of government ownership as peer SOEs. Crucially, all these firms (whether peers or not) access the same information about punishment events, which is made public by regulators and the media.

One might still worry that, despite being exposed to the same local time-varying shocks, the unobservables that determined the punishment of a local SOEs might increase the likelihood of punishment of other local SOEs more than that of local non-SOEs and for this reason local SOEs react more to the same punishment. We therefore also propose a second empirical strategy in which we only consider the reactions of peer SOE. Within this group, we exploit variation in the extent to which CEOs have an incentive to react. This strategy can be interpreted as the estimate of an intensive margin of the transmission of peer effects through salient punishments. Inspired by Gao, Kleiner, and Pacelli (2020),

[4]As we discuss in more detail below, these punishments started more than a decade before the anti-corruption campaign of 2013. We will show directly that our results do not change if we only focus on events and reactions up to 2013 and thus before the anti-corruption campaign started.

we consider SOE CEOs' career concerns as proxied by CEOs' distance from the legally-mandated retirement age or CEOs' involvement in establishing existing loan-guarantee contracts when the peer is punished.[5]

In the data, both SOEs and non-SOEs are punished due to fraudulent loan guarantees. We first show that the timing of punishments is neither predicted by a large set of observables that capture whether local governments want to avoid SOE punishment nor by indices capturing the development of local SOEs, local product markets, and financial markets.

To define pre- and post-punishment periods, we focus on the *first* punishments of SOEs in each Chinese prefecture, which should be the most salient relative to subsequent punishment events. These punishments mostly happened well before the Chinese anti-corruption campaign started (2013) and we show that our results are unrelated to the anti-corruption campaign (e.g., Agarwal, Qian, Seru, and Zhang (2020)).[6]

Information about punishments is made public by the China Securities Regulatory Commission (CSRC) and covered by local and national media. Information is thus the same for everybody and under the standard deterrence argument studied in the literature all firms should react similarly to punishments of the same size, same gravity, and generally displaying similar characteristics, irrespective of whether they are peers of the punished firms. One might argue that it is difficult to separate the peer effect from the information-acquisition effect (e.g., Liu (2022)), because SOE local peers might pay more attention to information about punishments. However, it is unlikely that Chinese non-SOE firms, who often strive to survive from policy changes as a result of government intervention, are not attentive to the public enforcement of securities regulation.[7]

In our baseline analysis, we define peers based on geography, because geographic peers face the same institutional settings, the same local business cycle shocks, the same local regulators that are responsible for a large fraction of the punishments we exploit, and the same business culture and local norms (e.g., Barrios et al. (2022)). Nevertheless, we show that our results are similar if we define peers based on industries to capture firms that operate in the same product markets and potentially interact with the same suppliers and

[5]In a different setting, Gao, Kleiner, and Pacelli (2020) find that corporate credit events impose disciplining consequences on bankers for structuring poorly performing corporate loans.

[6]Our results are unchanged if we exclude the few years and events in our sample that happen on and after 2013, when the anti-corruption campaign was conceived and implemented.

[7]We thank Miao Liu for this suggestion.

customers. Empirically, geography and industry capture largely independent transmission channels of peer effects, because in horse-race tests both dimensions are relevant to predict the reaction to peers' punishments.

When we implement our first design, we find that after Chinese regulators punish a listed SOE for tunnelling via inter-corporate loan guarantees, unpunished local (peer) SOEs cut their (legal or fraudulent) loan guarantees to related parties significantly more relative to non-local SOEs and local non-SOEs. This effect is economically and statistically significant: peer SOEs reduce their loan guarantees over total assets by 2.4 percentage points — about 25% of a standard deviation of the scaled loan guarantees in the sample. Instead, peer non-SOEs and SOEs in other locations barely react.

These baseline results survive a set of robustness tests, such as excluding the largest Chinese prefectures and cities from the analysis, focusing on localities that experienced at least one punishment during our sample period, as well as fixing the SOE status of firms at the time in which they experienced the punishment of a local peer. This last test is important because China faced a massive wave of SOE privatization in the 2000s, and hence the SOE status of a substantial portion of the firms in our sample varies over time.

We then implement our second research design, which only exploits variation in the incentives to react within peer SOEs. Following Jiang, Wan, and Zhao (2015), we use CEOs' age as a proxy for CEOs' careers concerns. These earlier papers show that age is a meaningful proxy for career concerns in China, because of mandatory retirement at age 60 for men and 55 for women.[8] SOE CEOs who are closer to retirement age face weaker career concerns relative to younger SOE CEOs. Older CEOs might face restrictions to their retirement packages if punished. Younger CEOs face, on top of the same restrictions to retirement packages, also negative shocks to their career up to retirement age. The stakes of facing punishment are thus substantially larger for younger SOE CEOs than for older SOE CEOs.

We find that peer SOEs run by CEOs within two years of their mandated retirement age react less than those run by younger CEOs to the punishment of the same peers in the same locations and at the same time. Importantly, the likelihood that firms headed by young or old CEOs are punished is similar, even within the subsample of SOEs.

We then consider the timing of peer SOE's CEO appointments. CEOs who have been

[8]We also account for the fact that central-government employees can retire three years later than the mandatory age for their gender.

appointed long before the peer's punishment were likely involved in setting up the loan guarantees in place at the time of the peer's punishment, whereas CEOs who have been appointed shortly before are more likely to face loan guarantees to related parties that were established by previous executives. For this reason, CEOs with a longer tenure in the firm should find it more costly to cut existing loan guarantees relative to newer CEOs, who can always blame their predecessors. And, indeed, we find that recently-appointed peer SOE CEOs react economically and statistically more than other peer SOE CEOs.

As discussed above, the salience of direct exposure to peers' punishment should be relevant also along peer dimensions different from geography, such as industry links. Consistently, in horse races we find that *both* geographic and industry proximity determine a stronger reaction to SOE peers' punishment by SOEs than non-SOEs, and these effects are largely unrelated to each other.

A prominent channel alternative to the salience of peers' punishments is that local politicians might engage in moral suasion to convince local peer SOEs to cut loan guarantees after the first punishments are implemented in the locations in which they operate. Against this possibility, our results are similar for SOEs under the control of the State-owned Assets Supervision and Administration Commission of the State Council (SASAC), which has a direct objective to improve the corporate governance of SOEs.

Unfortunately, in most cases the authorities do not indicate a precise size of the fine or other punishments they imposed. To assess whether peers still react more while keeping constant proxies for the severity of punishments, we propose using the cumulative abnormal returns (CARs) of punished peers' stock prices in the days around the punishment announcement, which capture the market's assessment of the size of the punishment in terms of its overall impact on the punished firm's equity value. A larger drop in CARs is likely to signal harsher consequences of punishment, and this informational content is the *same* for all firms, whether peers or not, who observe the punished firm's stock prices. We find that peer SOEs react more than others after both higher and lower shocks, but differences in reactions are higher after punishments that result in more negative CARs for punished firms.

In the third part of the paper, we assess whether peers' loan-guarantee cuts have far-reaching effects for them and their related parties. This question has no obvious answer ex ante. On the one hand, peer SOEs might arrange for alternative and more opaque channels

to allow their related parties to access external debt, which would neuter the effect of cutting loan guarantees,[9] such as not disclosing loan guarantees altogether. Contrary to this possibility, we verify that related parties' access to external debt unambiguously drops after peer SOEs cut their loan guarantees. We also find that peer SOEs cut their investment and obtain higher total factor productivity (TFP) over time, which suggests that peers' punishments might sober agency problems above and beyond the cut of loan guarantees to related parties.

Cost effectiveness is an interesting feature of peer punishment as a corporate governance mechanism, because regulators only need to monitor and punish a small set of firms to obtain broad compliance, which reduces dramatically the costs of monitoring by regulators and activist shareholders. Peer punishments could be especially viable in settings in which other forms of governance are ineffective and/or too costly (e.g., Allen, Qian, and Qian (2005)). Punishing the wrongdoing of one firm could reduce the scope for misbehavior by peers without the need of monitoring or investigating them directly.

A Related Literature

Our paper contributes to several strands of literature in accounting, finance, and political economy. First, we relate to work studying the causes and consequences of managerial wrongdoing (e.g., Dyck, Morse, and Zingales (2010); Zeume (2017); Bennedsen and Zeume (2017); Gao, Kleiner, and Pacelli (2020)) and whether mechanisms different from formal, binding contracts can reduce the scope for wrongdoing (e.g., D'Acunto, Xie, and Yao (2021)).[10] In this paper, we study the indirect effects of sanctions on *non-punished* firms instead of quantifying the direct and indirect effects on those punished. Closest in spirit to our analysis is Goldman and Zeume (2021), who show that anti-bribery enforcement actions for violators of the U.S. Foreign Corrupt Practices Act in non-OECD countries can improve performance of firms operating in the same industry and country. Unlike the deterrence effect that we document, Goldman and Zeume (2021) document a "leveling the playing field" effect — with violators of anti-bribery regulation being removed, nonbribing firms can compete more fairly.

We also contribute to the recent literature on the effects of salience on decision-

[9] We thank Stefan Zeume for raising this possibility.

[10] Earlier work has documented that firms might learn from events that happen to other firms such as lawsuits (e.g., Gande and Lewis (2009); Arena and Julio (2015)).

making. Theories exist explaining how the salience of environmental characteristics affects economic decision-making with and without risk (Gennaioli and Shleifer (2010); Bordalo, Gennaioli, and Shleifer (2012); Bordalo, Gennaioli, and Shleifer (2013)). Researchers in economics and finance have also employed the salience of environmental characteristics in experimental and field settings to test for the effects of such characteristics on individual decision-making (e.g., Benjamin, Choi, and Strickland (2010); D'Acunto (2018); D'Acunto (2017); D'Acunto, Malmendier, Ospina, and Weber (2021)). Dessaint and Matray (2017) are the first to test for overreaction to salient events in corporate finance. They find managers accumulate cash holdings to insure their firms against disaster risk after observing the effects of a natural disaster on firms close by, which increases managers' expected probability of disasters through salience of disaster risk. Managers then dissipate these precautionary cash accumulations over time, which suggests they overreacted to the salient events. In our setting, both Bayesian and non-Bayesian reactions to peers' punishment might help explain the results, although the stronger SOEs' reaction to more salient than to less salient events might be consistent with non-Bayesian updating.

Our paper departs from the large literature in accounting and finance on the deterrence role of punishments (for instance, see Desai, Dyck, and Zingales (2007) and Chalfin and McCrary (2017) for recent comprehensive reviews), which is based on the premise that mere information about the consequences of punishment sobers firms' behavior. For example, Gleason, Jenkins, and Johnson (2008) and Goldman, Peyer, and Stefanescu (2012) show that financial market participants also punish competitors of firms that engage in financial misconduct in the sense that these firms also realize negative returns around the days in which accusations of financial misreportings are announced for other firms in the same industry. The mechanism we study in this paper suggests that above and beyond the deterrence effect of information about the consequences of punishment, informed firms who are closer peers of those punished — for instance, because they operate in the same locations or in the same industry — react more to the same punishment events relative to similarly-informed firms for whom the events are less salient, given the lack of close connection to those punished. The second important way in which we depart from the literature is that we study firms' own actions after punishments rather than the reactions of financial-market participants through stock-price movements.

Our results also speak to research that documents the spillover effects of wrongdoing

and/or improved governance across firms through interlocked boards (e.g., Bizjak, Lemmon, and Whitby (2009); Gopalan, Gormley, and Kalda (2018); Ding, Lin, Schmid, and Weisbach (2021); Ding, Lin, Schmid, and Weisbach (2023)), geographic proximity (e.g., Parsons, Sulaeman, and Titman (2018)), regulators' preferences (e.g., Kedia and Rajgopal (2011)), and shareholder activism (e.g., Gantchev, Gredil, and Jotikasthira (2019))). Whereas most of this research studies transmission mechanisms related to the exchange of information across firms, which is facilitated by direct connections, we study the effects of the *salience* of events that are public and about which all firms, irrespective of their location, see the same public information. Punishments as governance mechanisms are cheap because they do not require the collection and elaboration of private information and could be applied in developing countries, in which shareholder activism is not highly diffused due to the absence of large institutional investors in equity markets.

We also relate to the large body of work on corporate governance mechanisms in the presence of large shareholders (e.g., Faccio and Lang (2002)) and their effects on corporate outcomes. Recent examples include wolf-pack activism (e.g., Brav, Dasgupta, and Mathews, 2017) and shareholder coalitions (e.g., D'Acunto, 2016). Governments as blockholders are common in emerging markets as well as in firms that belong to strategic industries such as energy, defense, and aerospace.[11] We contribute to the body of research studying settings in which governments own productive resources (e.g., Bortolotti and Faccio (2008)) and in which political connections are valuable to firms (e.g., Faccio, 2006). For the case of China, the increasing availability of data has expanded the scope of this area of research over the last few years.

II Institutional Setting

In this section, we discuss two important features of our institutional setting. First, the process through which SOEs emerged in China, which is important for our first research design that compares SOEs with non-SOEs but less so for our second research design, which only compares the differential behavior across SOEs. Second, we describe the prevalence of loan guarantees to private related parties by Chinese public firms, which are ubiquitous to increase access to capital from banks to related private firms.

[11] Megginson (2017) surveys the literature on state ownership of businesses and D'Souza, Megginson, Ullah, and Wei (2017) study the performance of privatized firms.

A SOEs and Business Groups in China

The Chinese government imposed the transition to a market economy in several stages since 1978. A crucial tenet of the transition was an approach known as "dual-track liberalization" and "reform without losers" (e.g., Lin (2009); Xu (2011); Yoon (2021)). Under this approach, SOEs were allowed to keep operating alongside private businesses. To maintain direct control over the economy but allow for liberalization, the Chinese government developed a system labeled "networked hierarchy," which consists of vertically-integrated corporate groups that are organized by the State-Owned Assets Supervision and Administration Commission of the State Council (SASAC).

In the networked hierarchy, upstream sectors were still organized as SOE-controlled monopolies, whereas smaller and downstream SOEs were allowed to become private, thus initiating the so-called "grasp the large, let go the small" approach to privatization. The characteristics of SOEs and non-SOEs have been converging in at least two aspects. First, both types of firms are often connected to local governments and exploit such connections to ease financial constraints. Second, both types of firms have been introducing innovations such as basic forms of corporate governance.

In 1992, the Chinese government started the second stage of economic reforms. Downstream sectors faced a large-scale privatization wave. In the early 2000s, some upstream SOEs also started to be gradually privatized. Between 2001 and 2004, after China's access to the World Trade Organization (WTO), the number of firms that were SOEs dropped by 48%. In 2005, China's private sector was for the first time larger than the public sector (e.g., Engardio, 2005).

Differences between SOEs and non-SOEs have not completely disappeared over time. Throughout the second stage of economic reforms, surviving SOEs reinforced their monopoly power in upstream sectors, which are generally nontradable or regulated sectors, which was possible because surviving SOEs were protected from foreign competition even after access to the WTO. By contrast, non-SOEs faced more competition in downstream tradables sectors, which were open to foreign entry.

To date, most SOEs have only faced an incomplete restructuring process. They are typically organized into a parent/subsidiary structure, in which the most profitable part of the firm was carved out for public listing, whereas the parent company kept the excess workers, obsolete plants, and the financial and social liabilities of existing companies.

Through the incomplete restructuring process, the government-owned shares were in the hands of the SOE parent company that became the controlling shareholder.

The differences between SOEs and non-SOEs suggest that, despite a slow convergence of the characteristics of these two types of firms, SOEs and non-SOEs are likely to face different incentives to react to governance threats. We exploit these differences in our first empirical design.

B Loan Guarantees to Related Parties

The Chinese banking system is controlled through the (non-independent) central bank (People's Bank of China, PBOC) and the China Banking Regulatory Commission (CBRC). The banking system consists of four dominant actors — the four largest state-owned commercial banks, which primarily lend to large firms — and smaller banks. As regulators, practitioners, and academics have widely recognized, loans to SOEs by the major Chinese banks account for the largest part of the non-performing loans in China.[12]

To access capital from banks, smaller and private related parties often use loan guarantees from larger and typically listed firms, which is in contrast to other countries, where governments often use guarantees to finance small businesses or support homeownership (e.g., D'Acunto, Tate, and Yang (2018)). Loan guarantees, though, can also be used as a means to expropriate minority shareholders in listed firms. [13] For instance, suppose a private party related to the majority shareholder(s) of a listed company asks for a guarantee to obtain a loan and finance a wasteful project that produces private benefits to the majority shareholder(s) of the listed company. Because of the nature of the project, though, the loan might default. In this case, *all* the shareholders of the listed company will suffer losses and not just the majority shareholder(s).

In August 2003, the CSRC issued a notice to regulate guaranteed loans provided by public firms.[14] According to the notice, firms should adhere to the following criteria when guaranteeing for their related parties. First, the amount of guarantees provided by a public firm cannot exceed 50% of its net worth. Second, public firms are not allowed to

[12]For how legal environments shape lending behavior of banks in emerging economies, see Haselmann and Wachtel (2010) and Haselmann, Pistor, and Vig (2010).

[13]Fisman and Wang (2010) describe the mechanisms through which Chinese corporations tunnel resources to related private parties through loan guarantees.

[14]See "Notice on several issues concerning regulating transactions between listed companies and their related parties and the external loan guarantees by listed companies".

provide guarantees for borrowers whose leverage ratio exceeds 70%. Third, public firms cannot guarantee related companies or natural persons in which they hold less than 50% of shares. Last, the guarantee should be approved by at least two-thirds of directors in the board meeting or be approved in the shareholder meeting.

Before 2007, SOEs were the most frequent users of loan guarantees to related parties. Since 2007, the central government has urged banks to expand lending to small firms. Because the cost of doing due diligence is high relative to the value of a small loan, banks usually insist on a guarantee in the absence of sufficient collateral. Indeed, even after 2007, more than a quarter of Chinese loans are backed by guarantees (McMahon, 2014).

III Data

We employ several data sources that cover listed and private firms in China.

A Punishment Events

We manually identify all the fraud events related to loan guarantees for private related parties of listed firms from the CSRC's *Enforcement Action Research Database*, which is part of the *China Stock Market and Accounting Research* (CSMAR) database. CSMAR gathers detailed information about corporate frauds involved with public firms listed on the Shanghai Stock Exchange and Shenzhen Stock Exchange from a variety of sources, which include CSRC public announcements, information firms under investigations make public, and newspaper articles. The time period for our analysis is 1997–2018.

The CSRC's *Enforcement Action Research Database* collects and standardizes the information regarding fraud events from press releases as well as from other official regulatory documents. Figure A.1 reports one such press release. The punished company is Xiang Jiugui (Hunan Drunkard), which is a liquor producer. The company provided guarantees to its controlling shareholders without the approval of the shareholder annual meeting or the board of directors. Because of this violation, the company was fined for an overall amount of 0.4 million RMB. The penalty also included a targeted punishment to the chairman of the board of directors, who faced a fine of 50,000 RMB and received a warning letter from the central CRSC. Other board members (as listed in the case) also received warning letters. These personally targeted punishments have reputational and

career-related costs for those punished.

The punishment-level information, which is publicly announced and available, includes the date on which a punishment for a firm committing fraud is announced, the regulator that announced the fraud event (national or local), the time period during which the fraud was committed, the reasons for the punishment, the extent of the punishment, and a detailed description of the activities in which the listed company engaged. Unfortunately, though, not all information details are available for all punishment events.

Although anecdotal evidence shows that the first fraudulent event the CSRC punished in China dates back to October 20, 1994, only a handful of fraud cases related to loan guarantees were detected and punished before 2000. We classify fraud events as related to loan guarantees if either the fraud database cites loan guarantee misconduct as at least one of the reasons for punishment or the description of the fraudulent activities includes the word "guarantee." Over the entire sample period (1997-2018), we detect 276 corporate fraud events involving irregular loan guarantees in which public firms and their related parties are involved.[15]

Four different agencies can implement punishments: the central CSRC, the province-level offices of the CSRC, and the two stock exchanges in mainland China — Shenzhen and Shanghai. Out of the 276 punishments we observe, the local CSRC offices implemented 39.5%, the central CSRC 17.8% and the remaining are equally split across the stock exchanges. Contrary to widespread beliefs, SOEs do get punished. Out of all 276 punishments, 40% involve SOEs. When we only consider the first punishment in each prefecture, 60% involve SOEs, which is prima facie evidence that not only listed SOEs are routinely punished but they also are more likely to be the first local punished firms relative to non-SOEs.

[15]For comparison, we also summarize the number of other fraud events over the same sample period. As for fraud events that possibly indicate governance issues, we detect 147 events regarding with earnings manipulation, 219 regarding controllers misappropriating firm assets, and 91 regarding controllers changing the prescheduled budget plans without shareholder approval. As for fraud events revealing improper disclosure practices, we detect 1,149 events regarding managers omitting material information and 756 regarding managers fabricating information to mislead investors. In addition, we also detect 105 fraud events regarding with insider trading.

A.1 Characteristics of Punishments

Figure 1 describes the spatial distribution of the 72 first punishment events for loan guarantee wrongdoing. The map plots Chinese prefectures, which represent the level at which we define geographic peers based on firm headquarters' locations. In the top map of Figure 1, the darker is a prefecture, the earlier is the first punishment event for loan-guarantee wrongdoing of a local listed firm in that prefecture. We observe substantial spatial variation in the timing of the first punishments. Moreover, no substantial spatial clustering of the timing of first punishments seems detectable in the map, which suggests that concerns about spatial correlation across observations in neighboring prefectures is barely relevant in our context.

In terms of the distribution of the events over time, we also fail to detect clustering in specific years: the proportion of punished firms each year is roughly constant, and each year about 40% of punished firms are SOEs. Moreover, 70% of the first punishments in a prefecture happened before 2012, which is when the anti-corruption campaign implemented under the Xi presidency started. Already in the main sample, therefore, the punishment events we study are largely unrelated to the anti-corruption campaign. To dismiss any remaining concerns about the anti-corruption campaign, we show that our results barely change if we end the sample in 2012.

As we discuss below, none of our research designs require the (implausible) assumption that the timing of punishment events is randomly assigned across prefectures. Our designs instead exploit the differential incentives to react to peer punishments across different types of firms that face the same contemporaneous local demand and supply shocks (e.g., D'Acunto, Liu, Pflueger, and Weber (2018); Gu and Xie (2023)). If we wanted to interpret our results in a causal way, we would need to assume that the punishment events are exogenous conditional on observables and unobservables related to the local economy.

To assess the extent to which local observables predict the emergence of punishments at the prefecture-year level, in Table A.1 we consider a panel of prefecture-year observations. We regress a dummy variable that equals 1 if the prefecture-year had a punishment of a SOE and zero otherwise, on a large set of potential local drivers of punishments. We select these prefecture-year and province-year observables based on earlier research. Specifically, we consider the following variables at the prefecture-year

level: logarithm of GDP, employment rate, logarithm of population, population density, share of GDP in heavy manufacturing and light manufacturing, prefecture-level fiscal deficit, the logarithm of the number of public firms operating in the prefecture-year, and the share of SOEs as a percentage of all firms in the prefecture-year. The following variables are instead available at the province-year level: an index of the strength of the government-market relationship, an index of the development of non-SOE firms, an index of the development of local product markets, an index of the development of local input markets, and an index of the development of local financial intermediation (Gao, Ru, and Tang (2017)). Our sources for the province-year-level data are Fan, Wang, and Zhu (2011) and Fan, Wang, and Wen (2016). We fail to detect any systematic associations between this set of observables and the emergence of punishment events at the prefecture level.

Finally, we assess whether subsequent punishment events within prefectures are clustered in time — for instance, because the regulator decided to target a prefecture and its firms for political or strategic reasons — in Table A.2. We use a dummy for whether a prefecture faced a punishment in year t, to predict the likelihood the same prefecture would face a punishment in subsequent years $t + n$ after conditioning on prefecture-level time-varying observables. We find that the emergence of a punishment in a prefecture is if anything negatively related to the emergence of another punishment in the same location in the first two years after the first punishment and statistically and economically unrelated from the third year onwards.

B Firm-Level Information

Our main source for firm-level variables is the CSMAR database, which contains balance-sheet information and other accounting variables, ownership structure, outstanding bank loans, and financial-fraud events sanctioned by the market authority for all Chinese listed firms. We exclude firms with distress identification (coded as "ST" firms) and firms cross-listed in B- or H-share stock market.

We use the information in CSMAR to construct all the accounting-based observables we use in the analysis for our sample of listed firms, including the establishment and IPO years, total assets, total and long-term liabilities, fixed assets, cash, operating sales, net income, and Tobin's Q. CSMAR also reports the identities of public firms' controlling shareholders and ultimate owners. It also indicates whether the controlling shareholder,

or ultimate owner, is state-owned or not. We manually read the names of shareholders to further verify their identities and double-check their government or private nature.

We extract information on public firms' location through company addresses in the IPO filing. We manually read these addresses and group them into categories at the level of prefecture. Excluding the two special administrative regions (Hong Kong and Macau), the administrative partitions of China consist of several levels: the provincial (province, autonomous region, and municipality), the prefecture, county, township, and the village. If firms' provincial classifications fall into province and autonomous region, we choose the prefecture-level city to identify firm location. For firms located in the four municipalities (i.e., Beijing, Shanghai, Tianjin and Chongqing), we identify their location at the provincial level. For firms located in autonomous counties and banners in China, we treat them as the same level as the prefecture. In the rest of the paper, we refer to the geographic level at which we group peers as the prefecture.

In each year, public firms disclose names and relations of all their related parties to public investors. We rely on Orbis Asia-Pacific for financial information on private related parties. Orbis collects companies' filed accounts from the Chinese Administration of Industry and Commerce, the National Tax Bureau, and the National Bureau of Statistics of China (NBSC). It includes 26 million active companies in mainland China. We extract company financial statements from Orbis from 2005 to 2018. We extract the names of related parties from the CSMAR related-party transaction database and use a string-matching algorithm (supplemented by manual work) to match them to private firms.

To track the direction and amount of guarantees either provided or received by public firms, we rely on disaggregated related-party transaction data from the *China Listed Firm's Related Party Transactions Research Database*, which we access via CSMAR.

Moreover, we download bank loan data at the disaggregated level from the *China Listed Companies Bank Loan Research Database*, which is also available through CSMAR. The database provides detailed information on loan characteristics based on company announcements for the period 1996–2018. From this dataset, we are able to obtain comprehensive information on each loan announced by listed companies, such as loan amount, interest rate, loan maturity, loan starting and ending date, identity of the originator, whether the loan was guaranteed by a third party, and the purpose of the loan.

B.1 Properties of the Firm-Level Data

The bottom map of Figure 1 describes the spatial distribution of the firms in our sample. The darker is a prefecture, the higher is the number of firms in that prefecture. The firms that enter our analysis are evenly distributed throughout China, which ensures our results do not rely on specific cities or prefectures. Specifically, the firms in our sample are not concentrated only in the largest Chinese urban conglomerates, such as Shanghai and Beijing, or only in special economic zones, such as Shenzhen.

Table 1 reports a set of summary statistics for our main variables. Each panel refers to one of the (sub)samples we use in the analysis. We report summary statistics for all the firms for which we observe each variable. Panel A of Table 1 refers to our main sample of Chinese listed firms.

The sample is an unbalanced panel at the firm×year level, the longest time span being from 1997 to 2018. *After Punishment* is a dummy variable that equals 1 for firm observations in the years after the first SOE peer firm in a given prefecture was punished. About 27% of our observations refer to years after the punishment events. *SOE* is a dummy variable that equals 1 if the firm is an SOE in year t, and 0 otherwise. About a third of our firm×year observations are SOEs.

Our main outcome variable of interest is the amount of loan guarantees listed firms extend to their related private parties, for which we report two alternative definitions. *Provided Guarantees/Assets* is the amount of loan guarantees extended by a firm to any private parent or subsidiary divided by the previous end-of-the-fiscal-year assets.[16] Because the distribution of *Provided Guarantees/Assets* is skewed, we also create a dummy variable (*Provided Guarantees/Assets>10%*) that equals 1 if *Provided Guarantees/Assets* exceeds 10%, and zero otherwise.

The rest of Panel A provides statistics for financial characteristics and other observables. *Capital Investment* is the amount of fixed assets divided by total assets at the end of year t. We also construct a measure of TFP following Olley and Pakes (1996). *Long-term Debt*, measured as long-term debt over total end-of-previous-fiscal-year assets, is 5.4% on average. We also use the share of cash-like instruments over total end-of-previous-year assets, which is 17.7% on average. Tobin's Q is 1.9 for the mean and

[16]We winsorize this variable as well as all other continuous variables at the 1% and 99% percent levels to ensure outlier observations do not affect our results.

1.5 for the median firm in the sample.

Panels B and C of Table 1 refer to the two subsamples based on whether the firm is a SOE or a non-SOE. Relative to SOEs, non-SOEs provide larger loan guarantees to related parties, which might indicate that banks are more cautious to lend money to the related parties of non-SOEs. Relative to non-SOEs, SOEs borrow more long-term debt, invest more, hold less cash, and have higher Tobin's Q.

IV Research Design and Empirical Strategies

To assess whether experiencing the punishment of a peer has a stronger effect than the mere information about punishment events, we propose two research designs. The first design compares firm-level outcomes before and after the punishment of the first SOE in each prefecture (first difference) across peer SOEs and peer non-SOEs (second difference) and across peer firms and non-peer firms (third difference). The second difference allows us to compare the behavior of listed firms that operate within the same prefecture and at the same time, and hence are exposed to the same local business cycle shocks and to any other unobservable time-varying shocks (and time-invariant characteristics) that might confound with the first punishment of an SOE in the prefecture. The third difference allows us to tackle the systematic differences between SOEs and non-SOEs by comparing the reaction of peer SOEs to the reaction of non-peer SOEs, which are likely to be similarly informed about the punishment — they are major interventions by regulators on listed firms and are disclosed publicly and systematically.

A concern with this first empirical design is that peer SOEs and non-peer SOEs might still differ along dimensions correlated with their location, which might trigger different reactions to the same event. For instance, they might have systematically different levels of support from the central government. To address this type of concern, we also propose a second strategy, whereby we compare reactions *within* the group of peer SOEs and exploit heterogeneous incentives to react across such firms. We consider two sources of heterogeneity in the incentives to react to the same event involving the same peer. First, we consider career concerns — SOE CEOs with stronger career concerns should have a higher incentive to react, because a potential future punishment would be more costly for them than for other SOE CEOs. As we discuss in more detail below, we follow Jiang,

Wan, and Zhao (2015) and use CEOs' age to proxy for career concerns, because China has a legally mandated retirement age. Second, we consider the extent to which CEOs might find it costly to cut loan guarantees. We capture this margin by comparing peer SOE CEOs who were appointed shortly before the peer punishment events — and hence were barely involved in setting up the existing loan guarantees when the peer was punished — and other CEOs. Intuitively, more-recently appointed CEOs should have an easier time to change existing policies, which they can blame on their predecessors. Instead, cutting established loan guarantees should be costlier for CEOs with longer tenure, who might benefit personally from such relationships or at a minimum would have to justify why they changed a policy they had implemented in the first place.

For the first strategy, we compare yearly listed-firm-level loan guarantees before and after the first punishment of a listed SOE in each prefecture, across peer SOEs and peer non-SOEs, and across firms operating in the same prefecture and those operating in other prefectures. The difference we aim to assess is as follows:

$$[(Outcome_{SOE,p,after} - Outcome_{SOE,p,before}) - (Outcome_{non-SOE,p,after} - Outcome_{non-SOE,p,before})]$$
$$- [(Outcome_{SOE,\bar{p},after} - Outcome_{SOE,\bar{p},before}) - (Outcome_{non-SOE,\bar{p},after} - Outcome_{non-SOE,\bar{p},before})]$$

where p indicates the Chinese prefecture in which the SOE is punished, and $\bar{p}$ indicates other Chinese prefectures.

To implement this strategy, our most restrictive linear specification is as follows:

$$\begin{aligned} Outcome_{i,p,t} = \alpha + \beta SOE_{i,p,t} \times After\ Peer\ Punishment_{p,t} \\ + \gamma_1 SOE_{i,p,t} + \gamma_2 After\ Peer\ Punishment_{p,t} + X'_{i,t}\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}, \end{aligned} \tag{1}$$

where $SOE_{i,p,t}$ equals 1 if firm i is a SOE in year t and operates in prefecture p, and zero otherwise. $After\ Peer\ Punishment_{p,t}$ is a dummy variable that equals 1 for firms in prefecture p in the years after the first SOE is punished in prefecture p, and zero otherwise. The coefficient β captures the difference in outcomes across SOEs and non-SOEs based on whether they are in a prefecture in which the first SOE was punished or not, before and after the punishment. In the most restrictive specification, we partial out firm-level time-varying characteristics ($X_{i,t}$) and restrict the variation within firms (η_i) to account for systematic differences in firm-level time-invariant characteristics and within prefecture-

by-years ($\eta_{p,t}$) to account for different local business cycles and economic shocks. Firm fixed effects fully absorb industry fixed effects; that is, they also account for systematic time-invariant characteristics of industries that might explain the differential reaction of SOEs and non-SOEs to the punishment of a listed peer firm. This restrictive specification absorbs systematic time-invariant differences across firms. In this case, the variation in SOE status we exploit is variation within firms over time. Moreover, the specification absorbs any time-varying local economic shocks at the prefecture level, which allows us to account for local business cycles that might affect both the likelihood of punishment of local firms as well as the drop of unpunished firms' loan guarantees.

Throughout the analysis, we also report results when imposing a less restrictive set of fixed effects. We report results when adding separate prefecture and year fixed effects. These less restrictive specifications exploit variation in the SOE status of firms in the cross-section as opposed to variation in SOE status within firm over time. They also allow us to assess the stability of our results.

In all the specifications of the paper, we cluster standard errors at the prefecture level to account for the autocorrelation of residuals within firms over time and across firms that operate in the same prefecture.

We implement the second empirical strategy using the following type of specifications:

$$\begin{aligned} Outcome_{i,p,t} = \alpha + \beta_1 SOE_{i,p,t} \times After\ Peer\ Punishment_{p,t} \times Reactive_{i,p,t} + \gamma_2 SOE_{i,p,t} \times \\ After\ Peer\ Punishment_{p,t} + \gamma_3 After\ Peer\ Punishment_{p,t} \times Reactive_{i,p,t} + \gamma_4 SOE_{i,p,t} \times \\ Reactive_{i,p,t} + \gamma_5 SOE_{i,p,t} + \gamma_6 Reactive_{p,t} + \gamma_7 After\ Peer\ Punishment_{p,t} + \\ X'_{i,t}\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}. \end{aligned} \tag{2}$$

In equation (2), $Reactive_{i,p,t}$ is a dummy that equals 1 for firms whose CEOs have a stronger incentive to react to peers' punishments because of stronger career concerns (younger in age) in the first set of specifications and a dummy that equals 1 for CEOs who face a lower cost of reacting (appointed shortly before the peer's punishment) in the second set of specifications.

V Reaction to Peers' Punishment: Loan Guarantees to Related Parties

We now assess whether, after the first punishment for fraudulent loan guarantees implemented against a local SOE, peer SOEs are more likely to reduce the amount of loan guarantees they provide to private related parties (whether fraudulent or not) relative to non-SOEs operating in the same prefecture and SOEs operating elsewhere. We estimate the following:

$$\begin{aligned} \textit{Loan Guarantees}_{i,p,t} = \alpha + \beta SOE_{i,p,t} \times \textit{After Peer Punishment}_{p,t} + \gamma_1 SOE_{i,p,t} \\ + \gamma_2 \textit{After Peer Punishment}_{p,t} + X_{i,t}\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}, \end{aligned} \tag{3}$$

where the continuous version of $\textit{Loan Guarantees}_{i,p,t}$ is the amount of loan guarantees extended by firm i in prefecture p in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; the discrete version of $\textit{Loan Guarantees}_{i,p,t}$ is a dummy variable that equals 1 if *Provided Guarantees/Assets* exceeds 10%, and zero otherwise; $X_{i,t}$ includes the logarithm of total assets, financial leverage, total amount of cash, and Tobin's Q as a proxy for firms' investment opportunities.

Note the hypothesis we bring to the data does not have clear-cut predictions for coefficients γ_1 and γ_2. The null hypothesis $\gamma_1 = 0$ states that, on average, SOE peers extend the same amount of loan guarantees as other firms. Moreover, the null hypothesis $\gamma_2 = 0$ states that, after the punishment of a SOE peer firm, non-SOE local firms and non-peer SOEs do not cut their loan guarantees. Our hypothesis suggests that the absolute value of $\hat{\beta}$ should be higher than that of $\hat{\gamma}_2$, but is silent regarding the sign and size of $\hat{\gamma}_2$.

Table 2 reports the results for estimating equation (3). Consistent with our hypothesis, the estimated coefficient $\hat{\beta}$ is negative and statistically significant for both continuous (columns (1)-(3)) and discrete measures (columns (4)-(6)) of loan guarantees. The results are similar when we absorb time-varying shocks that affect all firms operating in the same prefecture (columns (3) and (6)). For the two dummies in level we detect no systematic patterns: $\hat{\gamma}_1$ flips sign across specifications and we fail to reject the null that $\gamma_2 = 0$.

In terms of economic magnitude, the differential continuous cut ranges between 1.5

pp and 3.3 pp., which is about 20% of a standard deviation of loan guarantees over assets in the sample (0.17). The differential likelihood of cutting large amounts of guarantees ranges between 5.2 and 10.1 percentage points, which is about 15-25% of a standard deviation of the average likelihood of guarantees larger than 10% of assets.

Our unreported results show that the first peer punishment of a non-SOE does not result in a significant cut in loan guarantees by peer SOEs or non-SOEs. For the dummy variable that equals 1 if the amount of provided loan guarantees exceeds 10% of total assets, and zero otherwise, we find both statistically and economically insignificant effects of non-SOE peer punishments.

A Parallel-Trends Assumption

The validity of our research design relies on the assumption that listed non-SOEs headquartered in prefecture p represent a valid counterfactual for the behavior of listed SOEs headquartered in the same prefecture after the regulator imposes the first punishment of a listed SOE in prefecture p. This *parallel-trends* assumption states the outcomes of the two groups of peer firms would have followed parallel trends throughout the sample period — both before and after the peer's punishment — had the punishment not happened.

Testing for whether trends would be parallel in the unobserved potential outcome of no punishment happening is impossible. To assess the plausibility of this assumption, we can at most test whether the trends of outcomes across our treatment and control group of firms are parallel before the punishment year and maintain an assumption about potential similar behaviors after the punishment had it not happened. We do so by estimating the following:

$$\begin{aligned} \textit{Loan Guarantees}_{i,p,t} = \alpha + \sum_t \beta_t SOE_{i,p,t} \times Year_t + \gamma_1 SOE_{i,p,t} + \sum_t \gamma_{2,t} Year_t \\ + X'\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}, \end{aligned} \tag{4}$$

where $\sum_t \beta_t SOE_{i,p,t} \times Year_t$ is a set of interactions of a dummy variable for whether firm i is an SOE and year dummies for all the t event years around the first punishment of a listed firm in prefecture p, and the other variables are defined as in equation (1). The null hypothesis that pre-trends are parallel across treatment and control groups implies that

the estimates of β_t in equation (4) for the years before the first punishment equal zero.

Table 3 reports the estimates of our richest specification, which includes firm and prefecture-by-year fixed effects. Column (1) considers the continuous version of our outcome variable, whereas in column (2) we consider the non-linear version. In both cases, the excluded year is the one in which the first SOE was punished in the firm's prefecture. Ultimately, we fail to reject the null hypothesis—we do not detect any differential pre-trends across treated and control groups. In fact, for the years before the first SOE punishment, the estimates for β_t switch between positive and negative values, are small in magnitudes, and statistically insignificant. In untabulated results (available upon request), we fail to detect differential pretrends even for the SOEs that enter our second strategy across all the differences on which that strategy hinges.

B Robustness

In Table 4, we assess the robustness of our baseline findings. First, we exclude firms in prefectures that experienced no punishment in our sample period, which might differ systematically from other prefectures. Panel A shows that our results are unchanged with this sample restriction.

To address the concern that the timing of punishment of a few large cities might drive our results, Panel B shows the results are similar if we exclude the most important Chinese commercial cities — Beijing, Shanghai, and Shenzhen.

We also note that the SOE status of many firms changes during our sample period, as the Chinese government proceeded with privatizations. We show that this pattern is not material for our results in Panel C of Table 4. Results are similar if we fix the SOE status of firms at the time of the first announcement of a punishment in the peer's location.[17]

In Panel D of Table 4, we estimate our baseline specification by weighted least squares (WLS). To assess whether large urban conglomerations or less concentrated areas mainly drive our results, we weigh observations based on the total number of firms in the prefecture. Results stay statistically significant and the size of the estimated effects is similar to the baseline analysis of Table 2, suggesting both large urban conglomerates and other prefectures are important.

[17]Here, we again exclude firms located in prefectures in which SOEs had never been punished during the sample period

In a similar vein, Panel E of Table 4 proposes a WLS analysis in which we weigh observations by assets at the firm level to test whether large firms drive the results. Because our point estimates across specifications are similar to the baseline in Table 2, we conclude the largest firms in the sample do not fully drive our baseline results.

In Panel F of Table 4, we add a full set of interactions of our baseline control variables in equation (3) with the dummy for the years after the local punishment event. The idea is to verify that time-varying controls at the firm level do not wash out the effect we attribute to the SOE status of the firm. Again, we find our baseline estimates are virtually unchanged.

Panel G of Table 4 considers an important potential reporting issue with our data. Because the enforcement of reporting standards was weaker before 2004, for the period between 1997 and 2004, we do not know whether firms did not report any loan guarantees were indeed not providing guarantees or simply not reporting them. In Panel G we exclude all firms that reported no loan guarantees up to 2004 and suddenly reported a positive value in 2004. We argue that these firms are more likely to include non-reporters that in fact were extending guarantees before 2004. Results are qualitatively similar.

Our unreported results also show the results are similar within a set of subsamples, including dropping observations after 2012, when the Anti-corruption campaign started.

C Collapsed Sample: Pre– and Post–Peer Punishment

Our strategy uses repeated observations of the same firm over time for several periods both before and after the treatment, and hence could raise concerns about statistical inference and the identification of the local treatment effect (e.g., Bertrand, Duflo, and Mullainathan (2004)). Our clustering of standard errors at the prefecture level reduces the concern of incorrect statistical inference due to autocorrelation. To further dismiss these concerns, we estimate the specification proposed by Bertrand, Duflo, and Mullainathan (2004), in which we average all the variables in the analysis at the firm level before and after the first punishment in the firms' prefecture. This "collapsed sample" leaves us with at most two observations for each firm — one before and one after the peer's punishment.

Using the collapsed sample, we can also assess the build-up of our effects over time. Intuitively, we would expect that listed firms might at least take some time to renegotiate the debt contracts in which they provide guarantees to related parties. Consistently, in

Table 5 the baseline effect builds up slightly in the first several years after the punishment in terms of magnitude but then gradually fades away over time.[18]

D Second Empirical Strategy: Peer SOEs with Different Incentives to React

The results we have presented so far might raise the concern that systematic differences between SOEs and non-SOEs in the same location and/or between local SOEs and SOEs elsewhere could vary around the punishment events and such changes might explain the differential trends in loan guarantees rather than peer SOEs' stronger reaction to the punishments.

To tackle this concern, we move on to our second empirical strategy, which, within peer SOEs only, exploits variation in the ex-ante incentives managers might have to react to the same peers' punishments in the same location and at the same time. First, we consider variation in CEOs' career concerns (see e.g., Gao, Kleiner, and Pacelli (2020)). Intuitively, being punished by the regulators has plausible reputation consequences for managers above and beyond the firm-level consequences of punishment. All else equal, including firm-level characteristics and the SOE status of the firm, SOE CEOs with more severe career concerns should be more willing to cut loan guarantees — a potential source of wrongdoing — relative to SOE CEOs with less severe career concerns after observing a peer's punishment.

To capture career concerns, we follow Jiang, Wan, and Zhao (2015), who consider CEO age. The rationale hinges on the fact that at the time of a firm's punishment, local SOE CEOs are at different stages of their careers. The closer the CEO is to retirement age — which China mandates at 60 years of age for men, 55 years of age for women, even for managerial and white-collar jobs, and allows for 3 additional years for central-government employees — the less severe are the career concerns and hence the less sensitive the CEO might be to react to a SOE peer's punishment. The intuition is that a CEO who is close to retirement would lose the retirement benefits in case of punishment, but a younger CEOs would lose more—not only the same retirement benefits but also the chance of a career

[18]Note that in the collapsed specifications, we have no scope to absorb year fixed effects, because the dummy variable *After Peer Punishment* (Time Fixed Effect) absorbs any systematic differences between the pre-punishment and post-punishment period averages at the firm level.

path, including a potential future political career, which is often the main aim of SOE CEOs (see e.g., Mehta and Zhao (2020)).[19] Before making this comparison, we verify in untabulated results that SOEs with young versus old CEOs or CEOs with long or short tenures do not differ systematically based on observable characteristics.

Panel A of Table 6 reports the results for this test. We define *Retiring* as a dummy variable that equals 1 if a CEO's age is within two years before the mandated retirement age.[20] Consistent with our conjecture, SOEs whose CEOs are younger and hence have more severe career concerns reduce loan guarantees in an economically and statistically significant manner after peers' punishments relative to before and to non-SOEs. Instead, for SOEs whose CEOs are closer to retirement age, the effect disappears as we can see by adding the coefficients in the first two rows of Panel A.

One might worry that punishments might affect younger CEOs more than older CEOs, who might have stronger connections with the party leadership.[21] To assess this potential explanation, we compute the probability of punishment for young and old CEOs separately. The unconditional likelihood of punishment is 0.70% for young CEOs and 0.73% for retiring CEOs. A t-test for whether these probabilities are equal cannot reject the null at any standard level of significance.

The second proxy to capture variation in CEOs' incentives to react is based on the intuition that CEOs who have been appointed shortly before the peers' punishments are likely to face a lower cost from terminating existing loan guarantee contracts with private related parties, because such contracts were likely signed by their predecessors. Thus, we would expect that recently-appointed SOE CEOs might react more to the direct experience of a local peer punishment.

Panel B of Table 6 reports the results, which are consistent with our conjecture — we detect an economically and statistically significant cut of loan guarantees by long-tenured SOE CEOs after the first SOE peer's punishment, but the effect is economically and statistically larger for SOE CEOs who were appointed up to two years before the event.

[19] In another important setting, Mehta and Zhao (2020) find that politicians that serve on U.S. congressional committees with SEC-relevant oversight responsibilities ("SEC-relevant politicians") display a 31% greater likelihood of losing a reelection campaign after a local firm faces SEC enforcement for financial misconduct.

[20] The results are qualitatively similar if we define *Retiring* as a dummy variable that equals 1 if a CEO's age is within one year before the mandated retirement age.

[21] We thank Stefan Zeume for suggesting this alternative explanation.

VI Alternative Explanations and Other Peer Dimensions

We move on to assess two prominent alternative explanations for our results. First, we consider the heterogeneous strength of political connections. Firms in our setting might react differently because of stronger or weaker ties to the local government, which induce different beliefs about the possibility of being punished (e.g., Huang, Pagano, and Panizza (2020)). Moreover, in areas where the local government is more connected to local listed firms, party officials might engage in moral suasion to convince non-punished SOEs to cut their guarantees after a local SOE is punished. This explanation would not imply a reaction of peers after salient exposure to punishment, but a reaction of peers to what local governments ask them to do.

Second, one might wonder whether our geographic peer definition captures other dimensions, such as industry ties. Note that our channel of an effect of direct experience of peer's punishments could definitely act at the same time at both a geographic and an industry level — any dimension that creates peer groups of firms interacting more closely than others is a candidate to test our channel.

A Heterogeneous Strength of Party Relationships

We start by assessing whether our effects might be driven by politicians engaging in moral suasion to convince local SOEs to cut their loan guarantees after the first local punishments.

Obtaining a meaningful proxy for the strength of the relationship between party officials and local SOEs is the main challenge to test this explanation, especially in a setting like China in which information about lobbying activities and other connections between firms and the government is not publicly available. We build on the fact that a subset of SOEs are under the control by the State-owned Assets Supervision and Administration Commission of the State Council (SASAC).

The Chinese government set up the SASAC in November 2002 at both the central and local government levels. One important objective of the SASAC has been to supervise the economic reform and restructuring of SOEs and to improve their corporate governance. To achieve these objectives, the SASAC appoints auditors and board members, among

other measures. By analyzing 63 SASAC-supervised SOEs between 2005 and 2007, Du, Young, and Tang (2012) find supervisors frequently communicate with SOE managers about the operations and management of SOEs.

We manually identify SOEs controlled by either central or local SASACs. The majority of local SASACs adopt monitoring rules similar to the central SASAC. If our findings were driven by direct private communications between SOEs and the SASACs after the first local SOE is punished, rather than by a unilateral reaction of SOEs to observing the punishment of their local SOE peers, our test results should be stronger for SOE peers under the watch of SASAC.

Panel A of Table 7 reveals that, across the board, we fail to detect any differential reaction of peer SOEs under SASAC control and other peer SOEs in terms of their reactions to peers' punishment. In Panel B of Table 7, we compare the reactions between SOEs directly controlled by the central SASAC and other SOEs. Again, we fail to detect any systematic differences between the two types of SOEs in terms of their reactions. Therefore, private communication and the resulting coordination between SOEs and the Chinese government, although important for matters such as performance evaluation, does not appear to be a materially relevant channel to explain our results.

B Industry Peers

So far, we have focused on a geographic definition of peers, where proximity captures the fact that peer firms operate in the same social environment, face similar demand, hire from the same pool of local workers, and are subject to the same local institutional features. At the same time, the experience of a peer's punishment could also be heightened through other connections between firms, such as belonging to the same industry. We therefore propose a specification in which peers are not only defined based on their geographic location, but also separately based on the industry to which they belong. Under the latter definition, all the direct competitors of a punished firm are peers, irrespective of their location.

In Table 8, we include the two definitions of peers — the geographic dummy we have used so far as well as a new dummy for the industry peers of a punished SOE. If geographic proximity was merely capturing industry ties, adding the new dummy and its interaction with firms' SOE status should eliminate our estimated baseline effect. If instead, industry

links matter above and beyond geographic proximity in transmitting the sobering effect of peer punishments, we should detect effects for both geographic and industry peers.

Our results are consistent with the second conjecture, whereby both geographic proximity and industry linkages are important dimensions that transmit the sobering effect of peers' punishment: SOEs react similarly in terms of both size and statistical significance to the punishment of both their geographic and industry peers.

To further verify that geographic peers do not substitute for industry linkages, in unreported tables we propose a robustness test in which we exclude all firms that are at the same time a geographic and an industry peer of the punished firms. Our baseline results are confirmed.

VII Salience of Punishment Events

Information about peers' punishments is publicly available in our setting and made public by the CSRC and local regulators based on which level implements the punishment. Because all events concern listed firms, it seems implausible that other listed firms are unaware of the punishments. At a minimum, peer SOEs and peer non-SOEs should be similarly informed about the punishment of a local firm. If firms were reacting due to a mere deterrence effect of punishments we should not observe differences in the reactions of peer SOEs and non-SOEs.

Our mechanism instead proposes that experiencing a peer's punishment is more salient to peers than to other firms and for this reason, even though everybody knows about the punishment, peer firms would react more. To provide a more direct test for this mechanism, we consider a proxy for the salience of punishment events. The proxy relies on the returns of punished firms around the announcement of their punishments (*market reaction*).

Of course, more negative market reactions likely capture harsher punishments and events that have a stronger negative effect on punished firms. This information content of the market reaction, though, is identical across all firms, who observe the punished firm's stock price over time. Stronger relative reactions by peers to harsher punishments thus cannot be driven by an information channel but are likely driven by the fact that stronger market reactions make the punishment events more salient to peers.

For this test, we define as salient a punishment event that results in a drop of more than 10% in cumulative abnormal returns for punished firms in the 30-day window around the announcement, but the results are similar if we vary the threshold or window length. Table 9 reveals that the differential effect of peers' punishment on peer SOEs' reaction is 2 to 3 times larger when the punishment is more salient.

VIII Peer Punishments and Other Corporate Policies

If SOE CEOs decided to cut loan guarantees to eliminate the possibility of being punished for wrongdoing, they might also be willing to engage in other costly signals to show that their companies do not engage in wrongdoing and hence change other corporate policies. Because inefficient investment is one of the most studied outcomes of the agency problem between firms' shareholders and managers, we assess whether the investment and the efficiency of the use of firm resources, captured by total factor productivity (TFP), change over time around peers' punishment and across peer SOEs and non-SOEs.

A Investment

Columns (1)-(3) of Table 10 report the results for estimating equation (3) when we use investment (fixed assets over previous end-of-year total assets) as the outcome variable. SOEs decrease their investment after the first SOE peer is punished in their location relative to before, to non-SOEs, and to SOEs in other locations. In terms of economic magnitude, the differential drop in investment by SOEs after the first peer's punishment is 1.1-2.1 percentage points, which corresponds to about 7-14% of a standard deviation of fixed assets over total assets in the sample.

B TFP

The drop in investment by SOEs might improve shareholder value by eliminating inefficient investment and wasteful projects or reduce shareholder value if SOEs cut positive net-present-value projects. As a rough proxy for the efficient use of firm-level resources, we compute firms' TFP and use it as an outcome in equation (3).

We run this analysis in columns (4)-(6) of Table 10. SOEs' TFP increases after the first peer punishment, relative to before, to non-SOEs in the same location, and to SOEs in other locations. In the specification that only includes prefecture and year fixed effects, we fail to reject the null hypothesis that the interaction coefficient $\hat{\beta} = 0$, but we can convincingly reject the null in the more restrictive specifications. In terms of economic magnitude, the size of the estimated effects range from 0.12 to 0.18, which is between 7% and 11% of a standard deviation of TFP in the sample.

IX Effects of Peers' Reaction on Related Parties

Our results so far do not rule out that SOE CEOs engage in substitution across wrongdoing activities. For instance, they might cut loan guarantees to avoid scrutiny but start to engage in different and more opaque forms of tunnelling.

If SOE CEOs substituted loan guarantees with other forms of tunnelling, related-party outcomes should not change systematically after the peers' punishment relative to before. Instead, detecting a systematic change in SOE-related parties' outcomes would suggest that related parties did indeed suffer a cut in financing and resources.

Table 11 reports the results of estimating equation (3) using the outcomes of related parties as the dependent variable. In columns (1)-(3), the outcome variable is a dummy that equals 1 if related parties obtain credit through bank loans guaranteed by a listed firm, and zero otherwise. For this analysis, we restrict our sample to firm-year observations associated with at least one borrowing transaction with banks, which explains the smaller size of this sample relative to the analysis so far. We find that the borrowing of related parties based on guaranteed loans drops significantly, both statistically and economically, after the SOEs' peers are punished because the size of the marginal effect is about 6-8 percentage points, which accounts for 13-17% of a standard deviation of the dependent variable.

In columns (4)-(6) of Table 11, we consider the related parties' long-term leverage as the outcome variable and we find that after peers' punishments, related parties of SOEs decrease long-term ratios substantially.

The decrease in SOEs' related parties borrowing after the punishment of peers of the related listed firms is evidence against the possibility that SOEs cut loan guarantees but

shifted to more opaque strategies to still ensure that resources could be transferred to related private parties in the form of bank borrowing.

X Conclusions

We propose an empirical laboratory to test whether the salience of direct experience of a peer's punishment affects choices above and beyond the informational deterrence effect of punishments.

In a first empirical design, we compare the reactions of local Chinese peer SOEs to those of non-peer SOEs and local non-SOEs after the same SOE is punished due to fraudulent loan guarantees to private related parties. We find that local peer SOEs cut the loan guarantees they extend to related private parties more than other firms. In a second empirical design, we zoom into peer SOEs and find that those with a higher incentive to react do indeed react more to peers' punishments.

This "punish one, teach a hundred" mechanism could be a cost-effective governance method in countries in which traditional forms of corporate governance are not available or in industries deemed strategic by governments, which are shielded from traditional governance mechanisms.

Our evidence opens a set of questions that beget further investigation. Is the sobering effect of peers' punishment a complement or can it be a substitute of more expensive mechanisms that aim to guarantee a level playing field in financial markets? What are the features of punishments and psychological mechanisms through which the reaction to peers' punishments operates? Further research using field data and experimental research designs might provide insights into these questions.

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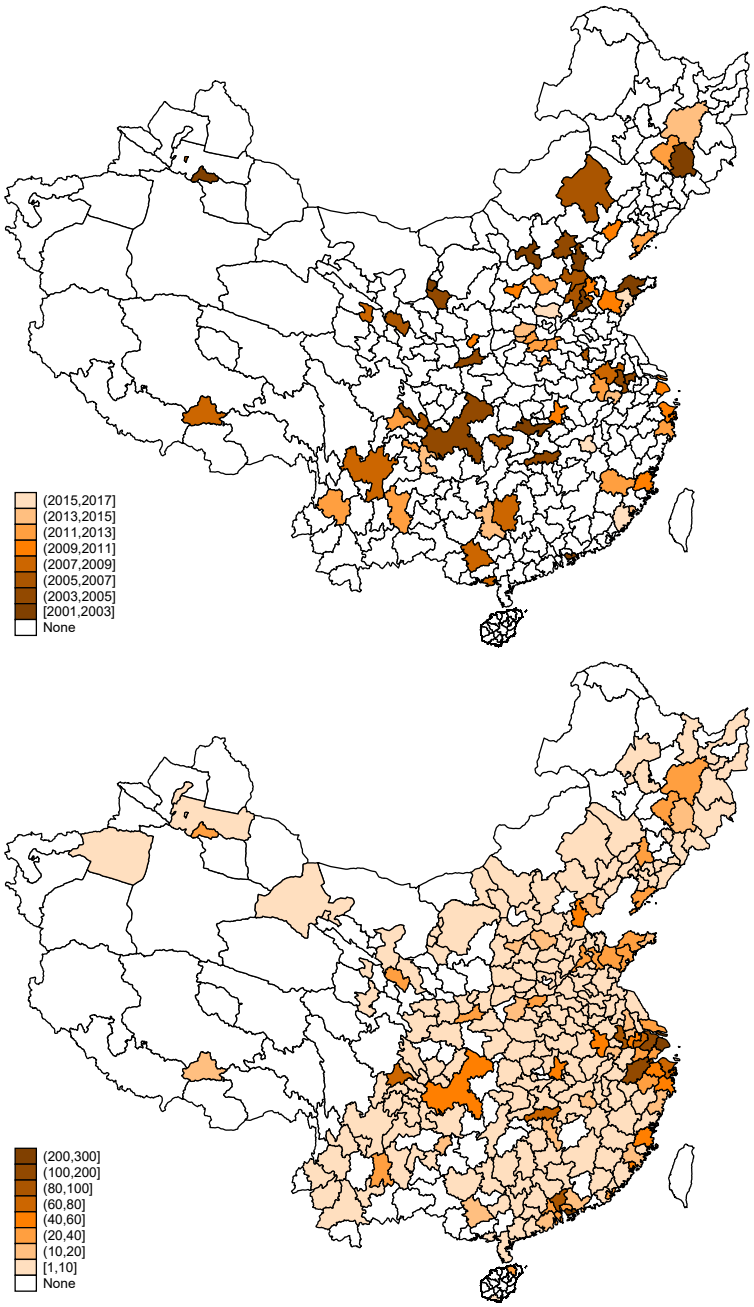
Figure 1: **Time of First Punishment and Number of Firms at the Prefecture Level**



This figure plots the time of the 72 first punishment in the top panel and the number of firms in the bottom panel at the prefecture level. The sample period is 1997 to 2018.

Table 1: **Descriptive Statistics**

This table reports summary statistics for the main variables we use in the analysis. Each panel refers to one of the samples we use and we report summary statistics for all the firms for which we observe each variable. Panel A refers to our main sample of Chinese listed firms, which did not experience any punishment for wrongdoings related to loan guarantees. Panels B and C refer to the two subsamples of SOEs and non-SOEs separately. The sample period is from 1997 to 2018. After Peer Punishment is a dummy variable that equals 1 if the prefecture has faced at least one punishment of a locally headquartered SOE firm, and zero otherwise. SOE is a dummy variable that equals 1 if the listed company is an SOE, and zero otherwise. Provided Guarantees/Assets is the amount of loan guarantees extended by a listed firm to any related parties scaled by the previous end-of-the-fiscal-year assets. Provided Guarantees/Assets > 10% is a dummy variable that equals 1 if Provided Guarantees/Assets exceeds 10%, and zero otherwise. We winsorize financial variables at the 1% and 99% levels.

Variable	N	Mean	Std	p10	p50	p90
Panel A. Main Sample						
After Peer Punishment	24,348	0.269	0.443	0.000	0.000	1.000
SOE	24,072	0.324	0.468	0.000	0.000	1.000
Provided Guarantees/Assets	23,773	0.077	0.169	0.000	0.000	0.252
Provided Guarantees/Assets >10%	23,773	0.208	0.406	0.000	0.000	1.000
Fixed Assets/Assets	20,888	0.148	0.153	0.002	0.102	0.370
TFP	15,252	-0.134	1.678	-2.115	-0.229	1.968
Ln (Assets)	23,773	21.693	1.235	20.319	21.544	23.284
Long-term Debt	21,972	0.054	0.082	0.000	0.016	0.169
Cash/Assets	23,740	0.177	0.133	0.048	0.141	0.361
Tobin's Q	22,849	1.890	1.128	1.071	1.526	3.079
Panel B. SOE Sample						
After Peer Punishment	7,809	0.230	0.421	0.000	0.000	1.000
Provided Guarantees/Assets	7,764	0.035	0.103	0.000	0.000	0.108
Provided Guarantees/Assets >10%	7,764	0.106	0.308	0.000	0.000	1.000
Fixed Assets/Assets	5,636	0.205	0.184	0.005	0.161	0.473
TFP	5,187	-0.108	1.737	-2.143	-0.200	2.144
Ln (Assets)	7,764	21.611	1.201	20.281	21.444	23.171
Long-term Debt	7,576	0.063	0.089	0.000	0.025	0.191
Cash / Assets	7,755	0.159	0.116	0.043	0.131	0.314
Tobin's Q	7,648	1.560	0.684	1.043	1.350	2.284
Panel C. Non-SOE Sample						
After Peer Punishment	16,263	0.288	0.453	0.000	0.000	1.000
Provided Guarantees/Assets	15,747	0.097	0.190	0.000	0.000	0.317
Provided Guarantees/Assets>10%	15,747	0.258	0.437	0.000	0.000	1.000
Fixed Assets/Assets	15,019	0.128	0.135	0.001	0.087	0.318
TFP	9,921	-0.148	1.640	-2.091	-0.242	1.879
Ln (Assets)	15,747	21.719	1.227	20.331	21.591	23.297
Long-term Debt	14,188	0.049	0.078	0.000	0.011	0.156
Cash/Assets	15,731	0.186	0.139	0.051	0.147	0.382
Tobin's Q	14,937	2.054	1.258	1.100	1.643	3.423

Table 2: **Loan Guarantees to Related Parties after Peer's Punishment**

This table reports estimates of β from the following linear equation:

$$\begin{aligned} \textit{Loan Guarantees}_{i,p,t} = \alpha + \beta \textit{SOE}_{i,p,t} \times \textit{After Peer Punishment}_{p,t} + \gamma_1 \textit{SOE}_{i,p,t} \\ + \gamma_2 \textit{After Peer Punishment}_{p,t} + X'\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}, \end{aligned}$$

where Loan Guarantees$_{i,p,t}$ is the amount of loan guarantees extended by firm i in prefecture p in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets (Provided Guarantees/Assets); Provided Guarantees/Assets > 10% is a dummy variable that equals 1 if Provided Guarantees/Assets exceeds 10%, and zero otherwise; SOE$_{i,p,t}$ is a dummy variable that equals 1 if listed company i in prefecture p was an SOE in year t, and zero otherwise; After Peer Punishment$_{p,t}$ is a dummy variable that equals 1 if prefecture p has faced at least one punishment of a locally headquartered SOE as of year t, and zero otherwise; X is a set of firm-level characteristics that include the logarithm of total assets, long-term leverage, cash over assets, and Tobin's Q; η_i and $\eta_{p,t}$ represent full sets of firm and prefecture-by-year fixed effects. We also propose specifications subject to less restrictive sets of fixed effects. We exclude firms in prefectures that experienced no punishment of a locally headquartered SOE throughout the sample period. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	Provided Guarantees/Assets			Provided Guarantees/Assets > 10%		
	(1)	(2)	(3)	(4)	(5)	(6)
After Peer Punishment	−0.002	0.001		−0.004	0.004	
	(−0.19)	(0.15)		(−0.23)	(0.20)	
After Peer Punishment × SOE	−0.015**	−0.020***	−0.033***	−0.052***	−0.059***	−0.101***
	(−2.11)	(−2.78)	(−3.36)	(−3.38)	(−3.32)	(−4.49)
SOE	−0.013***	0.018***	0.023***	−0.031***	0.034**	0.052***
	(−2.74)	(2.66)	(3.25)	(−2.78)	(2.08)	(2.94)
Ln(Assets)	0.006*	0.018***	0.013**	0.016*	0.048***	0.036**
	(1.66)	(3.16)	(2.20)	(1.82)	(3.41)	(2.32)
Long-term Debt	0.114***	0.076***	0.078***	0.249***	0.124**	0.140**
	(4.00)	(2.92)	(2.89)	(3.70)	(2.13)	(2.19)
Cash	−0.082***	−0.031**	−0.027**	−0.250***	−0.128***	−0.110***
	(−6.37)	(−2.58)	(−2.11)	(−8.03)	(−4.22)	(−3.47)
Tobin Q	−0.004*	0.001	−0.000	−0.016***	−0.002	−0.004
	(−1.81)	(0.26)	(−0.14)	(−3.01)	(−0.44)	(−0.65)
Constant	0.017	−0.281**	−0.212	0.075	−0.703**	−0.576*
	(0.20)	(−2.14)	(−1.65)	(0.37)	(−2.18)	(−1.67)
Year Fixed Effect	X	X		X	X	
Prefecture Fixed Effect	X			X		
Firm Fixed Effect		X	X		X	X
Prefecture-Year Fixed Effect			X			X
Observations	20,819	20,819	20,819	20,819	20,819	20,819
Adjusted R^2	0.16	0.41	0.39	0.16	0.38	0.36

t-statistics in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 3: **Parallel-Trends Assumption: Pre-trends**

This table presents the estimates of β_t from the following linear equation

$$Loan\ Guarantees_{i,p,t} = \alpha + \sum_t \beta_t SOE_{i,p,t} \times Year_t + \gamma_1 SOE_{i,p,t} + \sum_t \gamma_{2,t} Year_t + X'\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t},$$

where $\sum_t \beta_t SOE_{i,p,t} \times Year_t$ is a set of interactions of a dummy variable for whether firm i is a SOE and year dummies for all the t event years before the first punishment of a listed SOE in prefecture p, after partialling out firm characteristics (X), firm fixed effects (η_i), and prefecture-year fixed effects ($\eta_{p,t}$). $Year_{t\leq -7}$ is a dummy variable indicating all event years prior to $Year_{-6}$. $Year_{t\geq +7}$ is a dummy variable indicating all event years after $Year_{+6}$. The excluded period is event year 0, in which the first punishment of a locally headquartered SOE occurs. See Table 2 for definitions of other variables. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	Provided Guarantees/Assets	Provided Guarantees/Assets>10%
	(1)	(2)
$Year_{t\leq -7}$ × SOE	0.021	0.050
	(1.21)	(0.97)
$Year_{-6}$ × SOE	0.031*	0.052
	(1.92)	(1.14)
$Year_{-5}$ × SOE	0.010	-0.002
	(0.72)	(-0.05)
$Year_{-4}$ × SOE	-0.008	0.010
	(-0.54)	(0.31)
$Year_{-3}$ × SOE	0.013	0.043
	(0.81)	(0.95)
$Year_{-2}$ × SOE	-0.002	0.015
	(-0.17)	(0.34)
$Year_{-1}$ × SOE	-0.011	-0.010
	(-0.69)	(-0.23)
$Year_{+1}$ × SOE	-0.010	-0.087*
	(-0.54)	(-1.72)
$Year_{+2}$ × SOE	-0.016	-0.086*
	(-0.90)	(-1.77)
$Year_{+3}$ × SOE	-0.044***	-0.085**
	(-2.68)	(-2.22)
$Year_{+4}$ × SOE	-0.033	-0.078
	(-1.37)	(-1.49)
$Year_{+5}$ × SOE	-0.044	-0.130*
	(-1.55)	(-1.88)
$Year_{+6}$ × SOE	-0.039**	-0.136***
	(-2.18)	(-2.69)
$Year_{t\geq +7}$ × SOE	-0.043***	-0.096***
	(-3.09)	(-3.20)
SOE	0.020**	0.040*
	(2.10)	(1.73)
Ln(Assets)	0.013**	0.036**
	(2.25)	(2.36)
Long-term Debt	0.078***	0.141**
	(2.91)	(2.21)
Cash	-0.026**	-0.109***
	(-2.03)	(-3.44)
Tobin Q	-0.000	-0.004
	(-0.18)	(-0.70)
Constant	-0.213*	-0.578*
	(-1.70)	(-1.69)
Firm Fixed Effect	X	X
Prefecture-Year Fixed Effect	X	X
Observations	20,819	20,819
Adjusted R^2	0.39	0.36

t-statistics in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 4: **Loan Guarantees to Related Parties after Peer's Punishment — Robustness**

This table reports estimates of β from the following linear equation:

$$Loan\ Guarantees_{i,p,t} = \alpha + \beta SOE_{i,p,t} \times After\ Peer\ Punishment_{p,t} + \gamma_1 SOE_{i,p,t} + \gamma_2 After\ Peer\ Punishment_{p,t} + X'\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}.$$

We also propose specifications subject to less restrictive sets of fixed effects. See Table 2 for definitions of other variables. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	Provided Guarantees/Assets			Provided Guarantees/Assets > 10%		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Only if at least one Punishment			
After Peer Punishment × SOE	−0.014∗	−0.027∗∗	−0.035∗∗	−0.055∗∗	−0.076∗∗	−0.105∗∗
	(−1.82)	(−3.33)	(−3.49)	(−3.20)	(−4.19)	(−4.57)
Observations	15,389	15,389	15,389	15,389	15,389	15,389
Adjusted R^2	0.15	0.41	0.40	0.15	0.38	0.37
			Panel B. Excluding Beijing, Shanghai, Shenzhen			
After Peer Punishment × SOE	−0.019∗	−0.027∗∗	−0.038∗∗	−0.057∗∗	−0.071∗∗	−0.111∗∗
	(−1.83)	(−2.70)	(−2.81)	(−2.63)	(−3.28)	(−3.43)
Observations	17,581	17,581	17,581	17,581	17,581	17,581
Adjusted R^2	0.16	0.43	0.40	0.17	0.40	0.37
			Panel C. Fixing Initial SOE Status			
After Peer Punishment × SOE	−0.030∗∗	−0.034∗∗	−0.053∗∗	−0.067∗∗	−0.084∗∗	−0.141∗∗
	(−3.02)	(−3.41)	(−4.88)	(−2.20)	(−2.85)	(−5.16)
Observations	10,212	10,212	10,212	10,212	10,212	10,212
Adjusted R^2	0.14	0.37	0.36	0.14	0.35	0.35
			Panel D. Weighted Least Squares (w=N. local firms)			
After Peer Punishment × SOE	−0.010	−0.030∗∗	−0.036∗∗	−0.041∗∗	−0.072∗∗	−0.092∗∗
	(−1.39)	(−3.61)	(−2.98)	(−2.95)	(−5.04)	(−3.84)
Observations	20,819	20,819	20,819	20,819	20,819	20,819
Adjusted R^2	0.11	0.43	0.31	0.10	0.40	0.27
			Panel E. Weighted Least Squares (w=Total Assets)			
After Peer Punishment × SOE	−0.021	−0.033∗∗	−0.036∗∗	−0.060∗	−0.085∗∗	−0.087∗∗
	(−1.20)	(−2.26)	(−2.25)	(−1.88)	(−2.90)	(−3.30)
Observations	20,819	20,819	20,819	20,819	20,819	20,819
Adjusted R^2	0.27	0.59	0.62	0.28	0.56	0.61
			Panel F. Full Set Interactions Controls			
After Peer Punishment × SOE	−0.015∗∗	−0.023∗∗	−0.028∗∗	−0.052∗∗	−0.064∗∗	−0.090∗∗
	(−2.07)	(−3.12)	(−2.63)	(−3.40)	(−3.82)	(−3.82)
Observations	20,819	20,819	20,819	20,819	20,819	20,819
Adjusted R^2	0.16	0.41	0.39	0.16	0.39	0.36
			Panel G. Drop Firms with missing Guarantees pre–2004			
After Peer Punishment × SOE	−0.017∗∗	−0.022∗∗	−0.036∗∗	−0.056∗∗	−0.064∗∗	−0.105∗∗
	(−2.44)	(−3.19)	(−3.73)	(−3.63)	(−3.60)	(−4.63)
Observations	19,415	19,415	19,415	19,415	19,415	19,415
Adjusted R^2	0.15	0.42	0.39	0.16	0.39	0.37
Controls Table 2	X	X	X	X	X	X
Year Fixed Effects	X	X		X	X	
Firm Fixed Effects		X	X		X	X
Prefecture Fixed Effects	X			X		
Prefecture-Year Fixed Effects			X			X

t-statistics in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 5: **Loan Guarantees to Related Parties after Peer's Punishment: Collapsed Specifications**

This table reports estimates of β from the following linear equation:

$$\begin{aligned} \textit{Loan Guarantees}_{i,p,t} = \alpha + \beta \textit{SOE}_{i,p,t} \times \textit{After Peer Punishment}_{p,t} + \gamma_1 \textit{SOE}_{i,p,t} \\ + \gamma_2 \textit{After Peer Punishment}_{p,t} + X'\delta + \eta_p + \epsilon_{i,p,t}, \end{aligned}$$

where Loan Guarantees$_{i,p,t}$ is the average value of Provided Guarantees/Assets (Panel A), or Provided Guarantees/ Assets > 10% (Panel B), in the years before the first punishment event (t=pre) or in the years after the first punishment event (t=post); SOE$_{i,p,t}$ is a dummy variable that equals 1 if listed company i was an SOE in the year the first punishment occurs, and zero otherwise; After Peer Punishment$_{p,t}$ is a dummy variable that equals 1 in the period in which prefecture p has faced at least one punishment of a locally headquartered SOE firm, and zero otherwise; X is a set of average firm-level characteristics in the period before and after the first punishment event, which include the logarithm of total assets, long-term leverage, cash over assets, and Tobin's Q; η_p are a full set of fixed effects at the prefecture level. Across columns, we vary the years we use to average the observations at the firm level after the first punishment event in the prefecture. The overall sample period is from 1997 to 2018.

	t-3 - t+1	t-3 - t+2	t-3 - t+3	t-3 - t+4	t-3 - t+5	t-3 - t+7	t-3 - t+10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A. Provided Guarantees/Assets			
After Peer Punishment × SOE	−0.014∗	−0.022∗∗	-0.021***	−0.020∗∗	−0.017∗∗	−0.016∗	−0.012
	(−1.97)	(−2.76)	(-2.76)	(−2.91)	(−2.33)	(−1.82)	(−1.52)
Observations	1,309	1,321	1,327	1,334	1,337	1,338	1,342
Adjusted R^2	0.15	0.15	0.15	0.15	0.15	0.14	0.14
				Panel B. Provided Guarantees/Assets > 10%			
After Peer Punishment × SOE	−0.055∗∗	−0.060∗∗	-0.056**	−0.048∗∗	−0.046∗	−0.030	−0.020
	(−2.10)	(−2.39)	(-2.45)	(−2.12)	(−1.93)	(−1.26)	(−0.78)
Observations	1,309	1,321	1,327	1,334	1,337	1,338	1,342
Adjusted R^2	0.17	0.17	0.17	0.17	0.17	0.17	0.16
Controls Table 2	X	X	X	X	X	X	X
Prefecture Fixed Effects	X	X	X	X	X	X	X
Time Fixed Effects	X	X	X	X	X	X	X

t-statistics in parentheses

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 6: **Loan Guarantees to Related Parties after Peer's Punishment – Heterogeneous Incentives to React**

This table reports estimates of β from the following linear equation:

$$\begin{aligned} \textit{Loan Guarantees}_{i,p,t} = \alpha + \beta \textit{SOE}_{i,p} \times \textit{After Peer Punishment}_{p,t} \times \textit{Retiring}_{i,p} + \gamma_1 \textit{SOE}_{i,p,t} \times \\ \textit{After Peer Punishment}_{p,t} + \gamma_2 \textit{SOE}_{i,p,t} \times \textit{Retiring}_{i,p} + \gamma_3 \textit{After Peer Punishment}_{p,t} \times \\ \textit{Retiring}_{i,p} + \gamma_4 \textit{SOE}_{i,p,t} + \gamma_5 \textit{After Peer Punishment}_{p,t} + \gamma_6 \textit{Retiring}_{i,p} \\ + X'\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}. \end{aligned}$$

In Panel A, $\textit{Retiring}_{i,p}$ is a dummy variable that equals 1 if the of the CEO of firm i in prefecture p is within two years of the legal retirement age, and zero otherwise. In Panel B, $\textit{Recent}_{i,p}$ is a dummy variable that equals 1 if the CEO of firm i in prefecture p had been appointed at most two years before the peer's punishment, and zero otherwise. We drop observations for which the newly appointed CEO is within two years to legal retirement age. See Table 2 for definitions of other variables. We also propose specifications subject to less restrictive sets of fixed effects. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	(1)	(2)	(3)	(4)	(5)	(6)
	Provided Guarantees/ Assets			Provided Guarantees/Assets > 10%		
	Panel A. CEO Close to Retirement Age					
After Punishment × SOE × Retiring	0.040 * *	0.029*	0.027	0.156 * **	0.147 * **	0.146 * **
	(2.26)	(1.73)	(1.52)	(3.00)	(2.87)	(3.04)
After Punishment × SOE	−0.014*	−0.018 * *	−0.031 * **	−0.056 * **	−0.063 * **	−0.108 * **
	(−1.73)	(−2.21)	(−2.61)	(−3.19)	(−3.40)	(−4.28)
After Punishment × Retiring	−0.009	−0.020	−0.009	−0.056	−0.105 * **	−0.094 * **
	(−0.68)	(−1.40)	(−0.58)	(−1.62)	(−3.18)	(−2.68)
SOE × Retiring	0.018 * *	0.014*	0.020 * *	0.024	0.000	0.019
	(2.15)	(1.69)	(2.14)	(0.85)	(0.00)	(0.61)
Observations	19,694	19,694	19,694	19,694	19,694	19,694
Adjusted R^2	0.14	0.41	0.39	0.15	0.38	0.35
	Panel B. CEO Appointed Recently					
After Punishment × SOE × Recent	−0.014	−0.021*	−0.024 * *	−0.025	−0.049*	−0.068 * *
	(−1.36)	(−1.94)	(−2.25)	(−0.84)	(−1.69)	(−2.11)
After Punishment × SOE	−0.009	−0.010	−0.024 * *	−0.048 * *	−0.044 * *	−0.081 * **
	(−1.07)	(−1.32)	(−2.28)	(−2.43)	(−2.30)	(−3.28)
After Punishment × Recent	0.011	0.011	0.013	0.020	0.026	0.033*
	(1.07)	(1.30)	(1.45)	(1.00)	(1.50)	(1.83)
SOE × Recent	−0.004	−0.000	0.001	0.000	0.005	0.016
	(−0.70)	(−0.08)	(0.25)	(0.01)	(0.36)	(0.97)
Observations	17,980	17,980	17,980	17,980	17,980	17,980
Adjusted R^2	0.15	0.41	0.39	0.15	0.38	0.35
Controls Table 2	X	X	X	X	X	X
Firm Fixed Effects		X	X		X	X
Year Fixed Effects	X	X		X	X	
Prefecture Fixed Effects	X			X		
Prefecture-Year Fixed Effects			X			X

t-statistics in parentheses
$*p < 0.10, * * p < 0.05, * * *p < 0.01$

Table 7: **Loan Guarantees to Related Parties after Peer's Punishment – Private Communication between Government and SOEs**

This table reports estimates of β from the following linear equation:

$$\begin{aligned} \textit{Loan Guarantees}_{i,p,t} = \alpha + \beta \textit{SOE}_{i,p} \times \textit{After Peer Punishment}_{p,t} \times \textit{SASAC}_{i,p,t} + \gamma_1 \textit{SOE}_{i,p,t} \times \\ \textit{After Peer Punishment}_{p,t} + \gamma_2 \textit{SOE}_{i,p,t} \times \textit{SASAC}_{i,p,t} + \gamma_3 \textit{After Peer Punishment}_{p,t} \times \\ \textit{SASAC}_{i,p,t} + \gamma_4 \textit{SOE}_{i,p,t} + \gamma_5 \textit{After Peer Punishment}_{p,t} + \gamma_6 \textit{SASAC}_{i,p,t} \\ + X'\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}. \end{aligned}$$

In Panel A, $SASAC_{i,p,t}$ for firm i located in prefecture p is measured as a dummy variable that equals 1 if the SOE is under the control of either the central or local SASACs, and zero otherwise. In Panel B, $SASAC_{i,p,t}$ is measured as a dummy variable that equals 1 if the SOE is under the control of the central SASAC, and zero otherwise. See Table 2 for definitions of other variables. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	(1)	(2)	(3)	(4)	(5)	(6)
	Provided Guarantees/ Assets			Provided Guarantees/Assets > 10%		
	Panel A. SOEs controlled by either centeral or local SASACs					
After Punishment × SOE × SASAC	0.004	0.002	−0.000	0.003	0.021	0.018
	(0.40)	(0.24)	(−0.00)	(0.13)	(0.76)	(0.63)
After Punishment × SOE	−0.017∗	−0.021∗	−0.032∗∗	−0.050∗∗	−0.070∗∗	−0.111∗∗∗
	(−1.73)	(−1.90)	(−2.17)	(−2.31)	(−2.49)	(−3.07)
SOE × SASAC	−0.008	−0.008	−0.003	−0.019	−0.025	−0.020
	(−1.51)	(−1.36)	(−0.48)	(−1.34)	(−1.63)	(−1.14)
Observations	20,819	20,819	20,819	20,819	20,819	20,819
Adjusted R^2	0.16	0.41	0.39	0.16	0.38	0.36
	Panel B. SOEs controlled by centeral SASACs					
After Punishment × SOE × SASAC	−0.018	−0.011	−0.014	−0.037	−0.001	−0.014
	(−1.39)	(−0.71)	(−0.95)	(−0.99)	(−0.02)	(−0.42)
After Punishment × SOE	−0.010	−0.019∗∗	−0.030∗∗∗	−0.041∗∗	−0.061∗∗∗	−0.100∗∗∗
	(−1.28)	(−2.39)	(−2.85)	(−2.09)	(−2.94)	(−4.00)
SOE × SASAC	0.002	0.018∗	0.018∗	0.012	0.033	0.038
	(0.33)	(1.80)	(1.93)	(0.71)	(1.27)	(1.53)
Observations	20,819	20,819	20,819	20,819	20,819	20,819
Adjusted R^2	0.16	0.41	0.39	0.16	0.38	0.36
Controls Table 2	X	X	X	X	X	X
Year Fixed Effects	X	X		X	X	
Prefecture Fixed Effects	X			X		
Firm Fixed Effects		X	X		X	X
Prefecture-Year Fixed Effects			X			X

t-statistics in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 8: **Industry Peers**

This table reports estimates of β from the following linear equation:

$$\begin{aligned} \textit{Loan Guarantees}_{i,p,k,t} = \alpha + \beta_1 SOE_{i,p,k,t} \times \textit{After Geographic Peer Punishment}_{p,t} + \beta_2 SOE_{i,p,k,t} \times \textit{After Industry Peer Punishment}_{k,t} \\ + \gamma_1 SOE_{i,p,k,t} + \gamma_2 \textit{After Geographic Peer Punishment}_{p,t} + \gamma_3 \textit{After Industry Peer Punishment}_{k,t} + X'\delta + \eta_i + \eta_{p,t} + \eta_{k,t} + \epsilon_{i,p,k,t}, \end{aligned}$$

where After Geographic Peer Punishment$_{p,t}$ is a dummy variable that equals 1 if prefecture p has faced at least one punishment of a locally headquartered SOE but not in industry k as of year t, and zero otherwise; After Industry Peer Punishment$_{k,t}$ is a dummy variable that equals 1 if a firm operating in industry k but not located in prefecture p has faced at least one punishment as of year t, and zero otherwise. We also propose specifications subject to less restrictive sets of fixed effects. See Table 2 for definitions for other variables. We cluster standard errors at the level of the prefecture (p). We define industries based on the CSRC 2001 classification. The sample period is 1997 to 2018.

	Provided Guarantees/Assets			Provided Guarantees/Assets > 10%		
	(1)	(2)	(3)	(4)	(5)	(6)
After Geographic Peer Punishment × SOE	−0.012*	−0.017**	−0.020**	−0.048***	−0.054***	−0.077***
	(−1.70)	(−2.58)	(−2.08)	(−2.89)	(−2.74)	(−3.18)
After Geographic Peer Punishment	−0.003	0.000		−0.007	0.003	
	(−0.39)	(0.03)		(−0.36)	(0.12)	
After Industry Peer Punishment × SOE	−0.018**	−0.023***	−0.027**	−0.032**	−0.035*	−0.038
	(−2.37)	(−3.26)	(−2.09)	(−2.17)	(−1.96)	(−1.45)
After Industry Peer Punishment	−0.002	−0.001		0.000	0.001	
	(−0.20)	(−0.17)		(0.02)	(0.08)	
SOE	−0.001	0.028***	0.034***	−0.001	0.049***	0.062***
	(−0.13)	(4.11)	(3.51)	(−0.13)	(3.04)	(2.66)
Controls from Table 2	X	X	X	X	X	X
Year FE	X	X		X	X	
Industry FE	X			X		
Prefecture FE	X			X		
Firm FE		X	X		X	X
Industry-Year FE			X			X
Prefecture-Year FE			X			X
Observations	20,807	20,819	20,819	20,807	20,819	20,819
Adjusted R^2	0.19	0.40	0.39	0.19	0.37	0.35

t-statistics in parentheses

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 9: **Loan Guarantees to Related Parties after Peer's Punishment: The Role of Salience**

This table reports estimates of β from the following linear equation:

$$\begin{aligned} \textit{Loan Guarantees}_{i,p,t} = \alpha + \beta \textit{SOE}_{i,p,t} \times \textit{After Peer Punishment}_{p,t} \times \textit{Salience}_{i,p} + \gamma_1 \textit{SOE}_{i,p,t} \times \textit{After Peer Punishment}_{p,t} + \gamma_2 \textit{SOE}_{i,p,t} \times \textit{Salience}_{i,p} \\ + \gamma_3 \textit{After Peer Punishment}_{p,t} \times \textit{Salience}_{i,p} + \gamma_4 \textit{SOE}_{i,p,t} + \gamma_5 \textit{After Peer Punishment}_{p,t} + \gamma_6 \textit{Salience}_{i,p} + X'\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}, \end{aligned}$$

where $\textit{Salient}_{i,p}$ is a dummy variable that equals 1 if firm i located in prefecture p has faced at least one punishment of a locally headquartered firm as of year t and the CARs of the punished firm were lower than -10% in the 30 days around the punishment announcement. We also propose specifications subject to less restrictive sets of fixed effects. See Table 2 for definitions of other variables. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	Provided Guarantees/ Assets			Provided Guarantees/ Assets > 10%		
	(1)	(2)	(3)	(4)	(5)	(6)
After Peer Punishment × SOE × More Salient	−0.036	-0.041*	-0.043*	−0.067	−0.098 ∗ ∗	-0.135***
	(−1.42)	(-1.94)	(-1.85)	(−1.51)	(−2.24)	(-2.64)
After Peer Punishment × SOE	−0.014 ∗ ∗	-0.019***	-0.030***	−0.052 ∗ ∗∗	−0.061 ∗ ∗∗	-0.096***
	(−2.04)	(-2.65)	(-3.01)	(−3.25)	(−3.17)	(-4.15)
SOE × More Salient	0.033 ∗ ∗∗	0.037*	0.028	0.087 ∗ ∗	0.142 ∗ ∗∗	0.139***
	(2.89)	(1.79)	(1.12)	(2.57)	(3.05)	(2.67)
After Punishment × More Salient	0.033	0.039**		0.057	0.100 ∗ ∗∗	
	(1.49)	(2.40)		(1.25)	(3.20)	
Controls Table 2	X	X	X	X	X	X
Year Fixed Effect	X	X		X	X	
Prefecture Fixed Effect	X			X		
Firm Fixed Effect		X	X		X	X
Prefecture-Year Fixed Effect			X			X
Observations	19,916	19,916	19,916	19,916	19,916	19,916
Adjusted R^2	0.16	0.42	0.43	0.16	0.38	0.39

t-statistics in parentheses

$*p < 0.10, * * p < 0.05, * * *p < 0.01$

Table 10: **Investment and TFP After Peer's Punishment**

This table reports estimates of β from the following linear equation:

$$\begin{aligned} \textit{Real Outcome}_{i,p,t} = \alpha + \beta \textit{SOE}_{i,p,t} \times \textit{After Peer Punishment}_{p,t} + \gamma_1 \textit{SOE}_{i,p,t} \\ + \gamma_2 \textit{After Peer Punishment}_{p,t} + X'\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}, \end{aligned}$$

where Real Outcome$_{i,p,t}$ is either the amount of fixed assets scaled by total assets or the total factor productivity (TFP) of firm i in prefecture p in year t. See Table 2 for definitions of other variables. We also propose specifications subject to less restrictive sets of fixed effects. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	Fixed Assets /Assets			TFP		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.007	0.012**		−0.048	−0.038	
	(1.25)	(2.14)		(−0.86)	(−0.95)	
After Punishment × SOE	−0.021**	−0.016**	−0.011*	0.058	0.120**	0.181***
	(−2.40)	(−2.38)	(−1.65)	(0.68)	(2.18)	(2.83)
SOE	0.044***	0.007	0.007	0.018	0.003	−0.014
	(6.52)	(1.48)	(1.33)	(0.29)	(0.08)	(−0.38)
Ln(Assets)	−0.016***	−0.021***	−0.019***	0.914***	0.694***	0.688***
	(−3.46)	(−4.69)	(−4.15)	(29.21)	(24.39)	(23.48)
Long-term Debt	0.056	0.029	0.016	1.611***	0.449*	0.378
	(1.23)	(0.81)	(0.41)	(5.32)	(1.91)	(1.33)
Cash	−0.156***	−0.138***	−0.152***	−0.149	0.728***	0.651***
	(−10.10)	(−9.84)	(−9.62)	(−1.09)	(6.52)	(4.63)
TobinQ	−0.005**	−0.004**	−0.004***	0.027	0.102***	0.107***
	(−2.47)	(−2.54)	(−2.61)	(1.44)	(9.26)	(10.56)
Constant	0.466***	0.597***	0.610***	−20.307***	−15.451***	−15.412***
	(4.47)	(5.74)	(5.98)	(−28.07)	(−23.81)	(−24.20)
Year Fixed Effects	X	X		X	X	
Prefecture Fixed Effects	X			X		
Firm Fixed Effects		X	X		X	X
Prefecture-Year Fixed Effects			X			X
Observations	17,601	17,601	17,601	13,600	13,600	13,600
Adjusted R^2	0.28	0.75	0.77	0.53	0.83	0.83

t-statistics in parentheses

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 11: **Related-Party Borrowing after Peer's Punishment**

This table reports estimates of β from the following linear equation:

$$\begin{aligned} \textit{Related Party Outcome}_{i,p,t} = \alpha + \beta \textit{SOE}_{i,p,t} \times \textit{After Peer Punishment}_{p,t} + \gamma_1 \textit{SOE}_{i,p,t} \\ + \gamma_2 \textit{After Peer Punishment}_{p,t} + X'\delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}, \end{aligned}$$

where Related Party Outcome$_{i,p,t}$ is either a dummy variable that equals 1 if the amount of bank borrowing related parties obtain is positive or long-term debt of related parties scaled by total assets of firm i in prefecture p in year t; SOE$_{i,p,t}$ is a dummy variable that equals 1 if listed company i in prefecture p was an SOE in year t, and zero otherwise; After Peer Punishment$_{p,t}$ is a dummy variable that equals 1 if prefecture p has faced at least one punishment of a locally headquartered firm as of year t, and zero otherwise; X is a set of firm-level characteristics that include the logarithm of total assets, long-term leverage, cash over total assets, and Tobin's Q; Ln(Assets) (Related Party) and Current Liability (Related Party) are the logarithm of assets and current liability over assets measured at the level of related party; η_i and $\eta_{p,t}$ represent full sets of firm and prefecture-by-year fixed effects. We also propose specifications subject to less restrictive sets of fixed effects. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	Bank Borrowing			Related Parties' Long-term Debt		
	(1)	(2)	(3)	(4)	(5)	(6)
After Peer Punishment	0.027	0.023		−0.003	−0.001	
	(0.98)	(0.85)		(−0.61)	(−0.23)	
After Peer Punishment × SOE	−0.051	−0.067**	−0.082**	−0.011**	−0.007**	−0.010**
	(−1.42)	(−2.05)	(−2.07)	(−2.28)	(−2.11)	(−2.51)
SOE	−0.055**	0.017	0.036	0.011***	0.002	0.004
	(−2.02)	(0.58)	(0.99)	(2.90)	(0.68)	(1.36)
Ln(Assets)	0.027***	0.064***	0.078***			
	(3.38)	(3.18)	(4.34)			
Long-term Debt	0.142	0.036	0.059			
	(1.41)	(0.27)	(0.39)			
Cash	−0.356***	−0.181**	−0.165**			
	(−4.89)	(−2.26)	(−2.10)			
Tobin Q	−0.004	0.011	0.011			
	(−0.56)	(1.29)	(1.35)			
Ln(Assets) (Related Party)				0.015***	0.010***	0.010***
				(21.24)	(7.29)	(5.85)
Current Liability (Related Party)				−0.075***	−0.117***	−0.112***
				(−16.85)	(−11.74)	(−11.45)
Constant	0.104	−0.778*	−1.052***	−0.099***	−0.021	−0.009
	(0.59)	(−1.72)	(−2.68)	(−13.04)	(−1.15)	(−0.43)
Year Fixed Effect	X	X		X	X	
Firm Fixed Effect		X	X			
Related Party Fixed Effect					X	X
Prefecture Fixed Effect	X			X		
Prefecture-Year Fixed Effect			X			X
Observations	8,404	8,404	8,404	31,315	31,315	31,315
Adjusted R^2	0.12	0.46	0.42	0.22	0.67	0.69

t-statistics in parentheses

$*p < 0.10, **p < 0.05, ***p < 0.01$

Figure A.1: **Example of Announcement of Peer's Punishment by the CSRC**

This figure reports an example of a China Securities Regulatory Commission (CSRC) describing the punishment of a listed company due to wrongdoing related to the provision of loan guarantees to privated related parties.

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中国证监会行政处罚决定书（湘酒鬼、刘虹等9名责任人员）

证监罚字[2004]30号

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湖南酒鬼酒股份有限公司（以下简称湘酒鬼）证券违法一案，日前已由我会调查完毕，并依法履行了事先告知程序，举行了听证会，听取了当事人的陈述申辩。

经查明，湘酒鬼存在如下违法行为：1999年11月至2000年8月，湘酒鬼未经股东大会、董事会审议，累计为控股股东湖南湘泉集团有限公司（以下简称湘泉集团）提供了总金额为5082万元的贷款担保。对此贷款担保事项,湘酒鬼未按有关规定进行披露,直到2003年4月方在2002年年度报告中作披露，且所披露信息不完整。对此直接责任人员有原董事兼董事会秘书曹宏杰，在审议通过公司2002年年度报告决议上签字表示同意的董事彭善文、宋清宏、向选华、付光明、曹宏杰、杨波也负有一定的责任。

2002年11月27日湘酒鬼向银行出具继续履行担保责任承诺书,为湘泉集团2002年到期的两笔贷款1800万元再次提供担保,湘酒鬼未按规定及时准确披露上述事项。原代董事长、现任董事樊耀传代公司签定了上述协议，是对上述违法行为负有直接责任的主管人员。

2003年6月13日，在未经股东大会、董事会决议通过的情况下，湘酒鬼与中国工商银行湖南分行（以下简称工行）、湘西土家族苗族自治州人民政府、湖南湘泉大酒店有限公司（以下简称湘泉酒店）、湘泉集团签订《债权落实协议》（工行债协[2003]001号），约定湘酒鬼承接湘泉集团所欠工行贷款本金5082万元,在贷款到期前,由工行与湘酒鬼另行签订《借款合同》及其担保合同;由湘西自治州经济建设投资公司承担省盐业公司担保的5000万元贷款,由深圳利新源科技发展有限公司（湘酒鬼控股子公司）担保；湘酒鬼承诺对湘泉酒店承接湘泉集团所欠工行贷款本金16000万元提供保证等。湘酒鬼未按有关规定对上述协议进行披露。对此，签订该协议的湘酒鬼代表、董事长兼总经理刘虹是负有直接责任的主管人员，其他直接责任人员有湘酒鬼董事兼湘泉集团董事长杨波、现任董事兼财务总监杨建军、现任董事付光明。

证明上述事实的主要证据有：公司公开披露的有关文件、财会资料凭证、相关合同协议文本、公司提供的有关情况说明、当事人谈话笔录等证据证明，证据确实、充分，足以认定。

本会认为，湘酒鬼上述行为违反了《中华人民共和国证券法》（以下简称《证券法》）第五十九条“公司公告的股票或者公司债券的发行和上市文件，必须真实、准确、完整，不得有虚假记载、误导性陈述或者重大遗漏”的规定、第六十一条“股票或者公司债券上市交易的公司，应当在每一会计年度结束之日起四个月内，向国务院证券监督管理机构和证券交易所提交记载以下内容的年度报告，并予公告”中第（五）项“国务院证券监督管理机构规定的其他事项”的规定、我会《公开发行证券公司信息披露的内容与格式准则第二号〈年度报告的内容与格式〉》（1999年、2001年和2002年修订稿）关于关联交易披露的规定、《证券法》第六十二条“发生可能对上市公司股票交易价格产生较大影响、而投资者尚未得知的重大事件时，上市公司应当立即将有关该重大事件的情况向国务院证券监督管理机构和证券交易所提交临时报告，并予公告，说明事件的实质”中第（三）项“公司订立重要合同，而该合同可能对公司的资产、负债、权益和经营成果产生重要影响”的规定，构成了《证券法》第一百七十七条所述“依照本法规定，经核准上市交易的证券，其发行人未按照有关规定披露信息，或者所披露的信息有虚假记载、误导性陈述或者有重大遗漏的”行为。

根据湘酒鬼违法行为性质、情节，以及责任人员责任大小，依据《证券法》第一百七十七条的规定，经研究决定，对湘酒鬼处以40万元罚款，对刘虹给予警告、并处5万元罚款，对樊耀传给予警告、并处3万元罚款，对曹宏杰、杨波、杨建军、付光明、彭善文、宋清宏、向选华分别给予警告。

当事人应自收到本处罚决定书之日起15日内，将罚款汇交中国证券监督管理委员会（开户银行：中信实业银行总行营业部、账号7111010189800000162，由该行直接上缴国库），并将付款凭证的复印件送中国证券监督管理委员会法律部审理执行处备案。如对本处罚决定不服，可在收到本处罚决定之日起60日内向中国证券监督管理委员会提出行政复议；也可以在收到本处罚决定之日起3个月内直接向有管辖权的人民法院提起诉讼。复议和诉讼期间，上述决定不停止执行。

中国证券监督管理委员会

二〇〇四年七月二十七日

Table A.1: **Correlates of Punishment Events at the Prefecture-Year Level**

This table reports estimates of δ from the following linear equation:

$$Punishment\ Prefecture\ Year_{p,t} = \alpha + X'_{p,t}\delta + \eta_p + \eta_t + \epsilon_{p,t},$$

where Punishment Prefecture Year$_{p,t}$ is a dummy variable that equals 1 if a punishment of a listed SOE due to wrongdoing in loan guarantees to private related parties happens in prefecture p and year t, and 0 otherwise; the vector of potential determinants of punishment events (X) we consider include the following variables at the prefecture-year level: logarithm of GDP, employment rate, logarithm of population density, share of employment in heavy manufacturing, light manufacturing, and services, prefecture-level fiscal deficit ratio, a dummy for whether the prefecture changed its mayor around the year of the first punishment, a dummy variable for whether the prefecture changed its local party secretary around the year of the first punishment, the logarithm of the number of public firms operating in the prefecture-year, the share of SOEs as a percentage of all firms in the prefecture. The following variables are computed at the province-year level: an index of the strength of the government ownership of the local companies, an index of the development of non-SOE firms, an index of the development of local product markets, an index of the development of local input markets, and index of the development of local financial intermediation. The sources for province-year-level data is Fan et al. (2011) for the years 1997-2007 and Fan et al. (2016) for the years 2008-2014; η_p and η_t represent full sets of prefecture and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	Outcome Variable: Dummy=1 if Punishment Happens in a City and Year														
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Log GDP	-0.020														-0.039
	(-1.17)														(-1.01)
Employment Rate		-0.000													0.000
		(-0.90)													(0.65)
Log Population			-0.038												-0.046
			(-0.53)												(-0.49)
Log Population Density				0.002											0.001
				(0.12)											(0.04)
GDP% in Heavy Manufacturing					0.033										-0.215
					(0.45)										(-1.20)
GDP% in Light Manufacturing						-0.043									-0.083
						(-0.77)									(-0.69)
Fiscal Deficit							-0.000**								-0.000
							(-2.22)								(-0.89)
Government-Market Connectivity Index								0.001							-0.004
								(0.07)							(-0.39)
Non-SOE Development Index									-0.001						0.000
									(-0.27)						(0.05)
Product-Market-Development Index										0.000					-0.001
										(0.08)					(-0.11)
Input-Market-Development Index											0.003				0.004
											(1.12)				(1.02)
Financial Intermediation& Law Index												0.000			-0.000
												(0.23)			(-0.00)
Log Number of Public Firms													-0.003		0.005
													(-0.21)		(0.18)
SOE as a Percentage of Total # firms														0.001	0.006
														(0.09)	(0.53)
Constant	0.161	0.039	0.238	0.001	0.006	0.029	0.020**	0.032	0.047	0.032	0.015	0.033*	0.015	0.010	0.653
	(1.24)	(1.21)	(0.56)	(0.01)	(0.70)	(1.14)	(2.51)	(0.59)	(1.10)	(0.67)	(0.70)	(1.72)	(0.61)	(1.40)	(1.03)
Year Fixed Effect	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Prefecture Fixed Effect	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Observations	3696	3698	3699	3696	3671	3698	3699	3146	3701	3701	3701	3701	3701	3701	3701

t-statistics in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2: **Do Punishments Cluster within Locations over Time?**

This table reports estimates of β from the following linear equation:

$$\textit{Punishment Prefecture}_{p,t+n} = \alpha + \beta \textit{First Punishment Prefecture}_{p,t} + X'_{p,t+n}\delta + \eta_p + \eta_{t+n} + \epsilon_{p,t+n},$$

where First Punishment Prefecture$_{p,t}$ is a dummy variable that equals 1 if prefecture p had its first punishment of a local listed firm due to wrongdoing related to loan guarantees to private related parties in year t; Punishment Prefecture$_{p,t+n}$ is a dummy variable that equals 1 if the same prefecture had at least one punishment of a local listed firm due to wrongdoing related to loan guarantees to private related parties in year $t+n$. The vector of prefecture-level controls includes the set of controls in column (15) of Table A.1; η_p and η_{t+n} represent full sets of prefecture and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2018.

	Punishment in Year t+n?				
	n=1	n=2	n=3	n=4	n=5
	(1)	(2)	(3)	(4)	(5)
First Punishment in Prefecture (Year t)	-0.067*	-0.091***	-0.057	-0.065	-0.028
	(-1.82)	(-3.41)	(-1.64)	(-1.49)	(-0.64)
Constant	-0.021	-0.238	-0.270	-0.279	0.661
	(-0.03)	(-0.27)	(-0.47)	(-0.40)	(1.19)
Prefecture-level Controls	X	X	X	X	X
Year Fixed Effect	X	X	X	X	X
Prefecture Fixed Effect	X	X	X	X	X
Observations	2,627	2,626	2,423	2,193	1,962
Adjusted R^2	0.05	0.05	0.04	0.03	0.03

t-statistics in parentheses
$*p < 0.10, **p < 0.05, ***p < 0.01$