

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

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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.

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*JEL Classification:* D91, D72, G32, G41, K42.

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The Sobering Effect of Peer Punishment on the Unpunished\***

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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,<sup>1</sup> 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.

In this paper, we propose a setting to test if expert decision makers — managers of Chinese listed firms — behave in line with this mechanism.<sup>2</sup> 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 (e.g., Berkman, Cole, and Fu (2009); Jia, Shi, and Wang (2013)).<sup>3</sup> 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 (e.g., Jian and Wong (2003), Fisman and Wang (2010)).<sup>4</sup> Over the last two decades, Chinese regulators have tackled the punishment of fraudulent related-party transactions (Jiang, Lee, and Yue (2011)), and the tests in this paper exploit such punishment events to study the reaction of non-punished peers of the punished firms.<sup>5</sup>

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 that 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 (Chen, He, and Liu (2017); Gantchev, Gredil, and Jotikasthira (2018); Brav, Jiang, and Kim (2015); Fos (2016)). We thus compare the reactions to peer punishments by peer SOEs relative to peer

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<sup>1</sup>In Latin, “Unum castigabis centum emendabis.” A similar prescription stating “Punish One, Teach a Hundred” is often attributed to Mao Zedong.

<sup>2</sup>We focus on public firms, which are more likely to comply with regulation than private firms (Slutzky (2018)).

<sup>3</sup>For the effects of related-party transactions on other real and accounting outcomes, see also Jian and Wong (2010); Cheung, Rau, and Stouraitis (2006).

<sup>4</sup>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.

<sup>5</sup>As we discuss in more detail below, these punishments started more than a decade before the anti-corruption campaign of 2013 (e.g., see Griffin, Liu, and Shu (2017); Li, Wang, and Zhou (2017); Goh, Ru, and Zou (2018); Lin et al. (2016)). 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.

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.

In the data, both SOEs and non-SOEs are punished due to fraudulent loan guarantees.<sup>6</sup> To define proper pre-punishment and post-punishment periods, we focus on the *first* punishments of SOEs in each Chinese prefecture, which are likely the most relevant to which peer firms should react.<sup>7</sup> We focus on punishments of fraudulent loan guarantees rather than other wrongdoings that are more common in the US, such as earnings misstatement, because those other wrongdoings have been punished in China since much earlier than the start of our sample, thus hindering us from defining periods before and after the first local punishment of an SOE firm.

In most of our 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 local norms. 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 customers. Empirically, geography and industry capture largely independent transmission channels of peer effects, because both dimensions increase SOEs' reactions to peer punishments in horse-race tests.

In principle, peers might acquire more information about punishments, although information about our punishments is made public by the China Securities Regulatory Commission (CSRC).<sup>8</sup> To dismiss this possibility, we propose a second design in which we only focus on a punished firm's peer SOEs and vary the extent to which peer SOEs have incentives to react. We consider CEOs' career concerns proxied by CEOs' distance from legally-mandated retirement age (Jiang, Wan, and Zhao (2015)), and CEOs' costs of reaction. We argue that CEOs who were appointed shortly before a peer was punished were likely less involved in setting up the loan guarantees in place at the time of the peer's punishment, and hence faced a lower cost of cutting them.

When we implement our first design, we find that after Chinese regulators punish a listed SOE for tunnelling via inter-corporate loan guarantees, unpunished peer SOEs cut their (legal or fraudulent) loan guarantees to related parties significantly more relative to non-peer SOEs and peer 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.

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<sup>6</sup>We also show that the timing of punishments is not predicted by a large set of observables that capture whether local governments want to avoid SOE punishment (Ru (2018)) and indices capturing the development of local SOEs, local product markets, and financial markets (Fan et al. (2011); Fan et al. (2016)).

<sup>7</sup>As we describe in detail later, these punishments mostly happened before 2013, when the Chinese anti-corruption campaign started. Our results are unchanged if we exclude the few years and events after 2013.

<sup>8</sup>The CSRC is the main financial regulator in China and is responsible for most of the punishments we exploit in our setting.

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.<sup>9</sup>

We then implement our second research design, which only exploits variation in the incentives to react within the group of peer SOEs. First, we build on Karpoff et al. (2008) and Jiang, Wan, and Zhao (2015) and use CEOs' age as a proxy for CEOs' career 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.<sup>10</sup> 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 other SOEs 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 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 far as economic channels alternative to salience of direct exposure to peers' punishment are concerned, we first assess whether our main peer definition based on geographic proximity in fact captures other connections among firms, such as industry links. We find that our baseline results do not change if we exclude all geographic peer firms that are at the same time industry peers of the punished firms. Interestingly, and consistent with our mechanism, 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 almost completely orthogonal to each other.<sup>11</sup>

A second prominent alternative interpretation is that local politicians might engage in moral suasion

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<sup>9</sup>The last test is important because China faced a massive wave of SOE privatization in the 2000s (e.g., Fan, Wong, and Zhang (2007); Hsieh and Song (2015); Huang, Li, Ma, and Xu (2017)), and hence the SOE status of a substantial portion of the firms in our sample varies over time.

<sup>10</sup>We also account for the fact that central-government employees can retire three years later than the mandatory age for their gender.

<sup>11</sup>The hypothesis of salient exposure to punishment should apply similarly to any form of proximity that determines a close connection between peers, whether geographic or based on industry interactions.

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 in locations where the frequency of firms' visits by local politicians differs or in which the transparency of local governments' connections to listed firms differ.<sup>12</sup>

To provide more direct evidence of a role of salience in our proposed "punish one, teach a hundred" mechanism, we exploit the fact that all listed firms in our setting, whether peers of the punished SOE or not, access the same public information about the punishment. Because the information is the same for everybody, under a standard deterrence argument, all firms should react similarly to punishments of the same size, same gravity, and generally displaying similar characteristics. Instead, our channel suggests that the reaction of peer firms to the same information non-peer firms also observe should be higher, because peer firms relate more to those punished and hence the same information is more salient to them. Despite facing the same information about punishment events, then, under our channel we should observe that peer SOEs react more than non-peer SOEs or peer non-SOEs.

We propose 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 effects of the punishment, to vary the salience of the punishment event. Of course, a larger drop in CARs is likely to signal harsher consequences of punishment, but this informational content of market returns is the *same* for all firms, whether peers or not.<sup>13</sup> Salience predicts that, when facing the same informational content, peer firms should react more to more salient events. We find that peer SOEs' higher reaction is especially large after punishments that determine 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 envisage alternative and more opaque channels to allow their related parties to access external debt, which would neuter the effect of cutting loan guarantees,<sup>14</sup> 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 (Giannetti et al. (2017); Lagaras et al. (2017)), 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 peers' 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. Peers' effect could be especially viable where traditional 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

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<sup>12</sup>We use the measures developed by Nie et al. (2017) to capture variation in these dimensions.

<sup>13</sup>If anything, larger stock-market drops for punished firms should make information about the punishment percolate more to firms that were not close to the punished peer.

<sup>14</sup>We thank Stefan Zeume for raising this possibility.

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)) and whether mechanisms different from formal binding contracts can reduce the scope for wrongdoing (D'Acunto, Xie, and Yao (2021)).

Hail, Tahoun, and Wang (2018) investigate the extent to which regulation can reduce corporate scandals by acting on executives' ex-ante incentives. Ex post, the sanctions regulators impose on punished firms have a direct effect as well as a potential indirect reputational effect on punished firms, whose size is debated in the literature (e.g., see Karpoff, Lott, and Wehrly (2005) and Armour, Mayer, and Polo (2017)). 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.<sup>15</sup>

Our paper departs from the large literature in accounting and finance on the deterrence role of punishments (for instance, see Desai et al. (2007) and Chalfin and McCrary (2017) for a recent comprehensive review), which is based on the premise that mere information about the possibility of punishment sobers firms' behavior.<sup>16</sup> The mechanism we study hypothesizes that 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 such punishments than other firms who are similarly informed about the punishment events but for whom the events are less salient, given the lack of close connection to punished firms. A standard deterrence argument, instead, suggests that the information about a punishment event and/or information about the potential punishment of wrongdoing have the same sobering effects on all firms that are informed, irrespective of the nature of their relationship with the punished firm. 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., see Bizjak et al. (2009); Gopalan, Gormley, and Kalda (2018)), geographic proximity (e.g., see Parsons et al. (2018)), regulators' preferences (e.g., see Kedia and Rajgopal (2011)), and shareholder activism (e.g., see Gantchev et al. (2018)). 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

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<sup>15</sup>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)).

<sup>16</sup>Gleason et al. (2008) and Goldman et al. (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.

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 secondary equity markets.

We also relate to the large body of work on corporate governance mechanisms in the presence of blockholders (Faccio and Lang (2002)) and their effects on corporate outcomes, which Edmans (2014) surveys. Recent examples include wolf-pack activism (Brav, Dasgupta, and Mathews, 2017) and shareholder coalitions (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.<sup>17</sup> We contribute to the body of research studying settings in which governments own productive resources (Shleifer (1998); Bortolotti and Faccio (2008)) and in which political connections are valuable to firms (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 (e.g., Chen et al. (2017); Lennox et al. (2016); Hung et al. (2015)).

Finally, we contribute to the recent literature on the effects of salience on decision-making. Theory and evidence exist explaining how the salience of environmental characteristics affects economic decision-making with and without risk (Bordalo et al. (2012); Benjamin et al. (2010); D’Acunto (2018); D’Acunto (2017); Dessaint and Matray (2017); D’Acunto, Malmendier, Ospina, and Weber (2019)).

## 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 behavior of 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.

### 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” (Song and Xiong (2018)). 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) (Lin (2009); Xu (2011)).

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 (Song and Xiong (2018)). The characteristics

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<sup>17</sup>Meggison (2017) surveys the literature on state ownership of businesses, and D’Souza, Megginson, Ullah, and Wei (2017) study the performance of privatized firms.



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 (Bai et al. (2018)). Second, both types of firms have been introducing innovations such as basic forms of corporate governance and sound evaluation systems for employees (Song and Xiong (2018)).

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 (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. This was possible because surviving SOEs were protected from foreign competition even after the WTO entry. 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 were 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 system consists of four dominant players — 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.

To access capital from banks, smaller and private related parties often use loan guarantees from larger and often listed firms, which is in contrast to other countries, where governments often use guarantees to finance small businesses or support homeownership (e.g., see D'Acunto et al. (2018)). Loan guarantees, though, can also be used as a means to expropriate minority shareholders in listed firms. Fisman and Wang (2010) describe the mechanisms through which Chinese corporations tunnel resources to related private parties through loan guarantees. 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. The latter party wants that the listed company guarantees the loan. 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.<sup>18</sup> 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 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 for a guarantee in the absence of sufficient collateral. Indeed, even after 2007, more than a quarter of Chinese loans were backed by guarantees (McMahon, 2014).

### III Data

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

#### A Punishment Events

We 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.<sup>19</sup> 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 of the Online Appendix 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 targeted punishment to the chairman of the board of directions, who was fined 50,000 RMB

<sup>18</sup>[http://www.csrc.gov.cn/pub/newsite/flb/flfg/bmgf/ssgs/gljy/201012/t20101231\\_189866.html](http://www.csrc.gov.cn/pub/newsite/flb/flfg/bmgf/ssgs/gljy/201012/t20101231_189866.html)

<sup>19</sup>Earlier research has employed this source of data. For instance, see Chen et al. (2006) and Hung et al. (2015).

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 are relevant to our second empirical strategy, which exploits variation in the strength of the career concerns of the CEOs of Chinese listed SOEs.

The punishment-level information includes the date on which a punishment for a firm committing fraud is announced, the regulator that announced the fraud event (whether 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, the complete information about all these dimensions is only available for a small minority of 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 were detected and punished before 2000. We classify fraud events as related to loan guarantees either if the fraud database cites loan guarantee misconduct as at least one of the reasons for punishment or if 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.

Punishments can be implemented by four different agencies: 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.

### **A.1 Characteristics of Punishments**

Figure 1 describes the spatial distribution of the 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 (see D'Acunto, Liu, Pflueger, and Weber (2018)). 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 of the Online Appendix 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 et al. (2011) and Fan et al. (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 decides to target a prefecture and its firms for political or strategic reasons — in Table A.2 of the Online Appendix. We use a dummy for whether a prefecture had a punishment in year  $t$ , to predict the likelihood the same prefecture having 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 negatively related to the emergence of another punishment in the same location in the first two years post 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. Excluding the two special administrative regions (i.e., 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 to match those to the 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. The disclosure of related-party transactions became mandatory for all Chinese public firms starting in 2004.

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 map shows the firms that enter our analysis are 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 the 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 $\times$ 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 half of our firm $\times$ 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.<sup>20</sup> 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 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 suggests 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.

Panel D of Table 1 refers to the firm $\times$ year sample of all private related parties linked to a listed firm in our main sample. We observe more related parties in the period after peer punishments than before, relative to the main sample of listed firms (37%  $>$  26%), which suggests that the number of private firms

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<sup>20</sup>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.

to which listed firms are related has increased over time.

## 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 this 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 remaining 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 depending.

To address this type of concerns, 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 Karpoff et al. (2008) and 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, “fresher” CEOs should have an easier time to change existing policies, which can be blamed 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:

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

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 the  $SOE_{i,p,t}$  equals 1 if firm  $i$  is an 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  $\beta$  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 ( $\eta_i$ ) 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 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 any systematic variation across firms, which allows us to exclude the possibility that firm-level time-invariant characteristics explain the differential reaction to peer firms' punishment. In this case, the variation in SOE status we exploit is variation within firms and 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 fact unpunished firms cut their 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.

Note that our first research design does not make the (implausible) assumption that the timing and location of punishment events are randomly assigned at the prefecture level, which would suggest punishments are shocks exogenous to local observable and unobservable characteristics. Our design instead studies the differential reactions to peer punishments across different types of firms that face the



same local demand and supply shocks. If we wanted to interpret our results in a causal way, we would need to assume the punishment events are exogenous conditional on observables and unobservables related to the local economy.

In all the specifications of the paper, we draw statistical inference by correcting standard errors to allow for correlation of unknown form of the residuals at the prefecture level. This level of clustering allows us to account for the autocorrelation of residuals within firms over time and across firms 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.

## 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. We do so by estimating the following:

$$\begin{aligned}
Outcome_{i,p,t} = & \alpha + \sum_t \beta_t SOE_{i,p,t} \times Year_t \\
& + \gamma_1 SOE_{i,p,t} + \sum_t \gamma_{2,t} Year_t + X' \delta + \eta_p + \eta_t + \epsilon_{i,p,t},
\end{aligned} \tag{3}$$

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 before 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 consists thus in assuming that each of the estimates of  $\beta_t$  in equation (3) equals zero. Figure 2 reports the results for estimating the coefficients  $\beta_t$  for our main outcome of interest — the total amount of loan guarantees scaled by assets. Squares represent the estimated  $\hat{\beta}_t$ . The segments around each point represent 2-standard-error confidence bounds. We fail to reject the null hypothesis that any of the estimated  $\hat{\beta}_t$  differ from zero, either economically or statistically, before the punishments. This test suggests the trends in our main outcome variable are parallel before the punishments across peer SOEs and non-SOEs. In untabulated results (available upon request), we fail to detect differential pretrends even for the SOE firms that enter our second strategy across all the differences on which the strategy hinges.

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

We start our analysis by assessing 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. Both local and other firms are likely to be informed about the punishment, which is an official procedure against a listed company discussed in the media and reported in public reports. For these two groups, though, the punishment might be less salient given that it does not involve a direct peer.

We estimate the following linear specification by ordinary least squares:

$$\begin{aligned} \text{Loan Guarantees}_{i,p,t} = & \alpha + \beta \text{SOE}_{i,p,t} \times \text{After Peer Punishment}_{p,t} + \gamma_1 \text{SOE}_{i,p,t} \\ & + \gamma_2 \text{After Peer Punishment}_{p,t} + X_{i,t} \delta + \eta_i + \eta_{p,t} + \epsilon_{i,p,t}, \end{aligned} \quad (4)$$

where the continuous version of  $\text{Loan Guarantees}_{i,p,t}$  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  $\text{Loan Guarantees}_{i,p,t}$  is a dummy variable that equals 1 if Provided Guarantees/Assets exceeds 10%, and zero otherwise;  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  is an SOE in year  $t$ , and zero otherwise;  $\text{After Peer Punishment}_{p,t}$  is a dummy variable that equals 1 if prefecture  $p$  has faced at least one punishment of a locally-headquartered SOE firm as of year  $t$ , and zero otherwise;  $X_{i,t}$  is a set of firm-level characteristics that include the logarithm of total assets, financial leverage, total amount of cash, and Tobin's Q as a proxy for firms' investment opportunities; and  $\eta_i$  and  $\eta_{p,t}$  represent full sets of firm and prefecture-by-year fixed effects, respectively. For the sake of statistical inference, we cluster standard errors at the level of the prefecture ( $p$ ) to allow for correlation of unknown form across the residuals of listed firms headquartered in the same prefecture.

Note that the hypothesis we bring to the data does not have clear-cut predictions for coefficients  $\gamma_1$  and  $\gamma_2$ . The null hypothesis  $\gamma_1 = 0$  states that, on average, SOE peers extend the same loan guarantees as other firms. Moreover, the null hypothesis that  $\gamma_2 = 0$  states that, before the events, after the punishment of a SOE peer firm, non-SOE peer firms and non-peer SOEs do not cut the share of assets they extend in the form of loan guarantees to related parties, relative to the amounts they extended to related parties before the punishment of a peer firm. 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 (4). In columns (1)-(3) of Table 2, we define the dependent variable as the overall amount of loan guarantees listed firms provide to their private related parties, whereas in columns (4)-(6), we define the dependent variable as a dummy variable that equals 1 if the amount of provided loan guarantees exceeds 10% of total assets, and zero otherwise.

Consistent with our hypothesis, the estimated coefficient  $\hat{\beta}$  is negative, and we reject the null that it equals 0 at standard levels of significance for both definitions of loan guarantees. The results are similar across all the specifications of equation (4), including the most restrictive ones that absorb time-varying shocks that affect all firms operating in the same prefecture (column (3) and column (6)). For the two dummies in level we detect no systematic patterns:  $\hat{\gamma}_1$  flips sign across specifications and does not differ from zero statistically. We also fail to reject the null that  $\gamma_2 = 0$  in most specifications.

In terms of economic magnitude, the differential cut of loan guarantees over total assets by peer SOEs ranges between 1.5 pp (column (1) of Table 2) and 3.3 pp. This effect corresponds to about a 20% of a standard deviation of average guarantees over assets in the sample (0.17). The differential probability of cutting large amounts of loan guarantees ranges between 5.2 percentage points (column (4)) and 10.1 percentage points in the most restrictive specifications of column (6). This effect corresponds to about 13-25% of a standard deviation of the average probability of loan guarantees above 10% of total assets.

Table A.3 in the Online Appendix shows 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 Robustness

In Table 3, we assess the robustness of our baseline findings. First, we consider the fact that the sample of control firms in our baseline regressions include firms in prefectures that experienced no punishment throughout the sample period, which one might worry differ systematically from the prefectures that experience a punishment event in ways that might be related to our outcomes. In Panel A of Table 3, we show the results do not change if we restrict the sample to firms in prefectures that experience at least one punishment during the sample period.

To address the concern that the timing of punishment of a few large commercial cities might drive

our results, in Panel B of Table 3, we show the results are similar if we exclude the most important Chinese commercial cities, namely Beijing, Shanghai, and Shenzhen.

Furthermore, one might be concerned the SOE status of the firms in our sample could change dramatically during the period we consider, during which the Chinese government proceeded to privatize several SOEs. We show that this concern is unlikely to be material for our results in Panel C of Table 3. The results do not change substantially if we fix the SOE status at the time of the first announcement of a punishment in the peer's location. To do so, we have to exclude firms geographically located in prefectures in which SOEs had never been punished during the sample period.

In Panel D of Table 3, 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.

In a similar vein, Panel E of Table 3 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 3, we add a full set of interactions of our baseline control variables in equation (4) with the dummy for the years after the local punishment event. The idea is to verify 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 3 considers a potential reporting issue during the beginning of our sample period. Because before 2004, reporting loan guarantees was optional for listed firms, in that period, we do not know whether firms that do not report any loan guarantees are indeed not providing guarantees or are simply not reporting them. To assess whether this potential noise in the loan-guarantee variable before 2004 is relevant for our results, 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 the group of non-reporting firms that in fact were extending guarantees even before 2004, although of course we might also be excluding some firms that genuinely started to provide guarantees in 2004. Our results are qualitatively similar to the baseline results when we restrict the sample in this way.

Even though industry peers' punishments do not drive our baseline results, the "punish one, teach a hundred" mechanism is likely also operative at the industry level. Table A.3 in the Online Appendix shows that SOE firms strongly cut loan guarantees to the first punishment of a SOE firm in the same industry. These results underscore the relevance of peer firms along the industry dimension rather than only at the spatial dimension. We will come back to this point below.

Table A.5 in the Online Appendix also shows the baseline results are robust to different subsamples.

Specifically, in columns (1) to (3), we focus on a subsample starting in 2004 after the CSRC provided formal guidance requiring the disclosure of loan guarantees. In columns (4) to (6), we focus on a subsample ending in 2012 before the start of the Anti-corruption campaign.

## **B 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 (Bertrand et al. (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 et al. (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.

Column (1) of Table 4 reports the results for estimating the baseline specification on the collapsed sample, where we only include firms that report values for loan guarantees before the punishment.<sup>21</sup> If anything, the size of the estimated coefficient of interest is larger in absolute value relative to the baseline estimates in Table 2.

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 4 the baseline effect builds up slightly over time in terms of magnitude.

## **C 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 SOE CEOs' career concerns. 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

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<sup>21</sup>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.

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, whereas a younger CEOs would not only lose the same retirement benefits but also the costs related to a likely stop to their career path, including a potential future political career, which is often the step SOE CEOs aim to reach over time.<sup>22</sup>

Panel A of Table 5 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.<sup>23</sup> 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 compared to before and compared 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 concern with these results is the likelihood that punishment might be larger for younger CEOs than for older CEOs, for instance because older SOE CEOs have stronger connections with the party leadership, and hence younger CEOs react more for this reason.<sup>24</sup> To assess this alternative explanation, we compute the probability of punishment for young and old CEOs separately in our sample. The unconditional likelihood of punishment is 0.70% for young CEOs and 0.73% for retiring CEOs, defined as above, and a t-test for whether these probabilities are equal cannot reject the null at any standard level of significance. Similarly, we cannot reject the null that the probabilities are the same across young and retiring CEOs when we condition the sample within SOEs and within non-SOEs.

The second proxy to capture variation in SOE's 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 are likely to have been signed by the previous CEO and executive team of the firm. SOE CEOs with a long tenure at the firm at the time a peer SOE is punished, instead, should find it more costly to cut loan guarantees because they themselves were likely directly involved in signing such contracts with private related parties. Thus, we would expect that recently-appointed CEOs might react more to the direct experience of a local peer punishment.

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<sup>22</sup>We also verify in untabulated results that SOEs with young versus old CEOs or CEOs with long or short tenure do not differ systematically based on observable characteristics.

<sup>23</sup>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.

<sup>24</sup>We thank Stefan Zeume for suggesting this alternative explanation.

Panel B of Table 5 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 CEOs who were appointed up to two years before the event.<sup>25</sup>

## VI Alternative Explanations: Party Connections and Industries

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. Moreover, 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 local 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 the private and public sector is not publicly available. We build on Nie et al. (2017) and Nie et al. (2018), who develop a prefecture-level ranking of government-firm relationships based on the value-weighted scores of five variables, among which two are direct proxies for the likelihood of private communication between local party officials and local listed firms. The first variable is the frequency with which local governors visit (and talk to) local firms. The second is the transparency of local governments with respect to administrative activities and financial accounts, including direct transfers and contributions from local firms. Although Nie et al. (2017, 2018) constructed the indexes based on data from 2017 and 2018, they provide anecdotal evidence that government-firm relationships and connections at the local level are highly persistent characteristics of locations and hence the variation in the cross section of prefectures is substantially more relevant than the limited time-series variation within locations. Consistently, the rank correlation of prefectures based

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<sup>25</sup>We also verify in untabulated results that SOEs with young versus old CEOs or CEOs with long or short tenure do not differ systematically based on observable characteristics.

on the two variables for 2017 and 2018 is higher than 0.85.

We use the prefecture-level ranking of government-firm relationships in 2017 (Nie et al. (2017)) to estimate triple-interaction specifications, whereby we add a source of cross-sectional variation based on (1) whether publicly listed firms are located in prefectures in which the frequency with which government officials visit local firms is above the median of the sample (Panel A of Table 6); and (2) whether firms are located in prefectures in which the index of transparency of local government interactions with firms is below the median (Panel B of Table 6). If our findings were driven by direct private communications between SOEs and the government 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 where private communication with the local government is more intense.

Table 6 reveals that, across the board, we fail to detect any systematic differences between SOEs with different likelihoods of private communication with local governments in terms of their reactions to peers' punishment. Private communication between SOEs and the local government, although important for many other outcomes, does not appear to be a materially relevant channel to explain our results.

## **B Product-Market Dynamics? Geographic Peers vs. 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 7, we include the two definitions of peers — the usual geographic dummy 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 baseline estimated 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 Table A.4 of the Online Appendix 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 8 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 differentially over time around peers' punishment and across local SOEs and non-SOEs.

### A Investment

Columns (1)-(3) of Table 9 report the results for estimating equation (4) 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 the economic magnitude, the differential drop in investment for SOEs after

the peer punishment is about 1.6-3.7 percentage points, which corresponds to 10.5-24.2% of a standard deviation of fixed assets over total assets in the running 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 the SOE's management cut investment in 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 (4).

We run this analysis in columns (4)-(6) of Table 9. 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 7.15% and 10.72% 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, SOEs CEOs might cut loan guarantees to related parties just because the punishment of a peer produces media coverage of loan guarantees as a form of wrongdoing in listed companies. At the same time, the management might engage in different and more opaque forms of tunnelling and ultimately keep the extent of transfers to private related parties unchanged.

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 available resources after the SOE's peer punishment.

Table 10 reports the results of estimating equation (4) using the outcomes of related parties as the dependent variable. In columns (1)-(3), the outcome variable is a dummy variable 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 10, 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 design, we compare the reactions of local peer Chinese SOEs to those of non-peer SOEs and peer non-SOEs after the punishment of the same peer SOE 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 permanent or does it revert over time, which would require multiple interventions? Also, is this mechanism 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.

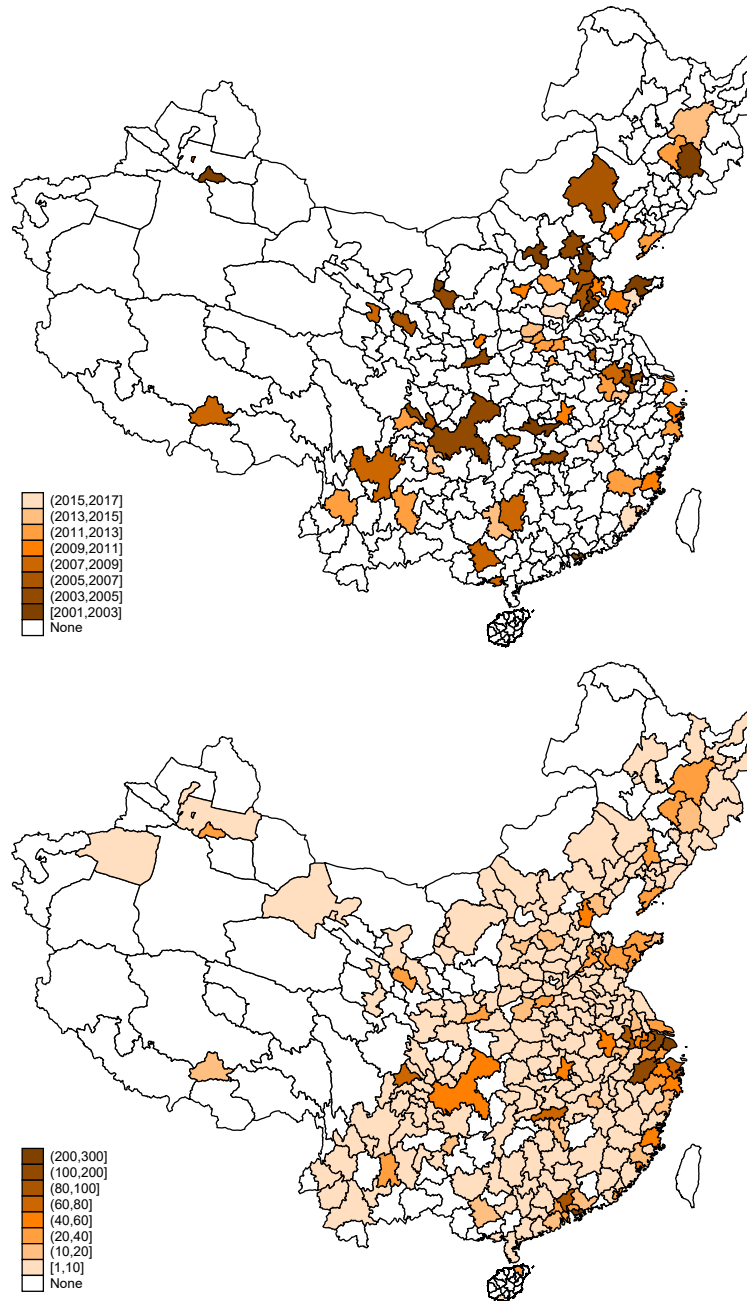
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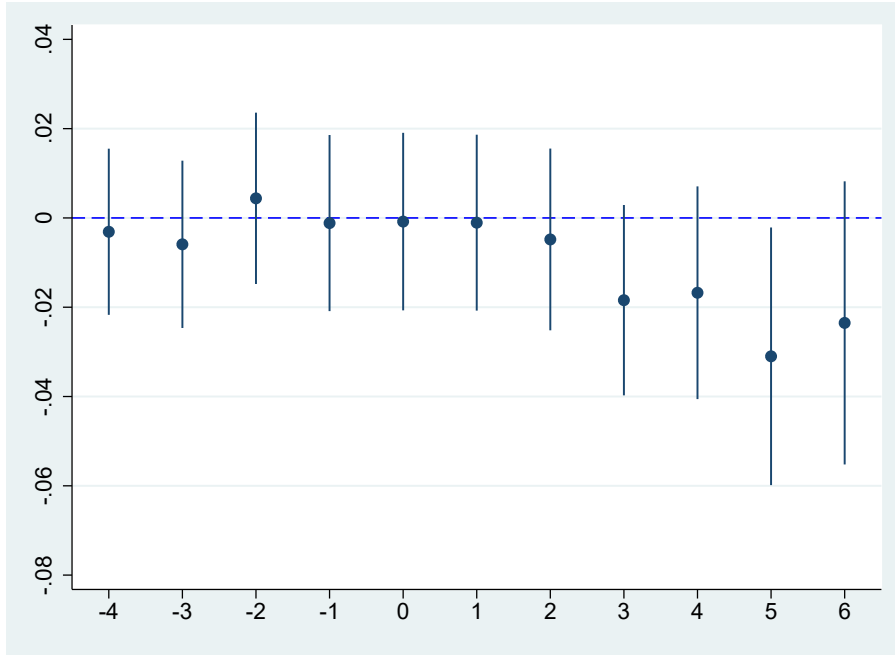
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Figure 1: Time of First Punishment and Number of Firms at the Prefecture Level



*This figure plots the time of the 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.*

Figure 2: **Parallel-Trends Assumption: Pre-trends**



This figure plots the estimates of  $\beta_t$  from the following linear equation

$$\text{Loan Guarantees}_{i,p,t} = \alpha + \sum_t \beta_t \text{SOE}_{i,p,t} \times \text{Year}_t + \gamma_1 \text{SOE}_{i,p,t} + \sum_t \gamma_{2,t} \text{Year}_t + X' \delta + \eta_t + \eta_p + \epsilon_{i,p,t},$$

where  $\text{Loan Guarantees}_{i,p,t}$  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*).  $\sum_t \beta_t \text{SOE}_{i,p,t} \times \text{Year}_t$  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 ( $X$ ), prefecture fixed effects ( $\eta_p$ ), and year fixed effects ( $\eta_t$ ). 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. In Panels A, B, and C, the sample period is from 1997 to 2018. Panel D refers to the firm×year sample of all private related parties linked to a listed firm in our main sample. The sample period for Panel D is 2005 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. Long-term Debt (Related Parties) is the long-term leverage ratio of related parties. We winsorize financial variables at the 1% and 99% levels.*

Variable	N	Mean	Std	p10	p50	p90
<b>Panel A. Main Sample</b>						
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
<b>Panel B. SOE Sample</b>						
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
<b>Panel C. Non-SOE Sample</b>						
After Peer Punishment	16,263	0.288	0.453	0.000	0.000	1.000
Total Guarantees/Assets	15,747	0.097	0.190	0.000	0.000	0.317
Total 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	14,954	2.054	1.258	1.100	1.643	3.423

Variable	N	Mean	Std	p10	p50	p90
<b>Panel D. Related Parties</b>						
Long-term Debt (Related Parties)	22,886	0.048	0.186	0.000	0.000	0.169
After Peer Punishment	38,041	0.369	0.483	0.000	0.000	1.000
SOE	38,041	0.000	0.000	0.000	0.000	0.000
Ln (Assets)	37,575	22.418	1.282	20.903	22.266	24.212
Long-term Debt	35,933	0.056	0.080	0.000	0.021	0.163
Cash /Assets	37,569	0.170	0.116	0.058	0.139	0.329
Tobin's Q	35,937	1.835	0.973	1.052	1.508	2.999

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

This table reports estimates of  $\beta$  from the following linear equation:

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

where  $\text{Loan Guarantees}_{i,p,t}$  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 ( $\text{Provided Guarantees}/\text{Assets}$ );  $\text{Provided Guarantees}/\text{Assets} > 10\%$  is a dummy variable that equals 1 if  $\text{Provided Guarantees}/\text{Assets}$  exceeds 10%, and zero otherwise;  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  in prefecture  $p$  was an SOE in year  $t$ , and zero otherwise;  $\text{After Peer Punishment}_{p,t}$  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  $Q$ ;  $\eta_i$  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.

	Provided Guarantees/Assets			Provided Guarantees/Assets > 10%		
	(1)	(2)	(3)	(4)	(5)	(6)
After Peer Punishment	-0.002 (-0.19)	0.001 (0.15)		-0.004 (-0.23)	0.004 (0.20)	
After Peer Punishment × SOE	-0.015 ** (-2.11)	-0.020 *** (-2.78)	-0.033 *** (-3.36)	-0.052 *** (-3.38)	-0.059 *** (-3.32)	-0.101 *** (-4.49)
SOE	-0.013 *** (-2.74)	0.018 *** (2.66)	0.023 *** (3.25)	-0.031 *** (-2.78)	0.034 ** (2.08)	0.052 *** (2.94)
Ln(Assets)	0.006* (1.66)	0.018 *** (3.16)	0.013 ** (2.20)	0.016* (1.82)	0.048 *** (3.41)	0.036 ** (2.32)
Long-term Debt	0.114 *** (4.00)	0.076 *** (2.92)	0.078 *** (2.89)	0.249 *** (3.70)	0.124 ** (2.13)	0.140 ** (2.19)
Cash	-0.082 *** (-6.37)	-0.031 ** (-2.58)	-0.027 ** (-2.11)	-0.250 *** (-8.03)	-0.128 *** (-4.22)	-0.110 *** (-3.47)
Tobin Q	-0.004* (-1.81)	0.001 (0.26)	-0.000 (-0.14)	-0.016 *** (-3.01)	-0.002 (-0.44)	-0.004 (-0.65)
Constant	0.017 (0.20)	-0.281 ** (-2.14)	-0.212 (-1.65)	0.075 (0.37)	-0.703 ** (-2.18)	-0.576* (-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 <sup>2</sup>	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: Loan Guarantees to Related Parties after Peer’s Punishment — Robustness

This table reports estimates of  $\beta$  from the following linear equation:

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

where  $\text{Loan Guarantees}_{i,p,t}$  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 ( $\text{Provided Guarantees}/\text{Assets}$ );  $\text{Provided Guarantees}/\text{Assets} > 10\%$  is a dummy variable that equals 1 if  $\text{Provided Guarantees}/\text{Assets}$  exceeds 10%, and zero otherwise;  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  in prefecture  $p$  was an SOE in year  $t$ , and zero otherwise;  $\text{After Peer Punishment}_{p,t}$  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  $Q$ ;  $\eta_i$  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.

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

This table reports estimates of  $\beta$  from the following linear equation:

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

where *Loan Guarantees*<sub>*i,p,t*</sub> 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*<sub>*i,p,t*</sub> is a dummy variable that equals 1 if listed company *i* was an SOE every year in period *t*, and zero otherwise; *After Peer Punishment*<sub>*p,t*</sub> 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*;  $\eta_p$  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. Column (1) averages across the full pre and post sample but requires firms to exist pre-punishment, and columns (2) to (8) study samples with varying windows over which we average observations at the firm level. The overall sample period is from 1997 to 2018.

	Firms Exist							
	Pre-punishment	t-2 - t+1	t-2 - t+2	t-2 - t+3	t-2 - t+4	t-2 - t+5	t-2 - t+7	t-2 - t+10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A. Provided Guarantees/Assets</b>								
After Peer Punishment × SOE	-0.063*** (-4.10)	-0.008 (-0.96)	-0.015 (-1.57)	-0.024** (-2.38)	-0.025** (-2.50)	-0.034*** (-3.42)	-0.042*** (-3.10)	-0.040*** (-3.04)
Observations	1,379	1,303	1,320	1,339	1,357	1,371	1,380	1,386
Adjusted R <sup>2</sup>	0.17	0.14	0.14	0.15	0.14	0.14	0.14	0.15
<b>Panel B. Provided Guarantees/Assets &gt; 10%</b>								
After Peer Punishment × SOE	-0.125*** (-3.57)	-0.052* (-1.83)	-0.061** (-2.24)	-0.073*** (-2.67)	-0.068** (-2.55)	-0.073*** (-3.01)	-0.087*** (-2.90)	-0.080** (-2.48)
Observations	1,379	1,303	1,320	1,339	1,357	1,371	1,380	1,386
Adjusted R <sup>2</sup>	0.22	0.16	0.16	0.16	0.17	0.17	0.16	0.16
Controls Table 2	X	X	X	X	X	X	X	X
Prefecture Fixed Effects	X	X	X	X	X	X	X	X
Time Fixed Effects	X	X	X	X	X	X	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 – Heterogeneous Incentives to React**

*This table reports estimates of  $\beta$  from the following linear equation:*

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

where  $\text{Loan Guarantees}_{i,p,t}$  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 ( $\text{Provided Guarantees}/\text{Assets}$ );  $\text{Provided Guarantees}/\text{Assets} > 10\%$  is a dummy variable that equals 1 if  $\text{Provided Guarantees}/\text{Assets}$  exceeds 10%, and zero otherwise;  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  in prefecture  $p$  was an SOE in year  $t$ , and zero otherwise;  $\text{After Peer Punishment}_{p,t}$  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  $Q$ ;  $\eta_i$  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. In Panel A,  $\text{Retiring}_{i,p}$  is a dummy variable that equals 1 if the CEO of firm  $i$  in prefecture  $p$  is within two years of the legal retirement age, and zero otherwise. In Panel B,  $\text{Recent}_{i,p}$  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. The sample period is 1997 to 2018.

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

This table reports estimates of  $\beta$  from the following linear equation:

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

where  $\text{Loan Guarantees}_{i,p,t}$  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);  $\text{Provided Guarantees/Assets} > 10\%$  is a dummy variable that equals 1 if Provided Guarantees/Assets exceeds 10%, and zero otherwise;  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  in prefecture  $p$  was an SOE in year  $t$ , and zero otherwise;  $\text{After Peer Punishment}_{p,t}$  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  $Q$ ;  $\eta_i$  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. In Panel A, Government Characteristics $_p$  for firm  $i$  located in prefecture  $p$  is measured as the frequency with which local government officials visit local firms. High Frequency is a dummy variable that equals 1 if the frequency is above the median value of the sample distribution, and zero otherwise. In Panel B, Government Characteristics $_p$  is measured as the transparency of a local government. High Transparency is a dummy variable that equals 1 if the transparency score is above the median value of the sample distribution, and zero otherwise. The sample period is 1997 to 2018.

	(1)	(2)	(3)	(4)	(5)	(6)
	Provided Guarantees/ Assets			Provided Guarantees/Assets > 10%		
<b>Panel A. Frequency of Local Politicians Visiting Local Firms</b>						
After Punishment $\times$ SOE $\times$ High Frequency	0.001 (0.09)	0.001 (0.05)	0.016 (0.86)	-0.009 (-0.27)	-0.007 (-0.23)	0.035 (0.76)
After Punishment $\times$ SOE	-0.018 (-1.38)	-0.028 ** (-2.52)	-0.050 ** (-3.39)	-0.050* (-1.67)	-0.065 ** (-2.60)	-0.131 *** (-3.27)
After Punishment $\times$ High Frequency	0.002 (0.14)	0.008 (0.52)		-0.007 (-0.22)	-0.005 (-0.15)	
SOE $\times$ High Frequency	0.008 (0.88)	0.029 *** (3.13)	0.030 ** (2.08)	0.018 (0.88)	0.045* (1.75)	0.034 (0.91)
Observations	20,819	20,819	20,819	20,819	20,819	20,819
Adjusted R <sup>2</sup>	0.16	0.41	0.39	0.16	0.38	0.35
<b>Panel B. Transparency of Local Government</b>						
After Punishment $\times$ SOE $\times$ High Transparency	-0.013 (-0.94)	0.002 (0.11)	0.006 (0.29)	-0.035 (-1.20)	0.014 (0.40)	0.008 (0.17)
After Punishment $\times$ SOE	-0.010 (-0.88)	-0.024 ** (-2.05)	-0.036 ** (-2.21)	-0.034 (-1.56)	-0.069 *** (-2.69)	-0.103 *** (-2.71)
After Punishment $\times$ High Transparency	0.014 (0.93)	0.005 (0.34)		0.019 (0.58)	-0.012 (-0.36)	
SOE $\times$ High Transparency	0.013* (1.74)	0.017* (1.67)	-0.002 (-0.13)	0.016 (0.86)	0.010 (0.37)	-0.022 (-0.63)
Observations	20,819	20,819	20,819	20,819	20,819	20,819
Adjusted R <sup>2</sup>	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	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 7: Horse Race Test: Geographic Peers vs. Industry Peers

This table reports estimates of  $\beta$  from the following linear equation:

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

where  $\text{Loan Guarantees}_{i,p,k,t}$  is the amount of loan guarantees extended by firm  $i$  in prefecture  $p$  in industry  $k$  in year  $t$  to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets (Provided Guarantees/Assets);  $\text{Provided Guarantees/Assets} > 10\%$  is a dummy variable that equals 1 if Provided Guarantees/Assets exceeds 10%, and zero otherwise;  $\text{SOE}_{i,p,k,t}$  is a dummy variable that equals 1 if listed company  $i$  in prefecture  $p$  and industry  $k$  was an SOE in year  $t$ , and zero otherwise;  $\text{After Geographic Peer Punishment}_{p,t}$  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;  $\text{After Industry Peer Punishment}_{k,t}$  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;  $X$  is a set of firm-level characteristics that include the logarithm of total assets, long-term leverage, cash over assets, and Tobin's  $Q$ ;  $\eta_i$ ,  $\eta_{p,t}$ ,  $\eta_{k,t}$  represent full sets of firm, prefecture-by-year, and industry-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 ( $p$ ). We define industries based on the CSRC 2001 classification. The sample period is 1997 to 2018.

	(1)	(2)	(3)	(4)	(5)	(6)
After City Peer Punishment $\times$ SOE	-0.012* (-1.70)	-0.017** (-2.58)	-0.020** (-2.08)	-0.048*** (-2.89)	-0.054*** (-2.74)	-0.077*** (-3.18)
After City Peer Punishment	-0.003 (-0.39)	0.000 (0.03)		-0.007 (-0.36)	0.003 (0.12)	
After Industry Peer Punishment $\times$ SOE	-0.018** (-2.37)	-0.023*** (-3.26)	-0.027** (-2.09)	-0.032** (-2.17)	-0.035* (-1.96)	-0.038 (-1.45)
After Industry Peer Punishment	-0.002 (-0.20)	-0.001 (-0.17)		0.000 (0.02)	0.001 (0.08)	
SOE	-0.001 (-0.13)	0.028*** (4.11)	0.034*** (3.51)	-0.001 (-0.13)	0.049*** (3.04)	0.062*** (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	
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 <sup>2</sup>	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 8: Loan Guarantees to Related Parties after Peer’s Punishment: The Role of Saliency**

This table reports estimates of  $\beta$  from the following linear equation:

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

where  $\text{Loan Guarantees}_{i,p,t}$  is the amount of loan guarantees extended by firm  $i$  in prefecture  $p$  in year  $t$  to any related parties scaled by the previous end-of-the-fiscal-year assets ( $\text{Provided Guarantees}/\text{Assets}$ );  $\text{Provided Guarantees}/\text{Assets} > 10\%$  is a dummy variable that equals 1 if  $\text{Provided Guarantees}/\text{Assets}$  exceeds 10%, and zero otherwise.  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  was an SOE in year  $t$ , and 0 otherwise;  $\text{Saliency}_{i,p}$  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;  $X$  is a set of firm-level characteristics that include the logarithm of total assets, long-term leverage, cash over total assets, and Tobin’s  $Q$ ;  $\eta_i$  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.

	Provided Guarantees/ Assets			Provided Guarantees/ Assets > 10%		
	(1)	(2)	(3)	(4)	(5)	(6)
After Peer Punishment $\times$ SOE $\times$ Saliency	-0.036 (-1.42)	-0.041* (-1.94)	-0.043* (-1.85)	-0.067 (-1.51)	-0.098 ** (-2.24)	-0.135*** (-2.64)
After Peer Punishment $\times$ SOE	-0.014 ** (-2.04)	-0.019*** (-2.65)	-0.030*** (-3.01)	-0.052 *** (-3.25)	-0.061 *** (-3.17)	-0.096*** (-4.15)
SOE $\times$ Saliency	0.033 *** (2.89)	0.037* (1.79)	0.028 (1.12)	0.087 ** (2.57)	0.142 *** (3.05)	0.139*** (2.67)
After Punishment $\times$ Saliency	0.033 (1.49)	0.039** (2.40)		0.057 (1.25)	0.100 *** (3.20)	
Controls Table 2	X	X	X	X	X	X
Year Fixed Effect	X	X	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 <sup>2</sup>	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 9: Investment and TFP After Peer's Punishment

This table reports estimates of  $\beta$  from the following linear equation:

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

where  $\text{Real Outcome}_{i,p,t}$  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  $t$ ;  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  in prefecture  $p$  was an SOE in year  $t$ , and 0 otherwise;  $\text{After Peer Punishment}_{p,t}$  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 assets, and Tobin's  $Q$ ;  $\eta_i$  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.

	Fixed Assets /Assets			TFP		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.007 (1.25)	0.012 ** (2.14)		-0.048 (-0.86)	-0.038 (-0.95)	
After Punishment $\times$ SOE	-0.021 ** (-2.40)	-0.016 ** (-2.38)	-0.011* (-1.65)	0.058 (0.68)	0.120 ** (2.18)	0.181 *** (2.83)
SOE	0.044 *** (6.52)	0.007 (1.48)	0.007 (1.33)	0.018 (0.29)	0.003 (0.08)	-0.014 (-0.38)
Ln(Assets)	-0.016 *** (-3.46)	-0.021 *** (-4.69)	-0.019 *** (-4.15)	0.914 *** (29.21)	0.694 *** (24.39)	0.688 *** (23.48)
Long-term Debt	0.056 (1.23)	0.029 (0.81)	0.016 (0.41)	1.611 *** (5.32)	0.449* (1.91)	0.378 (1.33)
Cash	-0.156 *** (-10.10)	-0.138 *** (-9.84)	-0.152 *** (-9.62)	-0.149 (-1.09)	0.728 *** (6.52)	0.651 *** (4.63)
TobinQ	-0.005 ** (-2.47)	-0.004 ** (-2.54)	-0.004 *** (-2.61)	0.027 (1.44)	0.102 *** (9.26)	0.107 *** (10.56)
Constant	0.466 *** (4.47)	0.597 *** (5.74)	0.610 *** (5.98)	-20.307 *** (-28.07)	-15.451 *** (-23.81)	-15.412 *** (-24.20)
Year Fixed Effects	X	X	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 <sup>2</sup>	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 10: Related-Party Borrowing after Peer Punishment

This table reports estimates of  $\beta$  from the following linear equation:

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

where *Related Party Outcome*<sub>*i,p,t*</sub> 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*<sub>*i,p,t*</sub> 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*<sub>*p,t*</sub> 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;  $\eta_i$  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 (0.98)	0.023 (0.85)		-0.003 (-0.61)	-0.001 (-0.23)	
After Peer Punishment × SOE	-0.051 (-1.42)	-0.067 ** (-2.05)	-0.082 ** (-2.07)	-0.011 ** (-2.28)	-0.007 ** (-2.11)	-0.010 ** (-2.51)
SOE	-0.055 ** (-2.02)	0.017 (0.58)	0.036 (0.99)	0.011 *** (2.90)	0.002 (0.68)	0.004 (1.36)
Ln(Assets)	0.027 *** (3.38)	0.064 *** (3.18)	0.078 *** (4.34)			
Long-term Debt	0.142 (1.41)	0.036 (0.27)	0.059 (0.39)			
Cash	-0.356 *** (-4.89)	-0.181 ** (-2.26)	-0.165 ** (-2.10)			
Tobin Q	-0.004 (-0.56)	0.011 (1.29)	0.011 (1.35)			
Ln(Assets) (Related Party)				0.015 *** (21.24)	0.010 *** (7.29)	0.010 *** (5.85)
Current Liability (Related Party)				-0.075 *** (-16.85)	-0.117 *** (-11.74)	-0.112 *** (-11.45)
Constant	0.104 (0.59)	-0.778* (-1.72)	-1.052 *** (-2.68)	-0.099 *** (-13.04)	-0.021 (-1.15)	-0.009 (-0.43)
Year Fixed Effect	X	X	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 <sup>2</sup>	0.12	0.46	0.42	0.22	0.67	0.69

t-statistics in parentheses

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

Online Appendix:

Punish One, Teach a Hundred:  
The Sobering Effect of Punishment on the Unpunished

*Not for Publication*

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 private 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,目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。

经审理查明,湘酒鬼存在如下违法行为:1999年11月至2000年6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(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长曹宏杰,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担保展期承诺书,为湘泉集团2002年到期两笔贷款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。原任董事长、现任董事樊耀传代公司签发了上述协议,对对上述违法行为负有直接责任的主要人员。

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《债权债务协议》(工行编号[2003]001号),约定湘酒鬼承接湘泉集团所欠工行贷款本息5082万元,在贷款到期前,由工行与湘酒鬼另行签订《借款合同》,由湘西自治州经济开发建设公司担保,湘酒鬼承接的5000万元贷款,由深圳利源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息16000万元提供担保等。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要人员,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至、现任董事付光明。

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

本院认为,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,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“发生重大事件的情况应当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、杨建至、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:中国实业银行总行营业部、账号71110101890000102),由该行直接上缴国库,并将付款凭证复印件送中国证券监督管理委员会法制部审理执行处备案。如对本处罚决定不服,可在收到本处罚决定书之日起40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书之日起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。

中国证监会

二〇〇四年七月

证监会

二十七

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

This table reports estimates of  $\delta$  from the following linear equation:

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

where  $\text{Punishment Prefecture Year}_{p,t}$  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;  $\eta_p$  and  $\eta_t$  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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Log GDP	-0.020 (-1.17)														-0.039 (-1.01)
Employment Rate		-0.000 (-0.90)													0.000 (0.65)
Log Population			-0.038 (-0.53)												-0.046 (-0.49)
Log Population Density				0.002 (0.12)											0.001 (0.04)
GDP% in Heavy Manufacturing					0.033 (0.45)										-0.215 (-1.20)
GDP% in Light Manufacturing						-0.043 (-0.77)									-0.083 (-0.69)
Fiscal Deficit							-0.000** (-2.22)								-0.000 (-0.89)
Government-Market Connectivity Index								0.001 (0.07)							-0.004 (-0.39)
Non-SOE Development Index									-0.001 (-0.27)						0.000 (0.05)
Product-Market-Development Index										0.000 (0.08)					-0.001 (-0.11)
input-Market-evelopment Index											0.003 (1.12)				0.004 (1.02)
Financial Intermediation& Law Index												0.000 (0.23)			-0.000 (-0.00)
Log Number of Public Firms													-0.003 (-0.21)		0.005 (0.18)
SOE as a Percentage of Total # firms															0.006 (0.53)
Constant	0.161 (1.24)	0.039 (1.21)	0.238 (0.56)	0.001 (0.01)	0.006 (0.70)	0.029 (1.14)	0.020** (2.51)	0.032 (0.59)	0.047 (1.10)	0.032 (0.67)	0.015 (0.70)	0.033* (1.72)	0.015 (0.61)	0.010 (1.40)	0.653 (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  $\beta$  from the following linear equation:

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

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;  $\eta_p$  and  $\eta_{t+n}$  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 (1)	n=2 (2)	n=3 (3)	n=4 (4)	n=5 (5)
First Punishment in Prefecture (Year $t$ )	-0.067* (-1.82)	-0.091*** (-3.41)	-0.057 (-1.64)	-0.065 (-1.49)	-0.028 (-0.64)
Constant	-0.021 (-0.03)	-0.238 (-0.27)	-0.270 (-0.47)	-0.279 (-0.40)	0.661 (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

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



Table A.3: **Loan Guarantees to Related Parties after non-SOE and Industry Peer's Punishment**

This table reports estimates of  $\beta$  from the following linear equation:

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

where  $\text{Loan Guarantees}_{i,p,t}$  is a dummy variable that equals 1 if Provided Guarantees/Assets exceeds 10%, and zero otherwise; Provided Guarantees/Assets 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;  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  in prefecture  $p$  was an SOE in year  $t$ , and zero otherwise;  $\text{After Peer Punishment}_{p,t}$  is a dummy variable that equals 1 if prefecture  $p$  has faced at least one punishment of a locally headquartered non-SOE as of year  $t$  in columns (1) to (3) and zero otherwise and a dummy 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  $t$  in columns (4) to (6) 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  $Q$ ;  $\eta_i$  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 ( $p$ ). We define industries based on the CSRC 2001 classification..The sample period is 1997 to 2018.

	non-SOE Punishment			Industry-Peer Punishment		
	(1)	(2)	(3)	(4)	(5)	(6)
After Peer Punishment	0.026 (1.20)	0.028* (1.91)		0.015 (0.77)	0.034* (1.92)	
After Peer Punishment $\times$ SOE	-0.003 (-0.10)	-0.031 (-1.33)	-0.023 (-0.74)	-0.034* (-1.83)	-0.069 *** (-4.41)	-0.052 *** (-2.92)
SOE	-0.144 *** (-13.36)	0.020 (1.28)	0.019 (1.02)	-0.016 (-1.04)	0.049 *** (3.46)	0.032 ** (2.12)
Ln(Assets)	0.058 *** (8.34)	0.045 *** (3.22)	0.034 ** (2.10)	0.021 *** (3.09)	0.046 *** (4.96)	0.042 *** (4.60)
Long-term Debt	0.161 ** (2.35)	0.119 ** (1.99)	0.143 ** (2.16)	0.246 ** (2.23)	0.120 (1.46)	0.132* (1.89)
Cash	-0.197 *** (-6.89)	-0.121 *** (-3.82)	-0.106 *** (-3.06)	-0.239 *** (-7.09)	-0.141 *** (-4.71)	-0.138 *** (-4.55)
Tobin Q	0.013 *** (3.07)	-0.001 (-0.14)	-0.000 (-0.01)	-0.015 *** (-4.53)	-0.000 (-0.03)	-0.007 (-1.45)
Constant	-1.005 *** (-6.69)	-0.667 ** (-2.05)	-0.534 (-1.49)	-0.042 (-0.27)	-0.800 *** (-3.93)	-0.680 *** (-3.38)
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	20,695	20,695	20,695	19,916	19,916	19,916
Adjusted R <sup>2</sup>	0.08	0.38	0.35	0.16	0.38	0.39

t-statistics in parentheses

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

Table A.4: Loan Guarantees after Peer Punishment: No Industry Peers

This table reports estimates of  $\beta$  from the following linear equation:

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

where  $\text{Loan Guarantees}_{i,p,t}$  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 ( $\text{Provided Guarantees}/\text{Assets}$ );  $\text{Provided Guarantees}/\text{Assets} > 10\%$  is a dummy variable that equals 1 if  $\text{Provided Guarantees}/\text{Assets}$  exceeds 10%, and zero otherwise;  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  in prefecture  $p$  was an SOE in year  $t$ , and zero otherwise;  $\text{After Peer Punishment}_{p,t}$  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  $Q$ ;  $\eta_i$  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 ( $p$ ). We exclude firms operating in the same industry from firms that are punished for wrongdoings. 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 Peer Punishment	0.001 (0.07)	0.001 (0.11)		-0.006 (-0.33)	0.001 (0.04)	
After Peer Punishment $\times$ SOE	-0.019 ** (-2.29)	-0.019 ** (-2.26)	-0.031 *** (-2.80)	-0.054 *** (-2.97)	-0.048 ** (-2.43)	-0.095 *** (-3.67)
SOE	-0.009* (-1.84)	0.016 ** (2.27)	0.021 ** (2.56)	-0.024* (-1.94)	0.033 ** (2.03)	0.052 *** (2.75)
Ln(Assets)	0.002 (0.70)	0.017 *** (3.19)	0.011* (1.84)	0.010 (1.13)	0.051 *** (3.75)	0.037 ** (2.28)
Long-term Leverage	0.092 *** (3.70)	0.057 ** (2.42)	0.056 ** (2.34)	0.181 *** (2.82)	0.068 (1.23)	0.074 (1.13)
Cash/Assets	-0.076 *** (-5.74)	-0.029 ** (-2.47)	-0.023* (-1.89)	-0.240 *** (-7.45)	-0.139 *** (-4.72)	-0.112 *** (-3.51)
Tobin's Q	-0.004* (-1.75)	0.002 (0.94)	0.002 (0.63)	-0.016 *** (-3.05)	0.000 (0.02)	-0.001 (-0.20)
Constant	0.095 (1.17)	-0.304 *** (-2.61)	-0.174 (-1.35)	0.219 (1.13)	-0.888 *** (-2.99)	-0.591* (-1.68)
Year Fixed Effect	X	X	X	X	X	X
Prefecture Fixed Effect	X			X		
Firm Fixed Effect		X	X		X	X
Prefecture-Year Fixed Effect			X			X
Observations	17,836	17,836	17,836	17,836	17,836	17,836
Adjusted R <sup>2</sup>	0.16	0.40	0.38	0.16	0.38	0.36

t-statistics in parentheses

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

**Table A.5: Loan Guarantees to Related Parties after Peer’s Punishment: Post Guidance and Pre Anti-corruption Campaign**

This table reports estimates of  $\beta$  from the following linear equation:

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

where  $\text{Loan Guarantees}_{i,p,t}$  is a dummy variable that equals 1 if Provided Guarantees/Assets exceeds 10%, and zero otherwise;  $\text{Provided Guarantees/Assets}$  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;  $\text{SOE}_{i,p,t}$  is a dummy variable that equals 1 if listed company  $i$  in prefecture  $p$  was an SOE in year  $t$ , and zero otherwise;  $\text{After Peer Punishment}_{p,t}$  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  $Q$ ;  $\eta_i$  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 ( $p$ ). Columns (1) to (3) focus on a sample starting in 2004 and columns (4) to (6) focus on a subsample ending in 2012. The overall sample period is 1997 to 2018.

	Sample 2004 – 2018			Sample 1997 – 2012		
	(1)	(2)	(3)	(4)	(5)	(6)
After Peer Punishment	−0.007 (−0.34)	−0.005 (−0.21)		−0.001 (−0.04)	−0.001 (−0.04)	
After Peer Punishment × SOE	−0.044 ** (−2.57)	−0.042* (−1.73)	−0.080 *** (−2.76)	−0.042 ** (−2.18)	−0.042 ** (−2.46)	−0.101 *** (−4.05)
SOE	−0.044 *** (−2.88)	0.025 (1.45)	0.043 ** (2.27)	−0.028 *** (−3.10)	0.017 (0.95)	0.036* (1.86)
Ln(Assets)	0.016 (1.54)	0.057 *** (3.67)	0.045 *** (2.67)	0.013 ** (2.07)	0.028 ** (2.02)	0.020 (1.36)
Long-term Debt	0.296 *** (3.59)	0.150 ** (2.17)	0.196 ** (2.59)	0.175 *** (2.84)	0.098 (1.52)	0.057 (0.87)
Cash	−0.276 *** (−7.64)	−0.122 *** (−3.20)	−0.103 *** (−2.63)	−0.203 *** (−7.35)	−0.088 *** (−2.76)	−0.077 ** (−2.31)
Tobin Q	−0.016 *** (−2.84)	0.005 (0.95)	0.003 (0.52)	−0.008 (−1.38)	−0.001 (−0.09)	0.002 (0.26)
Constant	0.090 (0.39)	−0.928 *** (−2.61)	−0.757 ** (−2.01)	0.026 (0.18)	−0.331 (−1.07)	−0.308 (−0.94)
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	17,186	17,186	17,186	12,963	12,963	12,963
Adjusted R <sup>2</sup>	0.14	0.41	0.39	0.12	0.32	0.27

t-statistics in parentheses

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