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

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

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Abstract

Direct experience of a peer's punishment might make non-punished peers reassess the probability and consequences of facing punishment and hence induce a change in their behavior. We test this mechanism in a setting, China, in which we observe the reactions to the same peer's punishment by listed firms with different incentives to react – state-owned enterprises (SOEs) and non-SOEs. After observing peers punished for wrongdoing in loan guarantees to related parties, SOEs – which are less disciplined by traditional governance mechanisms than non-SOEs – cut their loan guarantees. SOEs whose CEOs have stronger career concerns react more than other SOEs to the same punishment events, a result that systematic differences between SOEs and non-SOEs cannot drive. SOEs react more to events with higher press coverage even if information about all events is publicly available. After peers' punishments, SOEs also increase their board independence, reduce inefficient investment, increase total factor productivity, and experience positive cumulative abnormal returns. The bank debt and investment of related parties that benefited from tunneling drop after listed peers' punishments. Strategic punishments could be a cost-effective governance mechanism when other forms of governance are ineffective.

JEL classification: D91, D72, G32, G41, K42.

Keywords: Corporate Governance, Cultural Finance, Reputational Sanctions, Related Party Transactions, Minority Shareholders, Emerging Markets, Corporate Fraud, Government Ownership.

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I Introduction

From Ancient Rome to Mao's China, philosophers have argued that observing punishment might have a sobering effect on the behavior of non-punished peers.[1] According to this mechanism, decision-makers update their beliefs upwards regarding the probability of being punished and the negative effects of punishment once they observe both dimensions due to a peer's punishment. Decision-makers might react by reducing unilaterally any existing wrongdoing to avoid punishment. A priori, this mechanism can be consistent with both Bayesian updating—peers' punishments might help firms assess the probability and consequences of punishment events—and non-Bayesian channels that imply overreaction to salient signals.

We propose an empirical setting to test for the reaction-to-peer-punishment mechanism. Our decision-makers are Chinese public firms. We focus on public firms, which are more likely to comply with regulation than private firms (Slutzky (2018)).

Chinese public firms that head business groups often use intra-group transfers and loan guarantees to allocate resources to private related parties (e.g., Berkman, Cole, and Fu (2009); Jia, Shi, and Wang (2013)).[2] But on top of alleviating intra-group financial constraints, loan guarantees are also used as a means to tunnel resources from public companies to private related parties. In this mechanism, a private related party takes a loan from a bank despite a high risk of default, and in case of default, the wealth of the public company's minority shareholders will be dissipated to the benefit of the majority shareholders who have a stake in the private related party.[3] Over the last two decades, Chinese regulators have increased the punishment of fraudulent related-party transactions involving listed firms (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.[4]

Testing for this mechanism while controlling for local shocks that correlate with punishment events is empirically challenging. Comparing the behavior of all non-punished peers before and after a peer's punishment would not be meaningful, because any time-varying shock that correlates with the peer's punishment might explain the results. Instead, we need to observe two peers of a punished firm, one of which is more prone to updated its beliefs about punishment than the other after observing the same punishment event, all else equal. In this way, we could compare the reactions of two peers that operate in the same location, at the same time, and hence are exposed to the same local time-varying shocks, but one firm faces a higher treatment effect from observing the common punished peer than the other.

We argue that the Chinese setting allows for such a research design. We exploit the fact that Chinese

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

[2] For the effects of related-party transactions on other real and accounting outcomes, see also Jian and Wong (2010); Cheung, Rau, and Stouraitis (2006).

[3] See Fisman and Wang (2010), Jian and Wong (2003), Peng et al. (2011), and Liu and Zhang (2017).

[4] Jiang, Lee, and Yue (2011) document the effectiveness of these punishment measures in cutting the wrongdoing in punished firms. We show that these punishments are unrelated to the recent anti-corruption campaign, whose potential effects on firm-level outcomes are still debated (e.g., see Griffin, Liu, and Shu (2017); Li, Wang, and Zhou (2017); Goh, Ru, and Zou (2018); Lin et al. (2016)).

listed firms include both state-owned enterprises (SOEs)—whose controlling shareholder is the central government or local governments—and non-SOEs. In the data, both SOEs and non-SOEs are punished because of wrongdoings on loan guarantees.[5] In particular, the trends in punishments across SOEs and non-SOEs have tracked each other over time. Moreover, punishments are evenly distributed over time and about 81% of the punishments in our sample period happened before the recent anti-corruption campaign started, which makes our results largely unrelated to this campaign.

Ex-ante, SOE CEOs' beliefs of being punished are likely to be lower than those of non-SOEs, because SOE CEOs might expect protection from regulators through their connections to the party (Chen, He, and Liu (2017)). Moreover, the ultimate government ownership insulates SOEs more than non-SOEs from traditional internal and external governance mechanisms that restrict the scope for minority expropriation, such as shareholder activism (Gantchev, Gredil, and Jotikasthira (2018); Brav, Jiang, and Kim (2015); Fos (2016)), board monitoring, governance through trading, or cross ownership (He, Huang, and Zhao (2019)). For these reasons, we would expect that the salience of peers' punishment makes SOE CEOs update their subjective beliefs of being punished more relative to non-SOE CEOs.

Based on these arguments, in our first research design, we compare the reactions of SOEs and non-SOEs to common peers' punishments. We define peers based on geography—listed firms that operate in the same prefecture are peers. Below, we discuss in more detail how the geographic peer definition, relative to other definition based for instance on industry, can help us capture the effects of information spillovers across firms instead of product-market spillovers. This geographic peer definition allows us to compare the reactions of listed SOEs and listed non-SOEs operating in the same location of a punished firm and in the same year, and hence facing the same local supply and demand shocks.

A concern with our first research design is that SOEs and non-SOEs differ along several dimensions, including their regulation, the status of their CEOs within the party, and the fact that the financial sector treats SOEs more favorably when they apply for loans (Li, Wang, and Zhou (2017); Cong, Gao, Ponticelli, and Yang (2019)). Moreover, one might worry that the initial levels of loan guarantees differ systematically across SOEs and non-SOEs. If they were lower for SOEs, this might explain why SOEs react less than non-SOEs. These differences are a concern for our first research design if they vary systematically across time and over space, because controlling for the SOE status of firms washes away any time-invariant systematic differences between SOEs and non-SOEs.

To tackle this potential concern directly, we propose a second empirical design that only exploits variation in CEOs' incentives to react after observing a peer's punishment *within* the group of SOEs. The variation we exploit in this design does not rely on comparing SOEs with non-SOEs, and hence abstracts for all time-varying systematic differences between SOEs and non-SOEs.

When we implement our first design, we find that after Chinese regulators punish a listed firm for tunneling via inter-corporate loan guarantees, unpunished listed peer SOEs cut their loan guarantees

[5] Contrary to widespread beliefs, SOEs do get punished. As we discuss in more detail below, 33% of the punishments we exploit in our analyses involve SOEs.

to related parties significantly more relative to listed peer non-SOEs and to any listed firms in other locations. This effect is economically and statistically significant. After a peer is punished, SOEs reduce the amount of loan guarantees over total assets by 2.4 percentage points—about 25% of a standard deviation of the scaled loan guarantees in the sample. Moreover, non-punished peer SOEs are more likely than non-punished peer non-SOEs to implement changes in their governance that favor minority shareholders. For instance, they are 43% more likely to move from CEO duality—the CEO chairs the board – to a more independent board structure.[6] These results suggest that SOEs react strongly to the punishment of local peers by aligning their actions with the interests of minority shareholders. Instead, local non-SOEs barely react.[7]

Our baseline results survive a set of robustness tests, such as excluding the largest Chinese prefectures and cities from the analysis, limiting our tests to the prefectures that experienced at least one punishment between 1997 and 2014, as well as fixing the SOE status of firms at the time in which they experience the punishment of a local peer. The last test is important because the Chinese government implemented a massive wave of privatization of SOEs 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. We also perform a falsification test, whereby we assign placebo dates of peers' punishment to prefectures randomly and fail to replicate the baseline results.

We then move on to our second research design, which only exploits variation in the incentives to react within the group of SOE CEOs. Here, we use two proxies for CEOs' incentives to react to the peer's punishment. First, we consider CEOs' age. Because China imposes mandated retirement at the age of 60, SOE CEOs that are closer to retirement age face weaker career concerns relative to younger SOE CEOs (Karpoff et al. (2008)). Older CEOs might face restrictions to their retirement packages in case they are punished. But younger CEOs, on top of the same restrictions to retirement packages, would also face 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.[8]

Consistent with this variation in the incentives to react based on career concerns, we find that older SOE CEOs react less than younger SOE CEOs to the punishment of the same peers in the same locations. Crucially, we do not find any systematic differences in the likelihood that firms headed by young or old CEOs are punished, either in the full sample or when we split firms between SOEs and non-SOEs.

As a second test to exploit only variation in the incentives to react within SOEs, we consider SOE CEOs that have ever held managerial jobs overseas relative to other SOE CEOs. The rationale for this test is that SOE CEOs with foreign experience might have an easier access to the international CEO

[6]Note that moving from a duality to an independent board structure is not necessarily efficient. For instance, Yang and Zhao (2014) document that duality makes firms quicker and more effective in reacting to changing competition or other shocks.

[7]This result is consistent with the fact that traditional governance mechanisms might already restrict the scope for wrongdoing in non-SOEs.

[8]In the context of members of the board of directors in Chinese listed firms, Jiang, Wan, and Zhao (2015) also focus on younger directors as those with stronger career concerns.

labor market through the connections they created while working abroad (Giannetti et al. (2015)). If they were punished in China, they could move abroad and proceed their managerial career there, and hence face weaker career concerns relative to SOE CEOs without overseas experience. Consistently, we find that SOE CEOs with overseas experience react less than other SOE CEOS to the punishment of the same peers in the same locations.

All the results so far might be consistent with Bayesian or non-Bayesian updating. On the one hand, because punishments are rare events, any realization of a punishment (extensive margin) as well as observation of the size of the negative consequences of punishments (intensive margin) might cause substantial updating about both dimensions. This updating could be rationally higher for SOE managers than non-SOE managers if the former group thought their companies were less likely to be punished. At the same time, SOE managers might overreact to salient peers' punishments and change their policies based on a non-Bayesian updating process (Dessaint and Matray (2017)).

In the second part of the paper, we assess whether all our results are fully consistent with Bayesian updating. To this aim, we note that all listed firms, whether local or not and whether SOEs or non-SOEs, have access to the same public information regarding punishments, which they can observe from the China Security Regulatory Commission's (CSRC) official press releases and other public releases. Under Bayesian updating, we would expect variation in the objective characteristics of the punishment event to drive SOEs' reaction. Instead, non-neoclassical reaction based on salience predicts SOEs react more to more salient events, irrespective of the events' characteristics.

We propose two proxies for the salience of events: (i) the share of the total number of news stories about a punished firm in Chinese media outlets in the 60 days around the punishment announcement covering the punishment and (ii) the cumulative abnormal returns (CARs) of punished peers' stock prices in the days around the punishment announcement.

Because information about the punishment events if made public on the CSRC website and CEOs and their collaborators are sophisticated agents, we would not necessarily expect a stronger reaction to events covered relatively more by the media.[9] At the same time, external factors are likely to drive media coverage of otherwise similar events at least in part, such as natural catastrophes or the government's willingness to restrict the amount of information about the punishment of certain firms but not others. Overall, we find that SOEs' reaction to peers' punishments is about twice as large after events that are more salient relative to punishment events that are less salient.

Reducing loan guarantees after a peer's punishment might have no far-reaching effects on SOEs. At the same time, if peer firms want to avoid minority shareholders selling their shares, they might engage in additional costly actions to signal they are run transparently. Consistent with this second conjecture, SOEs whose peers are punished move to more independent boards after they observe the punishment.

[9]Note media coverage is decided by outlets also based on potentially strategic reasons (Hope, Li, Liu, and Wu (2018)). In this case, we might expect that media outlets decide to limit the coverage of punishments of SOEs given their sensitive role in the economy and the nature of their shareholders. We do not detect any systematic under-reporting of SOE punishments relative to non-SOE punishments.

Moreover, SOEs' investment drops by 1.1 percentage point, which is 18% of the average change in assets in the sample. Such cuts in investment might have positive or negative effects on shareholder value. We find the cuts lead to significant improvements in total factor productivity (TFP) in the medium run, which suggests SOEs were arguably not employing the firms' resources efficiently before the punishment events (Giannetti et al. (2017); Lagaras et al. (2017)). Event studies corroborate this interpretation because SOEs' CARs are positive in the days around the punishment events and substantially higher than the (statistically insignificant) CARs of non-SOEs after peers' punishments.

In the third part of the paper, we assess directly a set of concerns and potential alternative interpretations of our results. First, listed SOEs might shift to other forms of tunneling after a peer's punishment, which would still allow private related parties to obtain credit and invest in real assets. Potential shifting to other forms of tunneling would still suggest peers' punishments have an effect on firms' behavior. At the same time, such shifts might undermine the sobering effect of peers' punishments.[10] One extreme form of shifting to other forms of tunneling is the non-disclosure of loan guarantees to regulators (Piotroski et al. (2015)). Despite the rules in place, the managers of listed firms might fraudulently avoid disclosing the guarantees they provide to banks in favor of related private parties.[11]

To understand whether the sobering effect of peers' punishment is a real phenomenon, we test whether private related parties do indeed reduce their external financing and investment in real assets, which before the peers' punishments they could obtain through loan guarantees. Consistent with a sobering effect, we show the drop in loan guarantees has substantial negative real effects on SOEs' related parties, which cut their investment and reduce bank borrowing significantly.

Another potential mechanism for our results is that after a punishment, local governments might engage in acts of moral suasion ("private directives") toward local SOEs.[12] For instance, local governments might tell SOEs they should change their policies to avoid future punishment. This moral suasion might be more likely for SOEs than non-SOEs, because SOEs typically have stricter connections with local governments (Haveman, Jia, Shi, and Wang (2017); Huang et al. (2016)). Moral suasion is consistent with our mechanism. Based on this channel, governments' direct intervention in the management of SOEs would increase the expected likelihood punishment, as opposed to the mere observation of peer firms that are punished.

To assess whether punishments are less likely if local governments have stronger incentives to engage in moral suasion, for example, in prefectures whose local economy depends more on SOEs, we run predictive regressions for punishment events using a large set of observables that earlier literature has argued might capture whether local governments want to avoid SOE punishment. Such observables include the local government's fiscal deficit, whether a mayor or local party secretary was appointed around the year of punishment (Ru (2018)), and a set of indices capturing the development of the SOE

[10] We thank Stefan Zeume for proposing this potential mechanism.
[11] We thank Da Ke for suggesting this possibility.
[12] We thank Da Ke for suggesting this mechanism.

sector, local product markets, and financial markets (Fan et al. (2011); Fan et al. (2016)). We do not detect any significant association between these dimensions and the likelihood of punishment events at the prefecture-by-year level.

A third set of concerns relates to a potential direct effect of punishments on the industrial organization of markets in which peer firms operate (Stanfield, Zhang, and Zhang (2018)). If punished firms are direct competitors of non-punished peers, the latter group might gain market shares and potentially increase markups, thus earning higher profits per unit sold. Positive CARs and higher TFP of peers after the punishment might be consistent with this interpretation. At the same time, this interpretation seems less able to explain why peer firms' governance becomes more transparent, as well as the baseline outcome we consider – why peer firms cut their loan guarantees, which has real effects even on related parties. We provide a direct assessment of the relevance of this competition channel in a specification in which peers are not defined based on their geographic location, but on their industry. Under the competition story, peers that operate in the same industry as the punished firm should drive the effect. We find no effect of the punishments on SOE or non-SOE peers' loan guarantees or other outcomes when we define peers based on industry instead of geographic location, which seems inconsistent with the competition channel.

Another industrial-organization interpretation relates to the transmission of negative shocks through the supply chain. As we discuss in more detail in Section II, SOEs are more likely to operate in upstream industries than in downstream industries. Because peers' punishments include both SOEs and non-SOEs, the punishment of non-SOEs might represent a negative shock to a relevant downstream customer of SOEs. Such negative shocks might propagate upstream. For instance, the punished customer might stop paying for the goods it purchased or might cancel existing orders. But this shock would have a *negative* effect on the local upstream SOE peers, which should result in negative CARs for SOE peers around the events (Cohen and Frazzini (2008); Ozdagli and Weber (2017)). Instead, we document positive CARs for SOE peers around the punishment events.

Overall, our evidence is consistent with a sobering effect of observing peers' punishment on the behavior of Chinese listed SOEs. An interesting feature of peers' punishment as a corporate governance mechanism is cost effectiveness. Under this mechanism, regulators would only need to monitor and punish a small set of listed firms to obtain broad compliance, which reduces dramatically the costs of monitoring listed firms on the part of regulators and activist shareholders. This form of governance could thus be especially viable in settings like China in which more traditional forms of governance are less effective than in other settings (e.g., Allen, Qian, and Qian (2005)). Punishing the wrongdoing of one firm reduces the scope for misbehavior of peer firms without any need to monitor or investigate peer firms directly.

A Related Literature

Our paper contributes to several strands of literature in finance and political economy. First, we relate to the recent body of work studying the causes and consequences of managerial wrongdoing (e.g., Dyck, Morse, and Zingales (2010); Dyck, Morse, and Zingales (2016); Zeume (2017); Bennedsen and Zeume (2017)). 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* peer firms instead of quantifying the direct and indirect effects of sanctions on punished firms.[13] A Bayesian interpretation of our results suggests peers' punishments make firms update the probability of punishment, for instance because firms believe regulators increased their willingness to punish wrongdoings related to loan guarantees. This channel would be consistent with Desai, Dyck, and Zingales (2007), in which governments' enforcement actions change the size of the private benefits managers might enjoy when tunneling, and hence modify the equilibrium amount of tunneling. More broadly, our paper contributes to our understanding of the effects of deterrence on criminal activity and wrongdoing (see Chalfin and McCrary (2017) for a recent review). We contribute to this body of work by showing that the salience of deterrence initiatives, and not only the size and generality of such initiatives, affects the potential wrongdoing of non-punished agents. Indeed, every Chinese listed firm observes the punishments and has full information about them, but only the local geographic peers react and reaction is stronger for more salient punishment events, as captured by media coverage and market reaction.

Our results speak to existing 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)), the geographic proximity of firms (e.g., see Parsons et al. (2018)), the enforcement preferences of regulators (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 all firms, irrespective of their location, observe fully. For this reason, punishments as governance mechanisms are cheap because they do not require the collection and elaboration of private information and could apply to developing countries, in which shareholder activism is still not highly diffused given the absence of large independent 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 of governance mechanisms in the presence of blockholders include wolf-pack activism (Brav, Dasgupta, and Mathews, 2017) and shareholder coalitions (D'Acunto, 2016). Managers might appropriate or destroy shareholder value if internal governance mechanisms, such as board oversight, are ineffective (Hermalin and Weisbach, 2017). External mechanisms such as governance through trading might also be ineffective, especially if the government is a majority shareholder and does

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

not care about fluctuations in stock prices (Edmans and Manso, 2010).

Governments as blockholders are common in emerging markets as well as in firms in developed markets that belong to strategic industries such as energy, defense, and aerospace.[14] To safeguard the rights of minority shareholders, stock-market regulators monitor listed firms and penalize wrongdoing, producing both direct and reputational negative effects on the punished firms (Armour, Mayer, and Polo, 2017). Often, though, active monitoring and punishing is too costly and time consuming to allow their effective universal use. In this paper, we contribute to this line of research by studying an external governance mechanism that does not require shareholder activism, is valid when the threat of governance through trading is ineffective, and is not based on universal regulatory monitoring. Our mechanism exploits the direct observation of the reputational damage managers *could* face in case of punishment.

More broadly, 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)).

We also contribute to the recent literature on the effects of salience on decision-making. Theories exist explaining how the salience of environmental characteristics affects economic decision-making with and without risk (Gennaioli and Shleifer (2010); Bordalo et al. (2012); Bordalo et al. (2013)). Researchers in economics and finance have also employed the salience of environmental characteristics in experimental and field settings to test for the effects of such characteristics on individual decision-making (e.g., Benjamin et al. (2010); D'Acunto (2018); D'Acunto (2017); D'Acunto, Malmendier, Ospina, and Weber (2019)). Dessaint and Matray (2017) are the first to test for overreaction to salient events in corporate finance. They find managers accumulate cash holdings to insure their firms against disaster risk after observing the effects of a natural disaster on firms close by, which increases managers' expected probability of disasters through salience of disaster risk. Managers then dissipate these precautionary cash accumulations over time, which suggests they overreacted to the salient events. In our setting, both Bayesian and non-Bayesian reactions to peers' punishment might help explain the results, although the stronger SOEs' reaction to more salient than to less salient events might be consistent with non-Bayesian updating.

II Institutional Setting

In this section, we discuss two important features of our institutional setting. First, we describe the process through which SOEs emerged in China. The origin of the difference between SOEs and non-SOEs is especially important to our first empirical design (see Section V), which compares the reaction of SOEs

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

and non-SOEs to the same shocks. Our second empirical design instead relies only on comparing the reactions to punishment events by different SOEs operating in the same location and year. Second, we describe the prevalence of loan guarantees from Chinese listed firms to private subsidiaries, and we discuss why such loan guarantees can represent a form of tunneling resources at the expense of listed firms' minority shareholders.

A SOEs and Business Groups in China

The Chinese government imposed the transition from a Communist economic system to a market economy in several stages, starting with the first set of reforms after 1978. A crucial tenet of the reforms 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.

Although in the first phase of market reforms the government maintained strict direct control over the economy for decades, it also promoted a gradual, experimental, and pragmatic approach to improve the performance of corporations (Lin (2009); Xu (2011)). To maintain control over economic activity while allowing for private ownership, the 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).

Upstream sectors in the networked hierarchy were still organized as government-controlled monopolies through SOEs, which created the notion of "grasp the large, let go the small" to indicate the government's interest in maintaining the ownership of large monopolies in upstream industries and let smaller and downstream SOEs become private (Song and Xiong (2018)).

An important feature of the gradualism that characterizes Chinese economic and ownership structure reforms is the convergence of the characteristics of SOEs and non-SOEs in at least two aspects. First of all, private businesses are often closely connected to local governments and exploit such connections to ease financial constraints or obtain other type of advantages (Bai et al. (2018)) in a bank-centric financial system. Moreover, gradualism has forced large SOEs to introduce innovations such as basic forms of corporate governance and more sound evaluation systems for employees (Song and Xiong (2018)).

In 1992, the Chinese government started the second stage of economic reforms. A large-scale wave of privatization in downstream sectors characterized this second stage. In the early 2000s, some upstream SOEs also started to be gradually privatized. Between 2001 and 2004, the number of SOEs in China decreased by 48%. This period was also characterized by a substantial opening of the Chinese economy to international trade. The Chinese government reduced trade barriers, implemented major reforms of its banking system, and joined the World Trade Organization (WTO). In 2005, China's domestic private sector exceeded 50% of overall corporate ownership for the first time (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. Importantly, surviving SOEs were still protected from foreign competition following the WTO entry. By contrast, non-SOEs faced fierce competition in downstream tradables sectors, which are 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.

This brief analysis of the coexistence of SOEs and non-SOEs in China suggests that, despite a slow convergence of the characteristics of these two types of firms in terms of corporate governance and efficiency, SOEs and non-SOEs still have different incentives to react to governance threats in China. We exploit these differences in our first empirical design and abstract from these differences in our second empirical design, which focuses exclusively on the reaction of different types of SOEs to the same punishment events and similar SOEs to different punishment events.

B Loan Guarantees to Related Parties

The Chinese government engages in a strict monitoring of the banking system mainly through its central bank (People's Bank of China, PBOC) and the China Banking Regulatory Commission (CBRC). The banking system is one of the key sectors in the networked hierarchy underlying China's state capitalism. The dominant players are the four largest state-owned commercial banks, which primarily lend to SOEs, because of both political preference and because SOEs tend to have larger amounts of collateral assets to guarantee their loans. As regulators, practitioners, and academics have widely recognized, loans to SOEs by the major Chinese banks account for the largest part of the nonperforming loans in China.

The focus of our paper is on the role of guarantees in firms' ability to raise loans from the Chinese banking system. Unlike other countries, where governments use guarantees to finance small firms or support homeownership (e.g., see D'Acunto et al. (2018)), the role of guarantors in individual loans falls to individual firms in China. Fisman and Wang (2010) describe in detail the mechanisms through which Chinese corporations tunnel resources to related private parties through loan guarantees. For the scope of our analysis, loan guarantees that a listed company makes to a private related party can be a form of tunneling resources to the majority shareholders at the expenses of its minority shareholders. Suppose a private party related to the majority shareholder(s) of a listed company asks for a guarantee to obtain a loan to finance a wasteful project that produces private benefits to all its shareholders, including the majority shareholder(s) of the listed company. The latter party thus has an incentive to ensure the listed company guarantees the loan with banks to allow the related private party to invest in the project. 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 due to the need to pay back banks for the defaulted loan they had guaranteed.

Before 2007, SOEs were the most frequent users of guarantees to back loans for their under-capitalized subsidiaries or units. Since 2007, the central government has urged banks to expand lending to small enterprises. Because the cost of doing due diligence is high relative to the value of a small loan, banks usually insist that in the absence of sufficient collateral, someone else guarantees the loan. Private companies often struggle to form the so-called "guarantee chain" to obtain credit from state-owned banks. During that period, a quarter of the loans in China's banking system were backed by guarantees (McMahon, 2014).

A default on a guaranteed loan can result in large systemic events in whole regions through the guarantee chain of the network hierarchy. In August 2003, CSRC issued a notice to regulate guaranteed loans provided by public firms.[15] 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.

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.[16] 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 1997–2014.

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

[15] http://www.csrc.gov.cn/pub/newsite/flb/flfg/bmgf/ssgs/gljy/201012/t20101231_189866.html

[16] 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 CEOs of listed firms.

The punishment-level information harmonized across events includes the date on which a punishment for a firm committing fraud is announced, the regulator that announced the fraud event, the time period during which fraud was committed, the reasons for punishment, the extent of the punishment, and a detailed description of the activities in which the listed company engaged.

Although anecdotal evidence shows the very first fraud 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 (1997-2014), we obtained 254 corporate fraud events involving irregular loan guarantees in which public firms and their related parties were involved.

We observe punishments from four different agencies: the central CSRC, the province-level offices of the CSRC, and the stock exchanges in Shenzhen and Shanghai. Out of the 254 punishments events we observe, the local CSRC offices account for 38% of these events, the central CSRC for 20%, with the remaining punishment events almost equally split across the two stock exchanges. Contrary to widespread beliefs, SOEs do get punished. Out of the 254 punishments, 27% involve SOEs and 33% of the first punishments in a prefecture involve SOEs.

A.1 Properties of the Punishment Events

Figure 1 describes the spatial distribution of the punishment events we use in the empirical analysis. The units in the map are Chinese prefectures, which represent our main unit of analysis to define the local peers – we consider firms headquartered in the same prefecture as local peers. In the top map of Figure 1, the darker a prefecture, the earlier the first punishment event for loan-guarantee wrongdoing of a local listed firm in the prefecture. We observe substantial spatial variation in the timing of the first punishments. Moreover, no substantial spatial clustering of the timing of first punishments is detectable in the map, which suggests concerns about spatial correlation across observations in neighboring prefectures is not relevant in our context.

In terms of the distribution of the events over time, we also fail to detect clustering of events in specific years. In particular, 81% of the first punishment events in a prefecture, which are the events we use in the analysis, happened before 2012. The proportion of punished firms each year is roughly constant and about 2:1 for non-SOEs vs. SOEs. Overall, the events we consider are thus largely unrelated to the anti-corruption campaign implemented under the Xi presidency, and we do not find that SOEs only started to be punished during the anti-corruption campaign.

As we discuss below, our research design does not make the (implausible) assumption that the timing and location of punishment events is randomly assigned at the prefecture level, which would suggest punishments are shocks exogenous to local observables and unobservables. Our design instead exploits the differential incentives to react to peer punishments across different types of firms that face the same contemporaneous local demand and supply shocks similar to parts of the literature on credit-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 the punishment events are exogenous conditional on observables and unobservables related to the local economy.

To assess whether observables might 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, and 0 otherwise, on a large set of potential determinants of punishment events. We collect a set of prefecture-year and province-year variables that, based on earlier research, might be directly or indirectly related to the emergence of punishment events. We consider 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, a dummy for whether the prefecture changed its mayor and/or its local party secretary around the year of the first punishment (Ru (2018)), 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. The following variables are computed at the province-year level: an index of the strength of the government ownership of 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 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, with the notable exception of the share of SOEs operating in the location. If anything, this result seems to suggest punishments are more likely in areas with a high concentration of SOEs, which is hardly consistent with the notion SOEs are never punished because of their connections with local governments.

The results of a last test aim to understand the properties of the distribution of punishment events across space and over time and to assess the extent to which punishment events cluster within locations over time. For instance, one might argue the local regulators or the central CSRC decide to punish a set of firms all operating in the same location because of unobserved strategic or political motivations. In this case, we might worry that our claim that the punishment events are not systematically happening in one location or the other conditional on local observables is implausible. In Table A.2 of the Online Appendix, we thus use a dummy for whether a prefecture had a punishment in year t, to predict the likelihood the same prefecture had a punishment in subsequent years $t+n$ after conditioning on prefecture-level time-varying observables. We consider a horizon of five years after the first punishment in the prefecture.

Overall, we fail to reject the null the emergence of a punishment event in a location is statistically and economically unrelated to the emergence of a punishment event in the same location in any of the subsequent five years.

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 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 nature of the controlling shareholder, or ultimate owner, is an SOE or a non-SOE. 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 two sources for financial information on private related parties. The first is Orbis Asia-Pacific. 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 2014. The second source is the Annual Surveys of Industrial Production (ASIP), conducted by the NBSC. This dataset is the most comprehensive survey data for industrial firms in China. The surveys include all SOEs, and non-SOEs with revenues above 5 million RMB (about US$-600,000). 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.

We consider both the total gross amount of new loan guarantees as well as the net amount of new loan guarantees issued every year, that is, the difference between the amounts guaranteed by a public firm to all related parties and the amounts guaranteed by all related parties to the public firm. Total guarantees are nonnegative, whereas net guarantees can be positive or negative, depending on whether the public firm is a net receiver or net supplier of guarantees within the business group.

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 1996–2015. 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 a prefecture, the higher the number of firms in the prefecture that enter our sample. The map shows the firms that enter our analysis are distributed throughout China, which guarantees 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 – for example, Shanghai and Beijing – or only in special economic zones like Shenzhen, or only in coastal areas.

Table 1 reports the summary statistics for our main variables. Each panel refers to one of the 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 that are headquartered in a prefecture in which at least one listed company was punished by regulators because of irregular loan guarantees to related parties.

The sample is an unbalanced panel at the firm×year level, the longest time span being from 1997 to 2014. $After\ Punishment$ is a dummy variable that equals 1 for firm observations in the years after the first peer in their prefecture was punished. About 42% 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×year observations are SOEs.

Our main object of interest is the extent of loan guarantees listed firms extend to their related private parties, for which we report two alternative definitions. $Total\ Provided\ Guarantees$ is the overall amount of loan guarantees listed firms extend to related parties, scaled by total end-of-previous-year assets. $Net\ Provided\ Guarantees$ is the difference between the loan guarantees listed firms extend to related parties and the sum of the guarantees related parties provided to the listed firms. We winsorize these two variables as well as all continuous variables at the 1% and 99% percent levels to ensure outlier observations do not affect our results.

The rest of Panel A provides statistics for the financial characteristics and other observables we use in the analysis. As far as financials are concerned, $Long - term\ Leverage$, measured as long-term debt over total end-of-previous-year assets, is 6% on average, whereas $Total\ Leverage$, which includes short-term debt and trade credit, is on average 48% (Li et al. (2009)). We also use the share of cash-like instruments over total end-of-previous-year assets, which is 16% on average. Tobin's Q is larger than 1 for both the mean and the median firm in the sample. We define $Capital\ Investment$ as the change in fixed assets from year t-1 to year t, scaled by total assets of the firm as of the end of year t-1. We also construct a measure of TFP following Olley and Pakes (1996). Our main proxy for board independence, $CEO\ Duality$, is a dummy variable that equals 1 if the CEO of the company is the head of the board of directors, and 0 otherwise. Thirteen percent of our observations have dual CEOs.

Panel B of Table 1 refers to the individual-bank-loan dataset for the subset of firms in the main sample for which the data are available. Note we observe more loans in the periods after punishments relative to the distribution of firm×year observation in the main sample (55% > 42%). Moreover, the share of loans SOEs obtain is lower than the share of SOEs in the sample (31% < 50%). The firm-level characteristics weighted at the bank-loan level do not seem to display any other substantial departure from the main sample.

Panel C of Table 1 refers to the firm×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 (61% > 42%), which suggests the number of private firms to which listed firms are related has increased over time. Moreover, SOEs are less likely to have private related parties relative to SOEs' share of the overall sample of firms (34% < 50%). Related parties' leverage over assets, cash over assets, and Tobin's Q are similar to the corresponding dimensions measured for the listed firms in our main sample, in terms of both the average and standard deviation of the distributions.

IV Hypotheses

The formation of individual-level expectations about major but rare negative outcomes, such as natural disasters, blows of reputation, or wars, is complicated because decision-makers often do not have access to any reliable information regarding the distribution of the outcomes whose likelihood they need to assess (see D'Acunto, Rossi, and Weber (2019)). In this context, experienced realizations of such negative outcomes affect decision-makers' expectations. In this paper, we ask whether observing peers' punishments might make agents change their assessment of the probability and consequences of punishment. This mechanism is consistent with Bayesian updating—for instance, agents expect that punishment rates will increase and new punishments are more likely once they observe a punishment event in their location—or it could involve some overreaction to salient and personally experienced realizations of an outcome (Malmendier and Nagel (2009), D'Acunto et al. (2019), Dessaint and Matray (2017)).[17]

A major challenge to an empirical test of this channel in the field is that unobserved fundamental shocks that trigger the punishment of a peer might also increase the likelihood of punishment for the decision-makers of interest. This endogeneity problem is especially compelling when peers are defined based on spatial clustering and hence face the same local economic shocks.

To tackle this empirical challenge, we propose a setting that allows us to predict a differential reaction to the same peer's punishment by two types of local peers who face the same unobserved local economic shocks. In our setting, decision-makers are Chinese listed firms, for which we observe a broad set of outcomes over time. The CEOs of Chinese listed firms observe the punishment of local peers by regulators due to wrongdoing in the tunneling of resources to private related parties through loan guarantees.

The crucial feature of our setting is that for each geographic peer group, we observe two alternative subgroups of firms with different ex-ante probabilities of reacting to the punishment of a local peer. This feature allows us to compare the reactions to salient peer punishments between more receptive and less receptive peers in a setting in which both groups of peers face the same unobserved local economic shocks and trends. Thus, although in our setting the realization of a peer's punishment is *not* an exogenous event with respect to local and potentially unobserved economic conditions, we can compare the reactions to peer punishment across firms for which such unobserved economic conditions are identical.

The two alternative groups of local peers that have different incentives to react to local punishments are listed SOEs and listed non-SOEs. We expect SOE CEOs react more than non-SOE CEOs for at least two reasons. First, as we argue in the introduction, governance mechanisms that reduce non-SOEs managers' incentives to engage in wrongdoing are muted for SOE managers. Even if peer punishment increased the expected probability of punishment also for non-SOEs, traditional governance mechanisms would already restrain non-SOE CEOs from engaging in wrongdoing, and hence peer punishment might have no material consequence on their behavior.

Second, before observing peer punishment, SOE CEOs might expect regulators are unlikely to investigate and punish their firms. In the data, we confirm SOEs are, on average, less likely to be punished than non-SOEs. Conditional on punishment, SOEs benefit from higher "regulatory tolerance," in the sense that punished SOEs had perpetrated wrongdoing for longer than punished non-SOEs. Punishment of peer firms thus is likely to increase the expected likelihood of punishment and extent of losses due to punishment more for SOE CEOs than for non-SOE CEOs.

Based on these considerations, we formulate the first hypothesis we bring to the data:

[17] Below, we discuss non-expectations-based channels through which punishments might affect peers' outcomes – e.g., changes in the competitiveness of local product markets and shocks to the supply chain – and we discuss why these alternative channels are unlikely explanations for our results.

Hypothesis 1 – Loan Guarantees after Peer Punishment

After punishment of a peer firm, SOE peers are more likely to reduce loan guarantees to related parties than non-SOE peers operating in the same location.

One could worry systematic differences between SOEs and non-SOEs might cause them to react differently to the same local economic shocks, which might be the ultimate drivers of both punishments and outcomes. To assess this concern, we develop additional hypotheses that only exploit the *intensive margin of reaction* – the size of the predicted reactions within the group of SOEs. Because these hypotheses do not rely on comparing SOEs with non-SOEs, systematic differences across these two types of firms are irrelevant.

The intensive-margin test exploits heterogeneity in incentives to react by SOE CEOs due to differences in career concerns, because punishments include personally-targeted sanctions as we discuss above (see Figure A.1 in the Online Appendix). When deciding whether to reduce loan guarantees, the CEOs of listed firms will plausibly assess the consequences of being caught for shareholder value but also for their own career prospects. Personal motives matter because the CEO of a punished firm might face reputation damages and lower employability after facing punishment. To proxy for the severity of CEOs' career concerns, in Section VI, we introduce and motivate in detail two proxies. The first proxy is based on the age of the CEO. Intuitively, CEOs who are closer to retirement have weaker career concerns than younger CEOs, and hence older CEOs should react less to the punishment of local peers relative to younger CEOs for whom a punishment would loom more in terms of subsequent career opportunities (Karpoff, Lee, and Martin (2008)). The second proxy is based on the size of the job market that CEOs can access. The rationale is CEOs with managerial experience in China and abroad can access the international managerial job market and have more potential job opportunities than CEOs who spend their whole career in China (Giannetti, Liao, and Yu (2015)):

Hypothesis 2 – Heterogeneity across SOEs – CEOs' Career Concerns

After punishment of a peer firm, SOE peers whose CEOs have higher career concerns reduce loan guarantees to related parties by more than other SOE peers and than non-SOE peers operating in the same location.

If salience explained our baseline results, we should find a stronger reaction to more salient punishment events than to less salient events. The empirical challenge to test this hypothesis rests in defining plausible proxies for the salience of a peer punishment. In Section VI, we introduce and motivate in detail two proxies that aim to capture different aspects of the salience of punishment events. The first proxy is based on the size of the CARs of punished firms around the punishment event (*market reaction*). The second proxy is based on the relative coverage of the punishment events in the Chinese national and local media around the announcement of punishments (*media reaction*):

Hypothesis 3 – Heterogeneity across SOEs – Salience of Punishment

After facing more salient punishments of a peer firm, SOE peers reduce loan guarantees to parents and subsidiaries by more than SOE peers facing less salient punishments and than non-SOE peers operating in the same location.

The hypotheses we have proposed so far are aimed at establishing our baseline facts: that (i) SOEs react more to peers' punishment relative to non-SOEs operating in the same locations, and (ii) this variation is arguably driven by the higher incentives of SOE CEOs to react to peer punishment as opposed to systematic time-invariant or time-varying differences across SOEs and non-SOEs.

If CEOs decided to cut loan guarantees after peers' punishments to avoid potential future punishment and the perception they might engage in barely legal business practices, the same CEOs might also take additional costly steps to signal to shareholders, the public, and regulators that they run their company transparently. One such potential costly signal we can observe in our data is the CEO's decision to increase the independence of the board by abolishing CEO duality in the firm. Dual CEOs are at the same time the top executive and the chairman of the board of directors. By eliminating duality, CEOs allow for higher independence of the board at the expense of not controlling the board directly:

Hypothesis 4 – Costly Signals: CEO Duality and Governance

After punishment of a peer firm, SOE peers are more likely to abolish CEO duality than non-SOE peers operating in the same location.

Moverover, agency theory suggests CEOs might engage in empire building by investing in projects to enlarge the size of the firm independent of the effect on shareholder value. If CEOs truly reacted to peers' punishments by increasing their efforts to maximize shareholder value, we should observe that, after peers' punishment, SOE CEOs cut inefficient investment more than non-SOE CEOs. Clearly, we cannot assess what component of investment is efficient or inefficient. We therefore propose two hypotheses to assess this prediction indirectly. The first step tests whether a potential drop in the investment by SOE CEOs is accompanied by an increase in the productive use of inputs – TFP:

Hypothesis 5 – Real Outcomes after Peer Punishment

After punishment of a peer firm, SOE peers are more likely to decrease investment and increase TFP than non-SOE peers operating in the same location.

Intuitively, if investment was efficient before the peer's punishment, firm-level TFP should decrease or at most not change after a cut in investment. Instead, if investment was wasteful before the peer's punishment, we should observe that after a drop in investment, TFP increases because resources are employed more efficiently after the cut.

So far, we have only considered balance-sheet outcomes. But if CEOs engaged in tunneling of resources at the expense of minority shareholders, we might expect a change in the market value of firms after a stop in value-destroying activities:

Hypothesis 6 – Market Value after Peer Punishment
After punishment of a peer firm, the CARs of SOE peers are higher than the CARs of non-SOE peers operating in the same location.

The last hypothesis we consider aims at verifying that cuts in loan guarantees to related parties do indeed affect the financing and investment policies of such parties. This test is important, because if cutting loan guarantees did not relate to any change in the financing and investment of related parties, one might worry SOE CEOs would start engaging in more opaque forms of tunneling after the peers' punishment relative to before. Although this interpretation also contains a role for observing peers' punishment – CEOs would reassess the risks associated with loan guarantees only after observing the punished peer – the cut in loan guarantees would not be associated with any reduction in tunneling. Instead, if CEOs really decided to lower tunneling, we should find that after a peer's punishment, the availability of credit and the investment of private related parties drops. We therefore test the following hypothesis:

Hypothesis 7 – Financing and Real Outcomes of Related Parties
After punishment of a peer firm, the amounts of debt and investment decrease more for the related parties of SOE peers than for the related parties of non-SOE peers operating in the same location.

V Empirical Strategies

To test the hypotheses described above empirically, we propose two strategies. The first strategy is a difference-in-differences research design. We compare a set of yearly outcomes measured at the peer firm level before and after the first time a listed firm is punished in a prefecture and across listed SOE peers and listed non-SOE peers operating in the same prefecture. The double difference we aim to assess is therefore as follows:

$$(Outcome_{SOE,p,after} - Outcome_{SOE,p,before})$$
$$- (Outcome_{non-SOE,p,after} - Outcome_{non-SOE,p,before}),$$

where p indicates the Chinese prefecture in which the SOE and non-SOE peers operate, which determines the peer status in our setting.

To implement this strategy, we will estimate a set of linear specifications that restrict the variation allowed to estimate coefficients in different ways. Our most restrictive specification is as follows:

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

where the coefficient β captures the double difference defined above after partialling out firm-level characteristics ($X_{i,t}$) as well as firm and prefecture-by-year fixed effects (η_i and η_{pt}). 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 no fixed effects and when adding separate prefecture and year fixed effects. These less restrictive specifications exploit variation in the SOE status of firms in the cross-section as opposed to variation in SOE status within firm over time. They also allow us to assess the stability of our results.

In all the specifications in 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.

Note that our research design will not make the (implausible) assumption that the timing and location of punishment events is randomly assigned at the prefecture level, which would suggest punishments are shocks exogenous to local observable and unobservable characteristics. Our design instead exploits the differential incentives to react 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.

Our first empirical strategy might raise the concern SOEs and non-SOEs differ in many dimensions that invalidate the design. Note that time-invariant systematic differences between SOEs and non-SOEs in obtaining loan guarantees are already ruled out in the first empirical design. Differences between SOEs and non-SOEs are only a concern to our design if they vary systematically before and after peers' punishment events and in turn determine the differential changes in loan guarantees around the events across these two groups of firms. To address this concern, we propose also a second empirical strategy that exploits the heterogeneity in the response of SOEs with different incentives to react to the same punishment events. This strategy does *not* rely on comparing SOEs' reactions to non-SOEs' reactions, but only exploits the variation in the reaction to the same peer punishment events by different SOEs in the same prefecture. We implement this second strategy using the following type of specification:

$$\begin{aligned}
Outcome_{i,p,t} = \alpha &+ \beta_1 SOE_{i,p,t} \times After\ Peer\ Punishment_{p,t} \times Reactive_{i,p,t} \\
&+ \beta_2 SOE_{i,p,t} \times After\ Peer\ Punishment_{p,t} \times Non\ Reactive_{i,p,t} \\
&+ \gamma_1 After\ Peer\ Punishment_{p,t} \times Reactive_{i,p,t} \\
&+ \gamma_2 After\ Peer\ Punishment_{p,t} \times Non\ Reactive_{i,p,t} \\
&+ SOE_{i,p,t} + Reactive_{p,t} + X'_{i,t}\delta + \eta_i + \eta_{pt} + \epsilon_{i,p,t}.
\end{aligned} \tag{2}$$

In equation (2), the coefficients β_1 and β_2 capture the double difference of the outcomes across listed SOEs and non-SOEs and before and after the first punishment for loan guarantees in the prefecture, computed separately for SOEs whose CEOs have a higher incentive to react to peers' punishments ($Reactive_{i,p,t}$) and for SOEs whose CEOs have a lower incentive to react ($Non\ Reactive_{i,p,t}$). In Section VI, we propose two proxies for CEOs' incentives to react, namely, (i) CEOs' age and distance from the mandated retirement age in China and (ii) CEOs' connections to international managerial job markets as a potential backup in case they were convicted of fraud in China.

When discussing the economic channels behind our results in Section VI, we also estimate a version of equation (2) in which we replace the dummy $Reactive_{i,p,t}$ with two proxies for the salience of punishment events. In this case, we compare the reactions of SOEs to different peer punishment events in different locations and at different times, as opposed to comparing the reactions of different SOEs to the same peer punishment events at the same time, as we do in equation (2).

In terms of specification, a compelling alternative to equation (2) would be a more intuitive triple-interaction specification, by which we augmented equation (1) by adding a triple-interaction term with a dummy for whether the punishment is salient and the full set of double interactions. We prefer the specification in equation (2) that estimates associations separately for reactive and non-reactive SOEs and non-SOEs, because this specification allows us to test directly whether different non-SOEs react differently to the peers' punishment. We can perform this test under the null hypothesis that $\gamma_1 = \gamma_2 = 0$. This test is important because it allows us to corroborate the baseline result that non-SOEs do not react to peers' punishment events and that this result is not due to limited statistical power when estimating equation (1).

A Parallel-Trends Assumption

The validity of our difference-in-differences strategy 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 firm in prefecture p. This *parallel-trends* assumption states the outcomes of the two groups of firms – listed SOEs and listed non-SOEs – would have followed parallel trends throughout the sample period, that is, both before and after the 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 the assumption that we need to interpret the estimate of coefficient β in equation (1) causally, we can at most test whether the trends of outcomes across our treatment and control group are parallel before the punishment year. To test for whether pre-trends are parallel across treatment conditions, we estimate a set of specifications as follows:

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

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 years before the first punishment of a listed firm in prefecture p, and the other variables are defined as in equation (1).[18]

The null hypothesis that pre-trends are parallel across treatment and control groups consists thus in assuming each of the estimated coefficients β_t in equation (3) equals zero. We therefore estimate equation (3) by ordinary least squares, and we test this null hypothesis in the data.

Figure 2 reports the results for estimating the coefficients β_t separately for our two main outcomes of interest, that is, the total amount of loan guarantees scaled by assets and the net amount of guarantees scaled by assets.

In each panel, squares represent the size of the estimated coefficients β_t. The segments around each point represent 2-standard-error confidence bounds. We can see that for both variables, we fail to reject the null hypothesis that any of the estimated $\hat{\beta}_t$ coefficients in the years before the peers' punishment is different from zero, either economically or statistically. This test suggests the trends in our main outcome variables are parallel before the punishment events across listed SOEs and listed non-SOEs headquartered in the same prefecture as the punished listed firm. Note the estimates of the β_t coefficients are noisier for the years further away from the punishment date (t) than for the years closer to the punishment date. The noisier estimates occur because we lose observations before the punishment date for punishments early in our sample.

Although a test for whether the trends would have been parallel after the punishment events had the events not happened is impossible, the inability to detect differential pre-trends reassures us when assuming listed non-SOEs, might represent a viable counterfactual for the behavior of listed SOEs in the same prefecture had the punishment events not happened.

[18]Note that we write the full set of fixed effects η_t of equation (1) as $\sum_t Year_t$ in equation (3) to maintain symmetry with the interaction term in the specification.

Regarding the set of heterogeneity specifications in equation (2), we assess the parallel-trends assumption by comparing the estimated size of the coefficients γ_1 and γ_2, which capture the average outcome variables for salient and non-salient punishment events before the punishment. If these coefficients differ systematically across the two groups, we would worry that time-varying shocks or institutional changes that affect areas with salient versus non-salient events might explain our results differently. As we can see by comparing the two coefficients in columns (1), (2), (4), and (5) of Table 6, no systematic differences seem to exist in terms of either magnitude or statistical significance for the pre-trends across areas that face more or less salient punishment events.

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

Based on our discussion in Section IV, we first consider Hypothesis 1, which argues that after the punishment of a peer, SOEs are more likely to reduce potential wrongdoing – the amount of loan guarantees – than non-SOEs operating in the same prefecture.

We estimate the following linear equation by ordinary least squares:

$$\begin{aligned} Loan\ Guarantees_{i,p,t} &= \alpha + \beta SOE_{i,p,t} \times After\ Peer\ Punishment_{p,t} + \gamma_1 SOE_{i,p,t} \\ &+ \gamma_2 After\ Peer\ Punishment_{p,t} + X_{i,t}\delta + \eta_i + \eta_{pt} + \epsilon_{i,p,t}, \end{aligned} \tag{4}$$

where $Loan\ Guarantees_{i,p,t}$ is the amount of loan guarantees extended by firm i in prefecture p in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,p,t}$ is a dummy variable that equals 1 if listed company i is an SOE in year t, and 0 otherwise; $After\ Peer\ Punishment_{p,t}$ is a dummy variable that equals 1 if prefecture p has faced at least one punishment of a locally-headquartered firm as of year t, and 0 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 η_i and η_{pt} 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.

Based on Hypothesis 1 in Section IV, we predict $\beta < 0$; that is, after the punishment of a peer firm, SOEs in a certain prefecture cut the amount of loan guarantees they extend to related parties by a larger amount than non-SOEs in the same prefecture.

Note that our setting does not provide clear-cut predictions for coefficients γ_1 and γ_2. The null hypothesis that $\gamma_1 = 0$ states that, on average, SOE peers do not extend a higher share of their assets in the form of loan guarantees to related parties compared to non-SOEs. The null hypothesis that $\gamma_2 = 0$ states that after the punishment of a peer firm, non-SOE peer firms do not cut the share of assets they extend in the form of loan guarantees to related parties, compared to the amounts they extended to related parties before the punishment of a peer firm.

Table 2 reports the results for estimating equation (4). In columns (1)-(3) of Table 2, we define loan guarantees as the overall amount of loan guarantees listed firms provide to their related private parties, whereas in columns (4)-(6), we define them as the amount of loan guarantees to related parties net of the amount of loan guarantees the related private parties extend to the listed firms.

Consistent with our prediction, we find the estimated coefficient $\hat{\beta}$ is negative, and we can reject the null that this estimated coefficient equals 0 at standard levels of significance for both definitions of loan guarantees. This result obtains across all the specifications of equation (4) we consider, including the most restrictive specifications that absorb all time-varying shocks to loan guarantees that affect firms in the same prefecture (column (3) and column (6)). Indeed, SOEs reduce the loan guarantees they extend to their related private parties by more than non-SOEs in the same prefecture and year after the first listed firm is punished in their prefectures compared to before.

Regarding the coefficients associated with the two dummies, we do not detect any systematic pattern in the estimations. We find $\hat{\gamma}_1$ has different signs across specifications, and do not reject the null the coefficient equals zero at standard levels of significance across some of the specifications. Similarly, we fail to reject the null that $\gamma_2 = 0$ in most specifications, which suggests that after the punishment of the first prefecture peer, the loan guarantees that non-SOEs extend to their related private parties do not change significantly.

In terms of economic magnitude, the differential cut in loan guarantees to related private parties scaled by total assets of SOEs compared to non-SOEs after the peer's punishment ranges between 1.1 percentage points (column (2) of Table 2) and 2.4 percentage points in the most restrictive specifications of column (3) and column (6). This effect is economically large, because it corresponds to about a one quarter of a standard deviation in the amount of guarantees scaled by total assets (0.102), and to about 18% of a standard deviation in the amount of net guarantees scaled by total assets (0.134).

A Robustness

In Table 3, we propose a set of tests to 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.

One might be concerned the SOE status of the firms in our sample could change dramatically during the sample period, when the Chinese government proceeded to privatize several SOEs. We show 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.

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 an important potential reporting issue with our data. 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 simply not reporting them. The potential under-reporting could be an issue, especially for the punishment events that happen up to 2004, because the difference between pre-period loan guarantees and post-period loan guarantees might be mechanically higher than it would be had we observed the loan guarantees in the pre-period without error. To assess the extent of this concern, in Panel G, we exclude all firms that reported no loan guarantees up to 2004 and suddenly report a positive value in 2004. We argue 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 start to provide guarantees in 2004. Our results are qualitatively similar to the baseline results when we restrict the sample in this way.

Finally, in Panel H of Table 3, we propose a placebo test whereby we assign placebo dates of the first punishment to Chinese prefectures randomly – in the sample of prefecture$\times$years, we use a random number generator to match each prefecture to one of the first punishment-event dates observed in the data. We verify that we fail to reject the null hypothesis of no differential reactions of SOEs compared to non-SOEs in the prefecture after the placebo date of the first punishment of a local peer.

In the Online Appendix, we report the results of an additional robustness test in which we exclude all the peers of the punished firm– whether SOEs or non-SOEs – that belong to the same industry as the punished firm. The rationale for this robustness test is that the punished firm might have been a product-market competitor or a supplier/customer of peers operating in the same industry, and hence the punishment might have changed the competitive landscape or supply chain of the local industry. Note any such story should also differ systematically across SOEs and non-SOEs to explain our results. In Table A.3 of the Online Appendix, we exclude all local firms that operate in the same industry as the punished firm, and we find results consistent with our baseline analysis.

Moreover, we estimate our baseline effects separately across firms that belong to industries with high or low concentration. We compute concentration as the HHI index at the level of 4-digit SIC industries. We then split firms based on whether they in industries above the median or below the median by HHI. Table A.4 in the Online Appendix reports the results, which show that the size of the estimated coefficients of interest are quite similar across firms in high- or low-concentration industries. We fail to detect any systematic patterns based on this split.

B Collapsed Sample: Pre– and Post–Peer Punishment

Bertrand et al. (2004) show a difference-in-differences strategy suchs as the one we propose using repeated observations of the same firm over time for several periods both before and after the treatment might raise concerns about statistical inference and the identification of the local treatment effect. The repeated firm-level observations are not independent from each other, and hence standard errors are biased downwards. Our clustering of standard errors at the prefecture level reduces this concern, but we also propose the specification of 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 their prefecture. This procedure leaves us with at most two observations for each firm – one observation including the averages of all variables before the punishment and one afterwards.

Columns (1)-(2) of Table 4 report the results for estimating the collapsed specification. In column (1), we use the full sample of firms, whereas in column (2), we include only firms that do report values for guarantees before the punishment. Note that in the collapsed specifications, we have no scope to absorb year fixed effects as the dummy variable *After Punishment* absorbs the systematic differences between the pre-punishment and post-punishment observations within firms. Our results survive in this specification, and if anything, the size of the estimated coefficient of interest is larger in absolute value than the estimate in the baseline specification of Table 2.

With the collapsed specification, we can also assess the extent to which our effects build up 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, we see from columns (3)-(6) of Table 4 that the baseline effect builds up slightly over time, although we cannot reject the null hypothesis that the size of the effect is similar within firms over time.

C Second Empirical Strategy: Reaction within SOEs to the Same Events

The baseline results we have presented so far might raise the concern that systematic differences between SOEs and non-SOEs could vary around the punishment events and these changes might explain the differences in loan guarantees around the events instead of a direct reaction to peers' punishments. For instance, SOEs might have happened to have less loan guarantees outstanding at the time of punishment events relative to non-SOE peers in the same prefecture, and might have reacted less for this reason.

To tackle this concern, we move on to our second empirical strategy, which only exploits variation in the ex-ante incentives SOE managers might have to react to the same peers' punishments in the same location and at the same time. This strategy tests Hypothesis 2 of Section IV.

For this test, we exploit variation in proxies for 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, CEOs with more severe career concerns should thus be more willing to cut loan guarantees, which is a potential source of wrongdoing, after observing a peer's punishment.

Empirically, the challenge to bring this conjecture to the data is defining a proxy for career concerns in our setting. We propose two alternative proxies. The first proxy hinges on the fact that at the time of a firm's punishment CEOs at different stages of their career run peer firms of the punished one. Intuitively, the closer the CEO is to retirement age – which China mandates at 60 years of age for men and 55 years of age for women even for managerial and white-collar jobs – the less severe her career concerns and hence the less sensitive she might be to react to a peer's punishment. On the contrary, CEOs who are younger at the time of facing a peer's punishment should react more as the reputation costs of a punishment for their whole future career path would be larger. This intuition builds on Jiang, Wan, and Zhao (2015), who consider younger age as a proxy for stronger career concerns for directors in Chinese listed firms.

Panel A of Table 5 reports the results for this test. We split the sample of firms into two groups based on CEOs' age. Columns (1)-(3) focus on CEOs whose age is below 55 years, whereas columns (4)-(6) focus on CEOs whose age is 55 years or higher, and hence are close to the mandated retirement age.[19] 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 than before and compared to non-SOEs. Instead, for SOEs whose CEOs are closer to retirement age, the effect disappears. If anything, the estimate coefficients of interest are positive although they are not statistically different from 0.

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.[20] To assess this alternative explanation,

[19]The results are qualitatively similar if we use different cutoffs.
[20]We thank Stefan Zeume for suggesting this alternative explanation to our second strategy.

we compute the probability of punishment for young and old CEOs separately in our sample. The unconditional likelihood of punishment is 1.53% for young CEOs and 1.72% for old CEOs, 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 the probabilities are the same across young and old CEOs when we condition the sample within SOEs and within non-SOEs.

Our second proxy for career concerns is based on the intuition that CEOs who take part in the international managerial labor market have more outside opportunities than CEOs whose career has always been within China. The rationale for this conjecture is that CEOs with job experience outside China might more easily find alternative occupations in case they were to need to leave China as their job market is broader than other CEOs. We would thus expect CEOs with overseas job experience should react less to the peer's punishment.

Panel B of Table 5 reports the results when splitting the sample based on our alternative proxy for the severity of career concerns – whether CEOs have work experience overseas or not. Again, the results seem consistent with our conjecture – we fail to detect an economic or statistically significant effect for SOEs whose CEOs have experience overseas and hence have plausibly less severe career concerns, because they can access the international job market. Instead, SOEs whose CEOs never worked abroad drive our baseline results.

The results in Table 5 also help to assuage the concern that our proxies for the severity of career concerns might in fact proxy for the level of entrenchment of CEOs with the central and/or local governments. We might expect that the older CEOs and CEOs whose whole career was in mainland China are on average more connected to the party than other CEOs. At the same time, we see that these two groups of CEOs react quite differently to peers' punishments, which corroborates our career-concern interpretation over the entrenchment interpretation.

VII Salience of the Punishment Events

The results we have proposed so far are agnostic on whether SOEs' reaction to peers' punishments are fully consistent with Bayesian updating about the probability of rare events or if potential overreaction of SOE CEOs to salient punishments might partially explain our findings. To tackle this question, ideally we would test whether the extent of the reaction of SOEs to local peers' punishments is higher when the punishment events are more salient relative to when the punishment events are less salient after keeping constant the availability of information about peer events to SOEs.

An important feature of our setting is that information about peers' punishments is publicly available. For punishments that the CSRC decides directly, information about the involved company, the extent of punishment, and the motivation for the punishment are posted on the regulator's website and accompanied by press releases. For punishments that local regulators in Shanghai and Shenzhen decide, the regulators directly make information about the punishments publicly available. Because the set of Chinese listed firms is limited, we can assume any Chinese listed firm – and especially the local peers of punished firms – can easily access basic information about any punishments.

This feature *per se* casts doubts on the possibility that the reaction of local peers we have documented so far is fully Bayesian, because all Chinese listed firms and not only local peers are likely to know about the punishments once they are announced, and hence under Bayesian updating, all the firms, and not only local peers, should react to this information in a similar fashion.

A Press Coverage and Market Reaction

To dig deeper into the potential for non-Bayesian updating to explain part of our results, we construct two proxies for the salience of punishment events. The first proxy exploits the media coverage of peers' punishment (*media reaction*). We collect all pieces of news about each firm in our sample in national and local newspapers in the 60 days around the investigation of the punished peer from the *China Knowledge Resource Integrated Database*. For each piece of news in the database, we categorize whether it discusses the punishment or not by checking whether the title or the body of the news includes either the name of the punished firm or the punishment event. For each period around a punishment event, we then construct the ratio of the number of news items that cover the punishment over the total number of news items published over the period. We define as salient an event whose news coverage ratio was in the top 10% of the distribution of all punishments' news coverage ratios.

The rationale for this measure is that the higher the media coverage of a punishment event, the more likely the punishment's salience to local peers. Note that under this measure, the most salient events are not necessarily related to the largest punishments, or to the most abusive practices perpetrated by a punished firm. The ratio of total news covering the punishment event might be low when other important events are covered by the media and distract peers, or when the central government restricts access to information regarding the punished firms. In all these cases, the punishment event should be less salient to peers than when the share of news covering the punishment event is high. Moreover, because public information about any punishment is readily available throughout China, especially to the management of listed firms, a fully Bayesian interpretation for the reaction of local peers but not other firms based on the extent of media coverage is not obvious.

The second proxy for the salience of punishment events relies on the returns of punished firms around the announcement (*market reaction*). Intuitively, events that cause a larger drop in the stock price of the punished firm should be more salient than events that cause a smaller reaction, because the extent of fraud for which the firm is punished is large.

We define as salient a punishment event that causes negative cumulative abnormal returns in the worse 10% of the sample distribution (more than 37.7%) of the cumulative abnormal returns of punished firms in the 30-day window around the announcement. This proxy is potentially correlated with the media-coverage proxy, but does not necessarily capture the same variation. We find that the pairwise correlation of the two dummy variables in the sample of event is 63%

In the left panel of Table 6, we consider the first proxy for salience, whereas in the right panel we consider the second proxy.

For both proxies, we find that the effect of peers' punishment on SOEs is 2 to 3 times larger when the punishment event is more salient than when the punishment event is less salient. Note that non-SOEs do not react to any of the types of events, which is consistent with our interpretation of the baseline results—non-SOEs might not react, because other governance mechanisms are in place and discipline their behavior even absent the salience of peers' punishment.

Results (untabulated) are similar if we modify the rules and thresholds to compute our salience proxies. For instance, we find similar results if we consider the number of downloads of punishment news events over the total number of news downloads from the *China Knowledge Resource Integrated Database* to construct our measure of media coverage of the punishment event. This alternative proxy addresses the concern that news covering the punishment might not be read. Moreover, we find similar results if we change the threshold for the negative CARs of the punished firms or if we increase the event window when constructing our second measure of salience.

One concern with these results is that the larger is the punishment size, the higher is the media coverage and market reaction. Although this intuition is plausible in general, one of the main benefits of considering media coverage is that media coverage is likely to differ not only based on the size of the punishment, but also based on whether other events unrelated to the punishment crowd out the space that would have otherwise been devoted to the punished firm. Indeed, media coverage has a quasi-random component driven by other unrelated events, which neither the punished firms, not the peers or the punishing regulator can control. To tackle this concern more directly, we have also repeated our results by excluding all the punishment evens for which a direct fine measured in RMB is reported in the punishment announcement. Events that include a direct fine are the once regulators deemed as the worst fraud cases. In untabulated results, we replicate the coefficients of Table 6 closely in terms of both magnitude and statistical significance if we exclude these large punishment events from the sample.

B Product-Market Dynamics? Industry Peers

The results so far do not rule out directly that the effects we document do not derive from product-market dynamics after the punishment of peers – which might be competitors or suppliers/customers of other listed firms.

First, the punishment of a peer might have direct effects on the competitive pressure in the local product markets. For instance, the competing non-punished firms might gain market shares and potentially increase their markups in an oligopoly or monopolistic-competition setting. This channel might be consistent with the results we discuss below that peer firms enjoy positive CARs after the punishment and that they cut investment given lower competition as long as these competitive effects only affect SOEs. At the same time, this interpretation seems less able to explain why peer firms' TFP increases, why their governance becomes more transparent, as well as the baseline outcome we consider – why peer firms cut their loan guarantees, which also has real effects on related parties. Moreover, the competition-based interpretation cannot explain the heterogeneous reactions we document within SOEs based on CEOs' career concerns, unless the competitive effects of peers' punishments are stronger for SOEs whose CEOs have higher career concerns, which seems implausible.

To provide a direct assessment of the relevance of this competition channel, we propose a specification in which peers are not defined based on their geographic location, but on their industry. Under this definition, all the direct competitors of the punished firm are peers, irrespective of their location. Under the competition story, peers that operate in the same industry as the punished firm, and hence face a positive competitive shock, should drive any effect.

Table 7 reports the results for estimating equation (4) when using the alternative industry-based definition of peer firms. We define industries based on the CSRC 2001 classification. Across specifications, the size of the estimated interaction between the post-punishment period and the SOE status of listed firms is small. We do fail to reject the null hypotheses that this interaction coefficient equals 0 statistically at any plausible level of significance. We interpret these results as direct evidence that competition-based explanations, which would be especially relevant when peer firms are defined as members of the same industry, are unlikely explanations for our results.

The second industrial-organization interpretation relates to the transmission of negative shocks along the supply chain. As we discuss in Section II, SOEs tend to be proportionally more common in upstream industries than in downstream industries. Because peers' punishments include both SOEs and non-SOEs, the punishment of non-SOEs might represent a negative shock to a relevant downstream customer of SOEs. Such a negative shock might propagate upwards to SOEs. For instance, the punished customer might stop paying for the goods it purchased or might cancel existing orders. But this shock would have a *negative* effect on the local upstream SOE peers, which should result in negative CARs for SOE peers around the events (Cohen and Frazzini (2008); Ozdagli and Weber (2017); Pasten et al. (2017); Pasten et al. (2018)). Instead, we document positive CARs below for SOE peers around the punishment events.

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 their companies do not engage in wrongdoing, and change other corporate policies as we discuss in Hypotheses 4-6 of Section IV.

A Governance: CEO Duality

Table 8 reports the results for estimating equation (4) using the CEO-duality dummy as an outcome. As predicted in Hypothesis 4, we reject the null that the coefficient $\beta = 0$ at conventional levels of statistical significance within firm. We find SOE firms are about 5.6 percentage points less likely to display CEO duality after the first firm is punished in their prefecture than before and compared to non-SOE firms. This effect is economically large, because it represents about 43% of the average share of firms with dual roles for CEOs throughout the sample.

B Investment

Columns (1)-(3) of Table 9 report the results for estimating equation (4) with investment as an outcome. We find SOEs decrease investment after the first peer is punished in their location relative to before and to non-SOEs. Moreover, non-SOEs appear not to change their investment on average, as captured by the small size of the estimated coefficient $\hat{\gamma_2}$. In terms of the magnitude of the effect, the differential drop in investment for SOEs after the peer punishment is about 1 percentage point, which corresponds to 17% of a standard deviation of investment in the running sample.

C 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 had invested 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 alternative outcome when estimating equation (4).

We run this analysis in columns (4)-(6) of Table 9. SOEs' TFP increases after the first peer punishment, relative to before and to non-SOE firms in the same location. In the specification that includes firm and prefecture$\times$year fixed effects, we fail to reject the null hypothesis that the interaction coefficient $\hat{\beta} = 0$, because the p-value for the two-sided t-tests of the null hypothesis is about 12%. This non-result is not surprising given the high persistence of TFP within firm over time. In terms of economic magnitude, the size of the estimated effects range from 0.12 to 0.21, which is between 2% and 4% of the average TFP in the sample.

D Market Value of Firms after Peers' Punishment

Our evidence so far is not enough to conclude shareholder value increases in SOEs after a peer's punishment. If changing governance outcomes have no material effect on shareholder value and/or if the gains from increased efficiency via higher TFP are not distributed to shareholders but to other stakeholders of the firm, minority shareholders of SOEs would not be better off after a peer's punishment.

To assess directly whether shareholder value increases in listed SOEs after a peer's punishment, we run event studies around the punishment of peers and compare the CARs of peer SOEs and peer non-SOEs around the punishment dates. Figure 3 plots the average market-cap-weighted adjusted CARs for the listed SOEs and listed non-SOEs around punishments.[21]

Two patterns are worth noticing. First, the CARs of SOEs and non-SOEs follow trends that appear parallel at least up to the five days before the punishment. These parallel trends in CARs resemble the parallel trends of the outcome variables we consider in the regression analysis for the years before peers' punishments (see Section V). We detect only slightly diverging trends in the five days before the announcement. These pre-announcement diverging trends in the very few days before the event date might reflect information leakage about the upcoming punishment announcements.

Second, after the punishment, we observe an evident divergence of the trends in CARs for SOEs and non-SOEs. The average CARs of non-SOEs stay insignificantly negative throughout the sample period. Instead, the average CARs for SOEs increase significantly after the peer's punishment and keep increasing, staying statistically different from 0 throughout the 15 days after the peer's punishment.

Overall, the event-study results suggest the change in the outcomes of SOE firms we discussed above are paralleled by a positive and significant market reaction.

IX Effects of Peers' Reaction on Related Parties

Our results so far do not rule out that SOE managers engage in substitution across wrongdoing activities, as we discuss in Hypothesis 7 of Section IV. For instance, SOEs managers 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 tunneling at the expense of minority shareholders.

Note that in the previous section, we documented that after the punishment, SOE managers change a set of firm policies, and the shareholder value of these companies – as measured by CARs –increases, which suggests that even if managers engaged in alternative forms of tunneling resources to related parties, on average, the effect of peers' punishment is positive for minority shareholders.

If SOE managers substituted loan guarantees with other forms of tunneling, we would expect that related-party outcomes do not change systematically after the peers' punishment compared to before for SOE-related parties. Instead, detecting a systematic change in SOE-related parties' outcomes would suggest related parties did suffer a cut in available resources after the punishment.

Table 10 reports the results for estimating equation (4) using the outcomes of related parties as the dependent variable. In columns (1)-(3), the outcome variable is the amount of credit related parties of SOE and non-SOE firms obtain each year through bank loans guaranteed by a listed related party, scaled by assets. We find 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

[21] We market-adjust returns.

is about one-half of the average total amount of guaranteed credit over total assets.

The decrease in SOEs' related parties borrowing after the punishment of peers of the related listed firms corroborates the idea that SOEs do not substitute loan guarantees with more opaque forms of guarantees to allow related parties to borrow.

In columns (4)-(6) of Table 10, we consider the related parties' investment as the outcome variable, and we find that after the peers' punishment, related parties of SOEs decrease investment substantially. This result suggests the drop in borrowing through guaranteed loans has real implications for related parties, and corroborates the idea that listed SOEs do not engage in more opaque methods to tunnel resources to related parties for the purpose of related parties' investment.

X Conclusions

We propose an empirical laboratory to test whether direct experience of a peer's punishment might have a sobering effect on the wrongdoing perpetrated by non-punished peers.

In a first design, we compare the reactions of local Chinese SOEs and non-SOEs to the punishment of the same peer firm. We argue SOEs should be more prone to react, because they are less constrained by traditional governance mechanisms that instead restrict the behavior of listed non-SOEs. We find that after a local peer headquartered in the same prefecture is punished for wrongdoing in loan guarantees to related private parties, non-punished Chinese SOEs reduce the amount of loan guarantees they extend to related private parties, cut inefficient investment, and improve their governance by moving to non-dual boards.

To alleviate the concern that SOEs and non-SOEs differ along many dimensions, including the potential exposure to regulatory punishments, and these differences drive our findings, we propose a second empirical design that only exploits the reaction of SOEs with different incentives to the punishment of the same local peers. This design confirms our baseline findings.

Our results open a set of questions that beget further investigation. Is the sobering effect of peers' punishment a permanent change in agents' behavior, or does this effect revert over time? If the effect is permanent, to what extent could the salience-of-punishment mechanism – which is cost effective because it does not require universal monitoring or oversight on the part of the regulator – substitute more expensive mechanisms that aim to guarantee a level playing field in markets?

Moreover, we provide suggestive evidence that a Bayesian interpretation can barely explain all the facts we document. If non-Bayesian updating is involved, what are the psychological mechanisms through which the reaction to peers' punishments operates? For example, is the salience of the probability of punishment, the salience of the non-pecuniary costs of punishment, or both dimensions important to determine the reaction of non-punished peers? Further research using field data and experimental research designs might provide insights into these questions.

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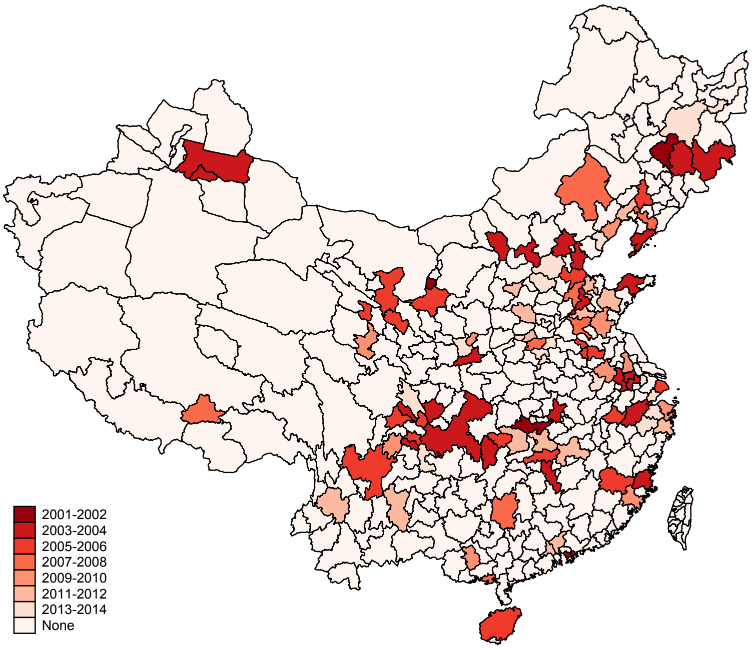
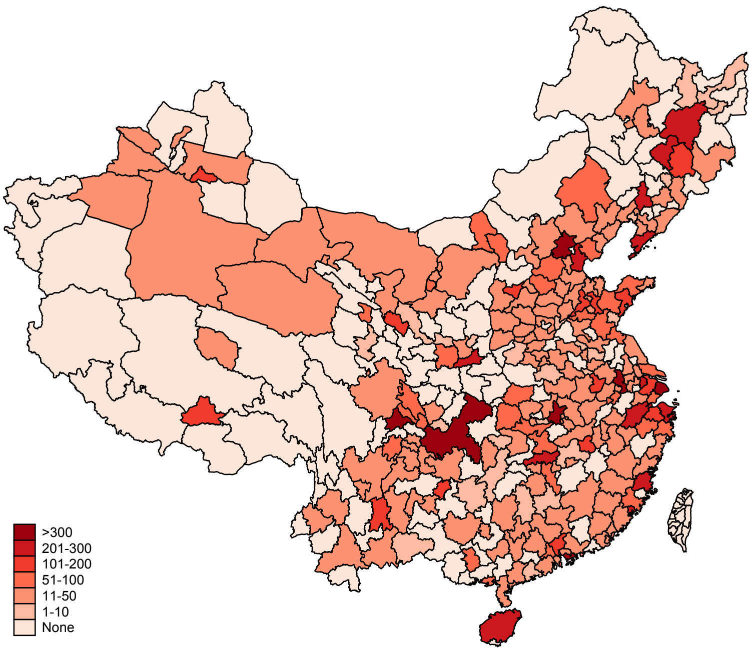
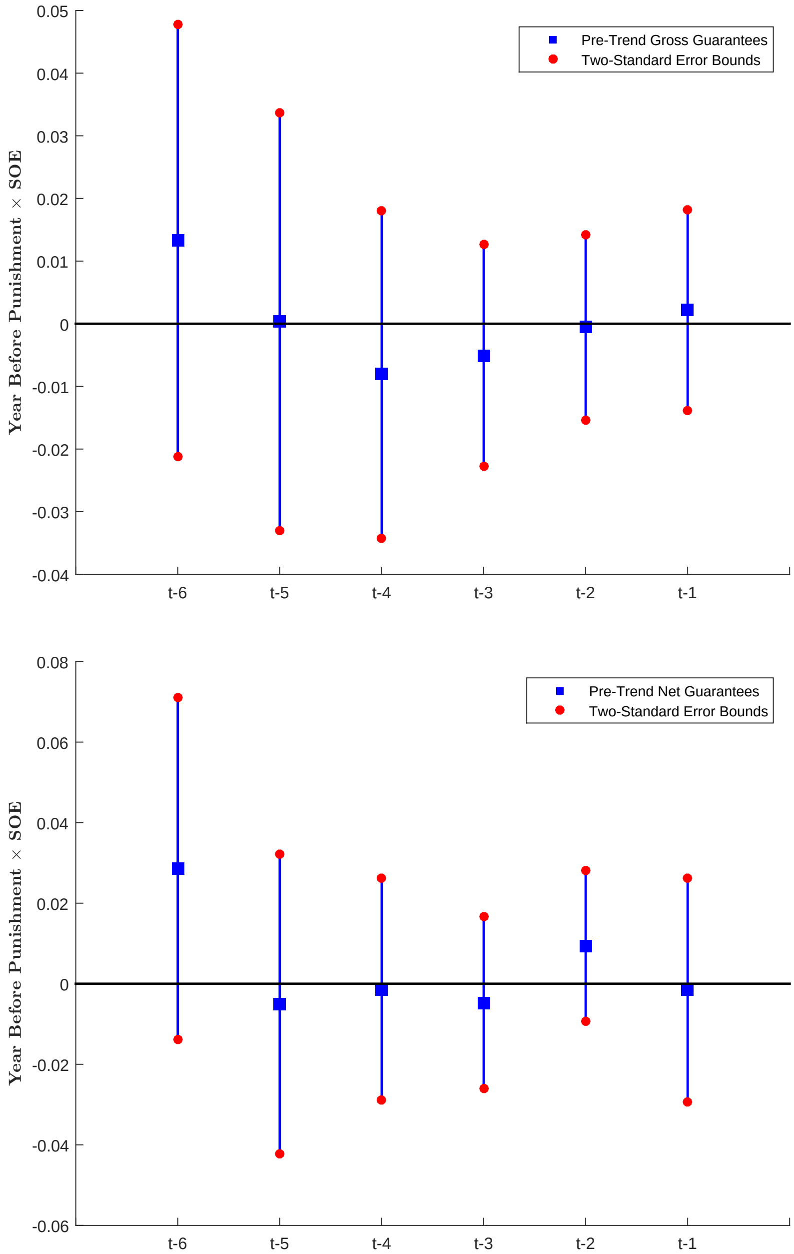
Figure 1: **Time of First Punishment and Number of Firms at the Prefecture Level**





This figure plots the time of the 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 2014.

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

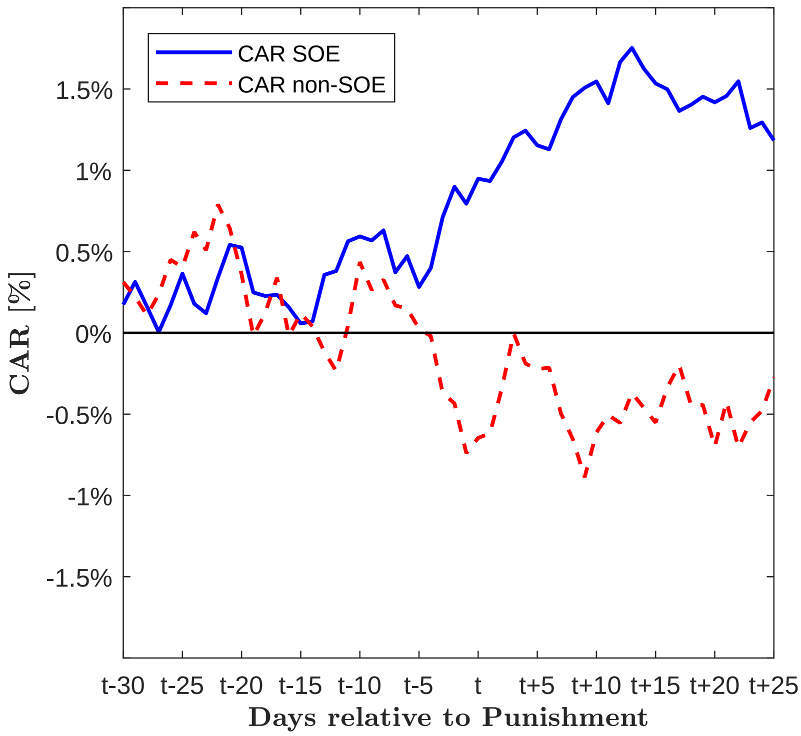


This figure plots the estimates of β_t from the following linear equation

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

where $\sum_t \beta_t SOE_{i,p} \times Year_t$ is a set of interactions of a dummy variable for whether firm i is an SOE and year dummies for all the t years before the first punishment of a listed firm in prefecture p, after partialling out firm characteristics (X) as well as firm fixed effects (η_i) for the total amount of loan guarantees scaled by assets in the top panel and the net amount of guarantees scaled by assets in the bottom panel. The sample period is 1997 to 2014.

Figure 3: **Cumulative Abnormal Returns (CAR) Around Peer's Punishment**



This figure plots the average CARs around punishments separately for SOEs (blue solid line) and non-SOEs (red dashed line). We estimate market-adjusted returns separately for SOEs and non-SOEs and winsorize returns at the 5% and 95% levels. The sample period is 1997 to 2014.

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 in the analysis, 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 that are headquartered in a prefecture in which at least one listed company was punished by regulators because of irregular loan guarantees to related parties. Panel B refers to the individual bank loans we observe for firms in our main sample. Panel C refers to the firm×year sample of all private related parties linked to a listed firm in our main sample. We winsorize financial variables at the 1% and 99% levels. The sample period is 1997 to 2014.

Variable	N	Mean	Std	p10	p50	p90
Panel A. Main Sample						
After Punishment	14,244	0.42	0.49	0.00	0.00	1.00
SOE	13,323	0.50	0.50	0.00	0.00	1.00
Total Guarantees / Assets	14,068	0.04	0.10	0.00	0.00	0.13
Net Guarantees / Assets	14,068	-0.01	0.13	-0.16	0.00	0.08
TFP	12,606	5.26	2.27	2.68	5.23	8.06
CEO Duality	13,862	0.13	0.34	0.00	0.00	1.00
Analyst Coverage (dummy)	14,244	0.28	0.45	0.00	0.00	1.00
Capital Investment / Assets	9,906	0.01	0.06	-0.03	0.00	0.06
Total Assets	14,068	21.57	1.21	20.23	21.43	23.09
Long-term Leverage	13,933	0.06	0.09	0.00	0.02	0.18
Total Leverage	13,933	0.48	0.22	0.23	0.49	0.72
Cash / Assets	14,041	0.16	0.12	0.04	0.13	0.31
Tobin's Q	14,113	1.77	1.44	0.48	1.35	3.54
Panel B. Bank Loan Sample						
After Punishment	2,899	0.55	0.50	0.00	1.00	1.00
SOE	2,674	0.31	0.46	0.00	0.00	1.00
Guaranteed Borrowings / Assets	2,741	0.08	0.23	0.00	0.04	0.19
Total Assets	2,872	22.08	1.08	20.74	22.02	23.48
Leverage	2,868	0.08	0.10	0.00	0.04	0.22
Cash / Assets	2,872	0.15	0.09	0.05	0.13	0.26
Tobin's Q	2,849	1.49	1.23	0.42	1.12	3.00
Panel C. Related Party Sample						
After Punishment	12,168	0.61	0.49	0.00	1.00	1.00
SOE	11,077	0.34	0.47	0.00	0.00	1.00
Capital Investment (All Parties) / Assets	12,168	0.06	0.27	-0.08	0.00	0.23
Total Assets	11,872	22.51	1.24	21.09	22.34	24.19
Leverage	11,847	0.07	0.10	0.00	0.03	0.21
Cash / Assets	11,872	0.15	0.10	0.05	0.13	0.28
Tobin's Q	12,016	1.37	1.17	0.38	1.00	2.71

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

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

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

where Loan $Guarantees_{i,p,t}$ is the amount of loan guarantees extended by firm i in prefecture p in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,p,t}$ is a dummy variable that equals 1 if listed company i in prefecture p was an SOE in year t, and 0 otherwise; 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 0 otherwise; X 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; η_i, η_p, and η_t represent full sets of firm, prefecture, and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). Columns (1)-(3) report results for the overall amount of loan guarantees, whereas columns (4)-(6) report results for net guarantees. The sample period is 1997 to 2014.

	Total Provided Guarantees			Net Provided Guarantees		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0158***	0.0018		0.0214***	0.0087	
	(2.96)	(0.30)		(2.71)	(0.92)	
After Punishment × SOE	−0.0121**	−0.0110**	−0.0241***	−0.0193***	−0.0135*	−0.0235**
	(−2.23)	(−2.15)	(−2.83)	(−2.64)	(−1.83)	(−2.03)
SOE	−0.0256***	−0.0055	0.0172**	−0.0126**	−0.0001	0.0157**
	(−7.06)	(−1.61)	(2.14)	(−2.17)	(−0.01)	(2.03)
Total Assets	0.0128***	0.0034	0.0122**	0.0102***	0.0043	0.0166***
	(5.20)	(1.28)	(2.17)	(4.61)	(1.56)	(2.71)
Leverage	0.0363**	0.0375**	0.0265	−0.0428	−0.0311	−0.0401
	(2.15)	(2.32)	(1.17)	(−1.31)	(−0.92)	(−1.17)
Cash	−0.0263***	−0.0319***	−0.0106	0.0069	0.0162	0.0233
	(−2.81)	(−3.20)	(−0.77)	(0.62)	(1.35)	(1.25)
Tobin's Q	−0.0016*	−0.0033***	0.0005	0.0053***	0.0014	0.0008
	(−1.81)	(−2.90)	(0.33)	(4.31)	(0.89)	(0.44)
Constant	−0.2229***	0.0296	−0.2286*	−0.2396***	−0.0680	−0.3611***
	(−4.36)	(0.49)	(−1.90)	(−5.17)	(−1.08)	(−2.78)
Firm Fixed Effect			X			X
Year Fixed Effect		X			X	
Prefecture Fixed Effect		X			X	
Prefecture-Year Fixed Effect			X			X
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.063	0.140	0.323	0.018	0.103	0.324

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 β from the following linear equation:

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

where all variables are defined as in Table 2. We cluster standard errors at the level of the prefecture (p). Columns (1)-(3) report results for the overall amount of loan guarantees, whereas columns (4)-(6) report results for net guarantees. The sample period is 1997 to 2014.

	Total Provided Guarantees			Net Provided Guarantees		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Only if at least one Punishment			
After Punishment × SOE	−0.0199∗∗∗	−0.0149∗∗∗	−0.0213 ∗ ∗	−0.0227 ∗ ∗	−0.0155∗	−0.0224∗
	(−3.73)	(−2.65)	(−2.56)	(−2.35)	(−1.73)	(−1.84)
Observations	9,418	9,418	9,418	9,418	9,418	9,418
Adjusted R^2	0.069	0.120	0.320	0.021	0.075	0.305
			Panel B. Excluding Beijing, Shanghai, Shenzhen			
After Punishment × SOE	−0.0122∗	−0.0131 ∗ ∗	−0.0290∗∗∗	−0.0198 ∗ ∗	−0.0179 ∗ ∗	−0.0270 ∗ ∗
	(−1.78)	(−2.24)	(−2.69)	(−2.23)	(−2.13)	(−1.99)
Observations	10,375	10,375	10,375	10,375	10,375	10,375
Adjusted R^2	0.070	0.153	0.314	0.018	0.116	0.324
			Panel C. Fixing Initial SOE Status			
After Punishment × SOE	−0.0090	−0.0144 ∗ ∗	−0.0159∗	−0.0111	−0.0062	−0.0192∗
	(−1.23)	(−2.02)	(−1.70)	(−1.26)	(−0.65)	(−1.78)
Observations	13,133	13,133	13,133	13,133	13,133	13,133
Adjusted R^2	0.047	0.140	0.297	0.010	0.098	0.304
			Panel D. Weighted Least Squares (w=N. local firms)			
After Punishment × SOE	−0.0126∗∗∗	−0.0118∗∗∗	−0.0251∗∗∗	−0.0191∗∗∗	−0.0131∗∗∗	−0.0236∗∗∗
	(−3.48)	(−3.19)	(−4.93)	(−3.95)	(−2.68)	(−3.54)
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.064	0.149	0.347	0.018	0.101	0.341
			Panel E. Weighted Least Squares (w=Total Assets)			
After Punishment × SOE	−0.0121∗∗∗	−0.0099∗∗∗	−0.0236∗∗∗	−0.0193∗∗∗	−0.0129∗∗∗	−0.0230∗∗∗
	(−3.40)	(−2.84)	(−4.74)	(−3.99)	(−2.65)	(−3.47)
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.063	0.141	0.317	0.018	0.102	0.323
			Panel F. Full Set Interactions Controls			
After Punishment × SOE	−0.0128 ∗ ∗	−0.0120 ∗ ∗	−0.0235 ∗ ∗	−0.0189∗∗∗	−0.0130∗	−0.0242 ∗ ∗
	(−2.35)	(−2.32)	(−2.60)	(−2.60)	(−1.80)	(−2.06)
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.064	0.141	0.323	0.018	0.103	0.324
			Panel G. Drop Firms with missing Guarantees pre–2004			
After Punishment × SOE	−0.0174∗∗∗	−0.0175∗∗∗	−0.0240∗∗∗	−0.0180 ∗ ∗	−0.0141∗	−0.0145
	(−2.67)	(−2.74)	(−2.77)	(−2.25)	(−1.76)	(−1.63)
Observations	7,511	7,511	7,511	7,511	7,511	7,511
Adjusted R-squared	0.059	0.138	0.337	0.014	0.099	0.325
			Panel H. Placebo Test: Random Punishment Date			
After Punishment × SOE	−0.0103∗	−0.0079	−0.0089	−0.0038	−0.0020	−0.0052
	(−1.74)	(−1.28)	(−0.85)	(−0.44)	(−0.24)	(−0.47)
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.062	0.140	0.321	0.015	0.102	0.323
Controls Table 2	X	X	X	X	X	X
Firm Fixed Effects			X			X
Year Fixed Effects		X			X	
Prefecture Fixed Effects		X			X	
Prefecture-Year Fixed Effects			X			X

Table 4: **Loan Guarantees to Related Parties after Peer's Punishment: Collapsed Specifications**

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

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

where $Loan\ Guarantees_{i,p,t}$ *is the average overall amount of loan guarantees extended by firm i in prefecture p to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets in the years before the first punishment event (t=pre) or in the years after the first punishment event (t=post);* $SOE_{i,p,t}$ *is a dummy variable that equals 1 if listed company i was an SOE every year in period t, and 0 otherwise;* $After\ Peer\ Punishment_{p,t}$ *is a dummy variable that equals 1 in the period in which prefecture p has faced at least one punishment of a locally headquartered firm, and 0 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, financial leverage, total amount of cash, and Tobin's Q as a proxy for firms' investment opportunities;* η_p *are a full set of fixed effects at the prefecture level. Across columns, we vary the years we use to average the observations at the firm level before and after the first punishment punishment event in the prefecture. Column (1) averages across the full pre and post sample, column (2) replicates column (1) but requires firms to exist pre-punishment, and columns (3) to (6) study samples with varying windows over which we average observations at the firm level. The overall sample period is from 1997 to 2014.*

	All Firms	Firms Exist Pre-punishment	t-2–t+1	t-2–t+4	t-2–t+7	t-2–t+10
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0441***	0.0452***	0.0099	0.0158**	0.0221***	0.0257***
	(6.80)	(6.94)	(1.48)	(2.24)	(3.01)	(3.55)
After Punishment × SOE	−0.0371***	−0.0358***	−0.0162*	−0.0186*	−0.0192*	−0.0221*
	(−3.57)	(−3.70)	(−1.80)	(−1.95)	(−1.80)	(−1.91)
SOE	−0.0049	−0.0023	−0.0064	−0.0090	−0.0121	−0.0138
	(−1.02)	(−0.51)	(−0.70)	(−1.00)	(−1.33)	(−1.51)
Total Assets	0.0073	0.0056	0.0099*	0.0093	0.0078	0.0070
	(1.56)	(1.27)	(1.80)	(1.65)	(1.31)	(1.09)
Leverage	−0.0186	0.0099	−0.0203	−0.0169	−0.0121	−0.0054
	(−0.57)	(0.35)	(−0.68)	(−0.64)	(−0.45)	(−0.19)
Cash	−0.0485***	−0.0303*	−0.0341	−0.0287	−0.0305	−0.0395*
	(−2.90)	(−1.82)	(−1.52)	(−1.23)	(−1.28)	(−1.85)
Tobin's Q	−0.0058***	−0.0059***	−0.0006	−0.0018	−0.0034	−0.0037
	(−3.07)	(−3.18)	(−0.20)	(−0.57)	(−0.94)	(−1.06)
Constant	−0.1118	−0.0829	−0.1709	−0.1549	−0.1180	−0.0983
	(−1.13)	(−0.88)	(−1.44)	(−1.28)	(−0.92)	(−0.71)
Observations	1,460	1,347	1,084	1,113	1,117	1,122
Adjusted R^2	0.201	0.213	0.226	0.212	0.186	0.189
Prefecture Fixed Effects	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 – The Role of Career Concerns**

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

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

where $Loan\ Guarantees_{i,p,t}$ is the overall 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 and all variables are defined as in Table 2. We cluster standard errors at the level of the prefecture (p). Panel A reports results by age of the CEO, and Panel B reports results by oversea work experience of the CEO. The sample period is 1997 to 2014.

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Age of the CEO					
	Age <= 55			Age > 55		
After Punishment × SOE	−0.0158***	−0.0137***	−0.0213 * *	0.0193	0.0203	0.0350
	(−2.82)	(−2.63)	(−2.47)	(1.31)	(1.15)	(0.39)
Observations	11,176	11,176	11,176	1,413	1,413	1,413
Adjusted R^2	0.065	0.147	0.326	0.050	0.136	0.351
	Panel B. Work Experience Overseas					
	Work Experience Overseas			No Work Experience Overseas		
After Punishment × SOE	−0.0139	0.0091	0.0084	−0.0127 * *	−0.0110 * *	−0.0232 * *
	(−0.65)	(0.35)	(0.12)	(−2.26)	(−2.03)	(−2.48)
Observations	847	847	847	12,122	12,122	12,122
Adjusted R^2	0.034	0.175	0.560	0.062	0.143	0.319
Controls Table 2	X	X	X	X	X	X
Firm Fixed Effects			X			X
Year Fixed Effects		X			X	
Prefecture Fixed Effects		X			X	
Prefecture-Year Fixed Effects			X			X

Table 6: **Loan Guarantees to Related Parties after Peer's Punishment: The Role of Salience**

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

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

where $Loan\ Guarantees_{i,p,t}$ is the amount of gross 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; $SOE_{i,p,t}$ is a dummy variable that equals 1 if listed company i was an SOE in year t, and 0 otherwise; in the left panel, $After\ Peer\ Punishment - Salient_{p,t}$ 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 the ratio between the number of news reports that discuss the punishment over the total number of news reports in the two months before and after the investigation window is in the top 10% of the distribution; in the right panel, $After\ Peer\ Punishment - Salient_{p,t}$ 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 the CARs of the punished firm were lower than the bottom 10% in the 30 days around the punishment announcement; $Salient_p$ is a dummy that equals 1 if prefecture p has faced a salient punishment of a locally headquartered firm; X 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; η_i, η_p, and η_t represent full sets of firm, prefecture, and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2014.

	Top 10% – Punishment News/Total News			Bottom 10% – CARs punished		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment Salient	0.0353**	0.0208		0.0265***	0.0044	
	(2.57)	(1.43)		(3.15)	(0.45)	
After Punishment Non-Salient	0.0126**	−0.0020		0.0158***	0.0017	
	(2.47)	(−0.35)		(2.78)	(0.27)	
After Punishment Salient × SOE	−0.0311**	−0.0236**	−0.0424***	−0.0186**	−0.0151*	−0.0483***
	(−2.59)	(−2.28)	(−4.03)	(−2.31)	(−1.86)	(−3.23)
After Punishment Non-Salient × SOE	−0.0075	−0.0083	−0.0199**	−0.0110*	−0.0105*	−0.0196**
	(−1.39)	(−1.60)	(−2.44)	(−1.88)	(−1.90)	(−2.37)
SOE	−0.0255***	−0.0055	0.0169**	−0.0253***	−0.0053	0.0174**
	(−7.11)	(−1.65)	(2.14)	(−6.98)	(−1.56)	(2.17)
Salient	−0.0036			−0.0111***		
	(−0.98)			(−3.26)		
Total Assets	0.0127***	0.0034	0.0123**	0.0125***	0.0034	0.0125**
	(5.15)	(1.28)	(2.20)	(5.10)	(1.26)	(2.21)
Leverage	0.0352**	0.0366**	0.0258	0.0373**	0.0374**	0.0268
	(2.10)	(2.27)	(1.14)	(2.20)	(2.30)	(1.17)
Cash	−0.0264***	−0.0322***	−0.0103	−0.0269***	−0.0324***	−0.0104
	(−2.84)	(−3.25)	(−0.75)	(−2.85)	(−3.26)	(−0.76)
Tobin's Q	−0.0015*	−0.0033***	0.0005	−0.0016*	−0.0033***	0.0005
	(−1.75)	(−2.92)	(0.34)	(−1.76)	(−2.88)	(0.34)
Constant	−0.2221***	0.0289	−0.2324*	−0.2170***	0.0307	−0.1964
	(−4.33)	(0.48)	(−1.93)	(−4.25)	(0.51)	(−1.57)
Firm Fixed Effect			X			X
Year Fixed Effect		X			X	
Prefecture Fixed Effect		X			X	
Prefecture-Year Fixed Effect			X			X
Observations	12,969	12,969	12,969	12,959	12,959	12,959
Adjusted R^2	0.065	0.141	0.323	0.063	0.140	0.324

t-statistics in parentheses

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

Table 7: **Product-Market Dynamics: Industry Peers**

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

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

where $Loan\ Guarantees_{i,k,t}$ *is the amount of loan guarantees extended by firm i in industry k in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets;* $SOE_{i,k,t}$ *is a dummy variable that equals 1 if listed company i in industry k was an SOE in year t, and 0 otherwise;* $After\ Peer\ Punishment_{k,t}$ *is a dummy variable that equals 1 if industry k has faced at least one punishment as of year t, and 0 otherwise; X 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;* η_i*,* η_k*, and* η_t *represent full sets of firm, industry, and year fixed effects, respectively. We cluster standard errors at the level of the industry (k). We define industries based on the CSRC 2001 classification. Columns (1)-(3) report results for the overall amount of loan guarantees, whereas columns (4)-(6) report results for net guarantees. The sample period is 1997 to 2014.*

	Industry Peer Definition					
	Total Provided Guarantees			Net Provided Guarantees		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0011			0.010		
	(0.22)			(1.63)		
After Punishment × SOE	−0.0086	−0.0030	−0.0056	−0.0133	−0.0009	−0.0026
	(−1.49)	(−0.31)	(−0.64)	(−1.60)	(−0.08)	(−0.24)
SOE	−0.0189***	−0.0055	0.0172**	−0.0126**	−0.0001	0.0157**
	(−4.55)	(−1.61)	(2.14)	(−2.17)	(−0.01)	(2.03)
Controls from Table 2	X	X	X	X	X	X
Firm Fixed Effect		X	X		X	X
Year Fixed Effect	X			X		
Industry Fixed Effect	X			X		
Industry-Year Fixed Effect		X	X		X	X
Prefecture-Year Fixed Effect			X			X
Observations	12,967	12,967	12,967	12,967	12,967	12,967
Adjusted R^2	0.155	0.328	0.323	0.098	0.298	0.298

t-statistics in parentheses

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

Table 8: **Traditional Governance after Peer Punishment**

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

$$CEODuality_{i,p,t} = \alpha + \beta SOE_{i,p,t} \times After\ Peer\ Punishment_{p,t} + \gamma_1 SOE_{i,p} + \gamma_2 After\ Peer\ Punishment_{p,t} + X'\delta + \eta_i + \eta_p + \eta_t + \epsilon_{i,p,t},$$

where $CEODuality_{i,p,t}$ is a dummy variable that equals 1 if firm i in prefecture p in year t displays CEO duality; $SOE_{i,p,t}$ is a dummy variable that equals 1 if listed company i in prefecture p was an SOE in year t, and 0 otherwise; $After\ Peer\ Punishment_{p,t}$ 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 0 otherwise; X 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; η_i, η_p, and η_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2014.

	(1)	(2)	(3)
After Punishment	0.0087	0.0257	
	(0.48)	(1.48)	
After Punishment × SOE	−0.0165	−0.0180	−0.0564 ∗∗
	(−0.76)	(−0.90)	(−2.02)
SOE	−0.0417∗∗∗	−0.0227∗	0.0508∗
	(−2.92)	(−1.84)	(1.96)
Total Assets	−0.0142∗∗∗	−0.0146∗∗∗	−0.0364 ∗∗
	(−3.02)	(−2.68)	(−2.05)
Leverage	−0.1042∗	−0.1231 ∗∗	0.0105
	(−1.92)	(−2.49)	(0.13)
Cash	−0.0059	0.0177	0.0150
	(−0.13)	(0.38)	(0.21)
Tobin's Q	0.0024	0.0003	−0.0016
	(0.64)	(0.07)	(−0.31)
Constant	0.4548∗∗∗	0.5054∗∗∗	0.9723 ∗∗
	(4.53)	(3.94)	(2.46)
Firm Fixed Effect			X
Year Fixed Effect		X	
Prefecture Fixed Effect		X	
Prefecture-Year Fixed Effect			X
Observations	12,622	12,622	12,622
Adjusted R^2	0.009	0.084	0.384

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 β from the following linear equation:

$$Real\ Outcome_{i,p,t} = \alpha + \beta SOE_{i,p,t} \times After\ Peer\ Punishment_{p,t} + \gamma_1 SOE_{i,p,t}$$
$$+ \gamma_2 After\ Peer\ Punishment_{p,t} + X'\delta + \eta_i + \eta_p + \eta_t + \epsilon_{i,p,t},$$

where Real Outcome$_{i,p,t}$ is either the investment scaled by total assets or the total factor productivity(TFP) of firm i in prefecture p in year t; SOE$_{i,p,t}$ is a dummy variable that equals 1 if listed company i in prefecture p was an SOE in year t, and 0 otherwise; After Peer Punishment$_{p,t}$ is a dummy variable that equals 1 if prefecture p has faced at least one punishment of a locally headquartered firm as of year t, and 0 otherwise; X 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; η_i, η_p, and η_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). Columns (1)-(3) report results for investment scaled by total assets, whereas columns (4)-(6) report results for TFP. The sample period is 1997 to 2014.

	Δ Fixed Assets / Total Assets			TFP		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	−0.0051***	0.0045		0.0689	−0.0433	
	(−3.15)	(1.20)		(0.58)	(−0.48)	
After Punishment × SOE	−0.0060**	−0.0054*	−0.0106*	0.3614***	0.2782**	0.0747
	(−1.98)	(−1.76)	(−1.84)	(2.97)	(2.51)	(0.90)
SOE	0.0125***	0.0098***	0.0096*	−0.3207***	−0.2123**	−0.0514
	(4.81)	(3.53)	(1.79)	(−3.05)	(−1.99)	(−0.75)
Total Assets	0.0027***	0.0041***	−0.0029	0.2772***	0.2394***	0.1587***
	(4.18)	(5.21)	(−1.22)	(6.76)	(5.26)	(3.40)
Leverage	0.0450***	0.0389***	0.0583***	2.5182***	2.8296***	0.4783*
	(5.02)	(4.07)	(3.67)	(4.88)	(4.88)	(1.74)
Cash	0.0258***	0.0209***	0.0316**	0.7248*	0.5976	1.4282***
	(4.18)	(3.11)	(2.28)	(1.95)	(1.55)	(7.39)
Tobin's Q	0.0011**	0.0028***	0.0008	0.1357***	0.1640***	0.1101***
	(2.30)	(4.83)	(0.65)	(5.62)	(5.21)	(7.92)
Constant	−0.0605***	−0.1007***	0.0595	−6.7754***	−5.9741***	−4.8777***
	(−4.12)	(−5.34)	(1.13)	(−7.73)	(−5.91)	(−4.92)
Firm Fixed Effects			X			X
Year Fixed Effects		X			X	
Prefecture Fixed Effects		X			X	
Prefecture-Year Fixed Effects			X			X
Observations	9,153	9,153	9,153	12,078	12,078	12,078
Adjusted R^2	0.018	0.048	0.156	0.055	0.162	0.842

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

Table 10: **Related-Party Borrowing and Investment after Peer Punishment**

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

$$Related\ Party\ Outcome_{i,p,t} = \alpha + \beta SOE_{i,p,t} \times After\ Peer\ Punishment_{p,t} + \gamma_1 SOE_{i,p,t} + \gamma_2 After\ Peer\ Punishment_{p,t} + X'\delta + \eta_i + \eta_p + \eta_t + \epsilon_{i,p,t},$$

where $Related\ Party\ Outcome_{i,p,t}$ is either the amount of bank borrowing related parties obtain scaled by previous end-of-year total assets or investment by related parties scaled by previous end-of-year total assets of firm i in prefecture p in year t; $SOE_{i,p,t}$ is a dummy variable that equals 1 if listed company i in prefecture p was an SOE in year t, and 0 otherwise; $After\ Peer\ Punishment_{p,t}$ is a dummy variable that equals 1 if prefecture p has faced at least one punishment of a locally headquartered firm as of year t, and 0 otherwise; X 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; η_i, η_p, and η_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). Columns (1)-(3) report results for bank borrowing scaled by total assets, whereas columns (4)-(6) report results for investment scaled by total assets. The sample period is 1997 to 2014.

	Bank Borrowing / Total Assets			Δ Fixed Assets / Total Assets		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0047	0.0070		−0.0085	0.0325*	0.0513
	(0.60)	(0.33)		(−0.90)	(1.82)	(1.28)
After Punishment × SOE	−0.0472***	−0.0429**	−0.0673**	−0.0086	−0.0261**	−0.0328
	(−2.63)	(−2.44)	(−2.16)	(−0.81)	(−2.42)	(−1.46)
SOE	0.0304*	0.0373**	0.0576*	0.0087	0.0223**	0.0275
	(1.88)	(2.39)	(1.97)	(0.97)	(2.36)	(1.28)
Total Assets	−0.0142***	−0.0195***	−0.0205*	0.0057**	0.0064**	0.0026
	(−3.72)	(−4.23)	(−1.72)	(2.17)	(2.40)	(0.22)
Leverage	0.1159**	0.0433	0.0201	0.0404	0.0207	0.0086
	(2.29)	(0.94)	(0.16)	(1.22)	(0.51)	(0.08)
Tobin's Q	0.0101**	0.0090**	0.0097	0.0115***	0.0106***	0.0135**
	(2.49)	(1.99)	(1.25)	(4.29)	(3.79)	(2.20)
Cash	−0.0156	0.0043	−0.0137	0.0510	0.0627*	0.0883
	(−0.35)	(0.10)	(−0.11)	(1.65)	(1.95)	(1.21)
Constant	0.3664***	0.5410***	0.5150**	−0.0859	−0.1434**	−0.0536
	(4.10)	(4.80)	(2.02)	(−1.49)	(−2.29)	(−0.20)
Firm Fixed Effect			X			X
Year Fixed Effect		X			X	
Prefecture Fixed Effect		X			X	
Prefecture-Year Fixed Effect			X			X
Observations	2,509	2,509	2,509	10,645	10,645	10,645
Adjusted R^2	0.025	0.164	0.572	0.003	0.019	0.061

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

Francesco D'Acunto, Michael Weber, and Jin Xie

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 privated related parties.

中国证券监督管理委员会
CHINA SECURITIES REGULATORY COMMISSION
证券期货监督管理信息公

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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）

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

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

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

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

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

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

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

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

中国证券监督管理委员会

二〇〇四年七月二十七日

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

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

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

where $Punishment\ Prefecture\ Year_{r,t}$ is a dummy variable that equals 1 if a punishment of a listed firm due to wrongdoing in loan guarantees to private related parties happens in prefecture r 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, a dummy variable for whether the prefecture changed its local party secretary around the year, the logarithm of the number of public firms operating in the prefecture-year, the share of SOEs as a percentage of all firms in the prefecture. The following variables are computed at the province-year level: an index of the strength of the government ownership of the local companies, an index of the development of non-SOE firms, an index of the development of local product markets, an index of the development of local input markets, and index of the development of local financial intermediation. The sources for province-year-level data is Fan et al. (2011) for the years 1997-2007 and Fan et al. (2016) for the years 2008-2014; η_r and η_t represent full sets of prefecture and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (r). The sample period is 1997 to 2014.

	Outcome Variable: Dummy=1 if Punishment Happens in a City and Year																	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Log GDP	-0.00																	-0.00
	(0.02)																	(0.02)
Employment Rate		-0.04																0.07
		(0.09)																(0.15)
Log Population			-0.07															-0.02
			(0.07)															(0.07)
Log Population Density				-0.00														-0.00
				(0.02)														(0.01)
Employment % in Heavy Manufacturing					-0.04													-0.10
					(0.05)													(0.10)
Employment % in Light Manufacturing						0.09*												0.08
						(0.05)												(0.05)
Employment % in Services							-0.07											-0.06
							(0.06)											(0.04)
Fiscal Deficit								-0.10										0.00
								(0.07)										(0.03)
Within 1 Year after Mayor Turnover									0.00									0.00
									(0.01)									(0.01)
Within 1 Year after Local Party Secretary Turnover										0.00								-0.00
										(0.01)								(0.01)
Government-Market Connectivity Index											0.00							0.00
											(0.01)							(0.01)
Non-SOE Development Index												0.00						0.00
												(0.01)						(0.01)
Product-Market-Development Index													0.01					0.01
													(0.00)					(0.00)
Input-Market-Development Index														0.00				0.00
														(0.00)				(0.00)
Financial Intermediation & Law Index															-0.00			-0.00
															(0.00)			(0.00)
Log Number of Public Firms																0.02*		0.03**
																(0.01)		(0.02)
SOE as a Percentage of Total # of Firms																	-0.01	-0.01
																	(0.01)	(0.01)
Constant	0.06	0.03**	0.47	0.05	0.03***	-0.02	0.06*	0.04***	0.03***	0.03***	0.02	0.01	-0.01	0.02	0.03*	-0.01	0.03***	0.04
	(0.14)	(0.01)	(0.38)	(0.10)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.45)
Year Fixed Effect	X	X	X	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	X	X	X
Observations	3696	3698	3699	3696	3671	3698	3699	3146	3701	3701	3701	3701	3701	3701	3701	3701	3701	3114

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

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

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

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 (18) of Table A.1; η_p and η_{t+n} represent full sets of prefecture and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). The sample period is 1997 to 2014.

	Punishment in Year t+n?				
	n=1	n=2	n=3	n=4	n=5
	(1)	(2)	(3)	(4)	(5)
First Punishment in Prefecture (Year t)	0.0683	-0.0432	-0.0789	-0.0239	-0.0121
	(0.0541)	(0.0534)	(0.0521)	(0.0521)	(0.0564)
Constant	-0.4367	-0.8039	-0.0473	0.1112	-0.4580
	(0.8172)	(1.0907)	(0.6625)	(0.5947)	(0.6916)
Prefecture-level Controls	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Prefecture Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	2831	2548	2267	1985	1703
Adjusted R-squared	0.152	0.166	0.172	0.160	0.175

Table A.3: **Loan Guarantees after Peer Punishment: No Industry Peers**

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

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

where Loan Guarantees$_{i,p,t}$ is the amount of loan guarantees extended by firm i in prefecture p in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,p,t}$ is a dummy variable that equals 1 if listed company i in prefecture p was an SOE in year t, and 0 otherwise; 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 0 otherwise; X 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; η_i, η_p, and η_t represent full sets of firm, prefecture, and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). Columns (1)-(3) report results for the overall amount of loan guarantees, whereas columns (4)-(6) report results for net guarantees. The sample period is 1997 to 2014.

	Total Provided Guarantees			Net Provided Guarantees		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0148***	0.0011		0.0205***	0.0080	
	(2.81)	(0.19)		(2.78)	(0.89)	
After Punishment × SOE	−0.0114**	−0.0101**	−0.0219***	−0.0185***	−0.0123*	−0.0209*
	(−2.12)	(−2.02)	(−2.63)	(−2.69)	(−1.74)	(−1.76)
SOE	−0.0243***	−0.0046	0.0157**	−0.0114**	0.0010	0.0149**
	(−6.58)	(−1.37)	(2.00)	(−2.01)	(0.18)	(2.01)
Total Assets	0.0117***	0.0024	0.0107*	0.0093***	0.0036	0.0146**
	(5.01)	(0.92)	(1.91)	(4.40)	(1.31)	(2.41)
Leverage	0.0448***	0.0471***	0.0322	−0.0330	−0.0244	−0.0378
	(2.77)	(3.00)	(1.34)	(−1.04)	(−0.75)	(−1.10)
Cash	−0.0211**	−0.0284***	−0.0067	0.0122	0.0186	0.0266
	(−2.28)	(−2.89)	(−0.47)	(1.06)	(1.53)	(1.34)
Tobin's Q	−0.0019**	−0.0033***	0.0004	0.0051***	0.0016	0.0006
	(−2.12)	(−2.74)	(0.24)	(4.09)	(0.98)	(0.34)
Constant	−0.2014***	0.0482	−0.2014*	−0.2234***	−0.0570	−0.3269**
	(−4.15)	(0.81)	(−1.65)	(−5.00)	(−0.89)	(−2.50)
Firm Fixed Effect			X			X
Year Fixed Effect		X			X	
Prefecture Fixed Effect		X			X	
Prefecture-Year Fixed Effect			X			X
Observations	12,470	12,470	12,470	12,470	12,470	12,470
Adjusted R^2	0.059	0.137	0.318	0.016	0.103	0.323

t-statistics in parentheses

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

Table A.4: Results Across Industries with High and Low Concentration (HHI)

This table reports estimates of β from the following linear equation

$$Outcome_{i,r,t} = \alpha + \beta SOE_{i,r} \times After\ Peer\ Punishment_{r,t} + \gamma_1 SOE_{i,r} + \gamma_2 After\ Peer\ Punishment_{r,t} + X'\delta + \eta_i + \eta_{kt} + \epsilon_{i,r,t},$$

where $Outcome_{i,k,r,t}$ is the outcome variable for firm i in region r in year t reported on top of each column; $SOE_{i,k,r}$ is a dummy variable that equals 1 if listed company i in industry k was an SOE at the time region r faced the first punishment of a locally headquartered firm due to excessive extension of loan guarantees to related parties, and zero otherwise; $After\ Peer\ Punishment_{r,t}$ is a dummy variable that equals 1 if region r 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, financial leverage, total amount of cash, and Tobin's Q as a proxy for firms' investment opportunities; η_i and η_{kt} represent full sets of firm and prefecture-year fixed effects, respectively. Odd columns report results for the subsample of firms in the industries above the median by HHI, whereas even column for the firms in industries below the median by HHI. We compute HHI at the level of 4-digit SIC industries. We cluster standard errors at the level of the prefecture (p). Columns (1)-(2) report results for the overall amount of loan guarantees, whereas columns (3)-(4) report results for net guarantees. The sample period is 1997 to 2014.

	Total Provided Guarantees		Net Provided Guarantees	
	High HHI (1)	Low HHI (2)	High HHI (3)	Low HHI (4)
After Punishment * SOE	−0.0209 **	−0.0272 **	−0.0212*	−0.0324*
	(−2.38)	(−2.28)	(−1.91)	(−1.68)
SOE	0.0103	0.0261 **	0.0122	0.0237
	(0.99)	(2.31)	(0.96)	(1.59)
Ln(Total Assets)	0.0065	0.0175 **	0.0066	0.0270***
	(0.92)	(2.46)	(0.65)	(3.82)
Leverage	−0.0215	0.0472	−0.1241 **	−0.0091
	(−0.72)	(1.42)	(−2.61)	(−0.25)
Cash	0.0080	−0.0084	0.0404	0.0293
	(0.43)	(−0.45)	(1.57)	(1.25)
Tobin's Q	−0.0014	0.0037	−0.0034	0.0064 **
	(−0.75)	(1.59)	(−1.30)	(2.58)
Firm Fixed Effects	X	X	X	X
Prefecture-Year Fixed Effects	X	X	X	X
Observations	5,320	5,298	5,320	5,298
Adjusted R-squared	0.360	0.311	0.324	0.306

Table A.5: **Loan Guarantees after Peer Punishment: Young vs. Old CEOs**

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

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

where $Loan\ Guarantees_{i,p,t}$ is the amount of loan guarantees extended by firm i in prefecture p in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,p,t}$ is a dummy variable that equals 1 if listed company i in prefecture p was an SOE in year t, and 0 otherwise; $After\ Peer\ Punishment_{p,t}$ 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 0 otherwise; X 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; η_i, η_p, and η_t represent full sets of firm, prefecture, and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). Columns (1)-(3) report results for firms with CEOs below age 55, whereas columns (4)-(6) report results for firms with CEOs above age 55. The sample period is 1997 to 2014.

	Age <= 55			Age > 55		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0181***	0.0051		−0.0043	−0.0314*	
	(3.36)	(0.82)		(−0.31)	(−1.76)	
After Punishment × SOE	−0.0158***	−0.0137***	−0.0213**	0.0193	0.0203	0.0350
	(−2.82)	(−2.63)	(−2.47)	(1.31)	(1.15)	(0.39)
SOE	−0.0233***	−0.0048	0.0115	−0.0492***	−0.0285**	0.0125
	(−6.28)	(−1.25)	(1.38)	(−4.09)	(−2.00)	(0.14)
Total Assets	0.0137***	0.0043	0.0118*	0.0069**	0.0001	0.0080
	(4.98)	(1.42)	(1.81)	(2.11)	(0.01)	(0.09)
Leverage	0.0396**	0.0386**	0.0248	0.0213	0.0561	0.0431
	(2.23)	(2.19)	(0.96)	(0.47)	(1.12)	(0.15)
Cash	−0.0254**	−0.0300***	−0.0021	−0.0283	−0.0461**	−0.0774
	(−2.52)	(−2.90)	(−0.14)	(−1.43)	(−1.99)	(−0.37)
Tobin's Q	−0.0015	−0.0033***	0.0004	−0.0022	−0.0024	0.0093
	(−1.50)	(−2.63)	(0.21)	(−1.19)	(−0.97)	(0.43)
Constant	−0.2441***	0.0144	−0.2214	−0.0764	0.1030	−0.1609
	(−4.26)	(0.21)	(−1.58)	(−1.07)	(0.91)	(−0.08)
Firm Fixed Effect			X			X
Year Fixed Effect		X			X	
City Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	11,176	11,176	11,176	1,413	1,413	1,413
Adjusted R^2	0.065	0.147	0.326	0.050	0.136	0.351

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

Table A.6: **Loan Guarantees after Peer Punishment: Overseas vs. Non-Overseas Experience**

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

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

where Loan Guarantees$_{i,p,t}$ is the amount of loan guarantees extended by firm i in prefecture p in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,p,t}$ is a dummy variable that equals 1 if listed company i in prefecture p was an SOE in year t, and 0 otherwise; 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 0 otherwise; X 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; η_i, η_p, and η_t represent full sets of firm, prefecture, and year fixed effects, respectively. We cluster standard errors at the level of the prefecture (p). Columns (1)-(3) report results for firms with CEOs who have overseas work experience, whereas columns (4)-(6) report results for firms with CEOs who have no overseas work experiece. The sample period is 1997 to 2014.

	Oversea Work Experience			No Oversea Work Experience		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	−0.0061	−0.0109		0.0169∗∗∗	0.0021	
	(−0.40)	(−0.25)		(3.06)	(0.34)	
After Punishment × SOE	−0.0139	0.0091	0.0084	−0.0127∗∗	−0.0110∗∗	−0.0232∗∗
	(−0.65)	(0.35)	(0.12)	(−2.26)	(−2.03)	(−2.48)
SOE	−0.0245	−0.0333	0.0156	−0.0243∗∗∗	−0.0042	0.0167∗
	(−1.30)	(−1.36)	(0.23)	(−6.81)	(−1.27)	(1.80)
Total Assets	0.0056	0.0060	−0.0365	0.0129∗∗∗	0.0035	0.0112∗∗
	(0.64)	(0.54)	(−1.07)	(5.59)	(1.26)	(2.03)
Leverage	0.0832	0.1074	0.1255	0.0322∗∗	0.0312∗∗	0.0237
	(1.08)	(1.20)	(0.67)	(1.99)	(2.13)	(0.95)
Cash	−0.0406	−0.0049	−0.0331	−0.0253∗∗	−0.0317∗∗∗	−0.0151
	(−1.11)	(−0.13)	(−0.31)	(−2.42)	(−2.84)	(−1.01)
Tobin's Q	−0.0092∗∗∗	−0.0088	−0.0011	−0.0012	−0.0028∗∗	0.0010
	(−2.88)	(−1.58)	(−0.13)	(−1.31)	(−2.49)	(0.63)
Constant	−0.0225	−0.0118	0.9252	−0.2278∗∗∗	0.0269	−0.2234∗
	(−0.12)	(−0.05)	(1.19)	(−4.78)	(0.43)	(−1.83)
Firm Fixed Effect			X			X
Year Fixed Effect		X			X	
City Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	847	847	847	12,122	12,122	12,122
Adjusted R^2	0.034	0.175	0.560	0.062	0.143	0.319

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