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

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– PRELIMINARY DRAFT –

Abstract

Direct experience of a peer's punishment might make salient the probability and negative consequences of facing punishment, and hence induce a change in the behavior of non-punished peers. We test for this mechanism in a unique setting. After observing peer firms punished for wrongdoing, Chinese listed State Owned Enterprises (SOEs) – which are less disciplined by traditional governance mechanisms than non-SOEs – cut the resources they tunnel to related private parties via loan guarantees, move to more independent boards, cut inefficient investment, and increase total factor productivity. SOEs experience positive cumulative abnormal returns around the announcements of peers' punishment, which suggests a positive association between peers' punishment and shareholder value. SOEs do not shift to more opaque forms of tunneling – the bank credit and investment of related parties drop and do not revert after peers' punishment.

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

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*All errors are our own.

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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 about the probability of being punished and the negative effects of punishment upwards once both dimensions become salient, and hence react by reducing potential wrongdoing. Testing for this mechanism requires observing two peers of a punished economic agent, one of whom is more prone to react to the salience of punishment than the other, all else equal.

In this paper, we use Chinese corporations to test for the sobering effect of peers' punishment for three reasons. First, Chinese listed firms include several State Owned Enterprises (SOEs). Listed SOEs are more insulated than listed non-SOEs from internal and external governance mechanisms, such as shareholder activism, board monitoring, or governance through trading. Salience of peers' punishment should therefore affect listed SOEs more than otherwise similar listed non-SOEs, for which traditional governance mechanisms are already in place. Second, managers of SOEs likey expect a low probability of punishment, because the government – their main shareholder – can exert moral suasion on regulators.[2] The salience of punishment might therefore affect SOEs' beliefs more than non-SOEs' beliefs, all else equal. Third, Chinese regulators have increased the punishment of related party transactions involving listed firms over the last few years, especially in the realm of tunneling through inter-corporate loan guarantees. Jiang, Lee, and Yue (2011) document the effectiveness of these punishment measures on the extent of wrongdoing in punished firms, which differ from the recent anti-corruption campaign for which research has not found significant effects on Chinese firms' outcomes (Griffin, Liu, and Shu (2017)).

We find that after Chinese regulators sanction a listed firm for tunneling via inter-corporate loan guarantees, non-punished listed SOEs operating in the same location cut their loan guarantees to related private parties substantially compared to listed non-SOEs in the same location and to listed firms in different locations. This effect is economically and statistically significant. After a peer is punished, SOEs in the same location reduce the

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

[2] In our sample listed non-SOEs are about three times as likely to face punishment than listed SOEs. We also find evidence for "regulatory tolerance" toward listed SOEs, because conditional on punishment, SOEs perpetrate wrongdoing for a longer period than non-SOEs.

amount of loan guarantees over total assets by 2.4 percentage points – about one quarter of a standard deviation of the amount of loan guarantees in the sample. Moreover, SOEs in the same location are 43% more likely to move from CEO duality – the CEO chairs the board – to a more independent board structure. These results suggest that SOEs react to the punishment of local peers by aligning their action with the interests of minority shareholders.

The mechanism we propose requires that the effects of peers' punishment on firms' behavior are stronger the more salient is the peer punishment event. Consistent with the salience-of-punishment mechanism, we find the effect of peers' punishment on firms' behavior is more pronounced after punishment events that are more salient, which we proxy in two ways – the share of total news in China in the 30 days around the investigation window that cover the punished firm, and the size of the drop in the abnormal returns of punished peers' stock prices in the days around the announcement of the punishment.

The improved governance of SOEs has real effects. A substantial decrease in SOEs' investment in fixed assets follows the cut of loan guarantees to related private parties. SOEs cut their investment in fixed assets by 1.1 percentage point, which is 18% of the average change of fixed assets in the sample. SOEs' reaction to peers' punishment might improve firm value by eliminating inefficient investment, or reduce firm value by eliminating efficient investment. We find the cuts lead to significant improvements in total factor productivity (TFP) in the medium run, which suggests SOE managers were not employing the firms' resources efficiently before cutting investment. Event studies corroborate this interpretation, because SOEs' cumulative abnormal returns are positive in the medium run and substantially higher than the (statistically insignificant) cumulative abnormal returns of non-SOEs after punishment of a local peer.

A concern with our interpretation of the results is that SOEs merely move to more opaque forms of tunneling after a peers' punishment, and hence punishments do not have a sobering effect on the unpunished. Contrary to this alternative interpretation, we find that the cut in inter-corporate loan guarantees has negative real effects on listed SOEs' private related parties, which cut their investment in fixed assets and reduce bank borrowing significantly after the drop in guarantees. This result suggests that listed SOEs do not engage in alternative opaque methods to tunnel resources to their private related parties. Under the presumption that lenders would have not financed related parties'

investment projects absent SOEs' loan guarantees, lower investment by private related parties might avoid a wasteful use of financial and real resources.

Our 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 2010, as well as fixing the SOE status of firms at the time in which they experience the punishment of a local peer. The latter test is important, because the Chinese government implemented a massive wave of privatization of SOEs in the 2000s, and hence the SOE status of a large 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 we verify that we fail to replicate our effects in this setting.

Overall, our evidence is consistent with a sobering effect of observing peers' punishment on the behavior of Chinese listed SOEs. An interesting feature of the mechanism we study as a form of corporate governance is its cost effectiveness. This mechanism requires that regulators monitor and punish only a small set of listed firms, thus reducing the costs of monitoring before and after detecting wrongdoing substantially. Punishing the wrongdoing of one firm might eliminate the misbehavior of peer firms without any need to monitor or investigate them.

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., see Dyck, Morse, and Zingales (2010), Dyck, Morse, and Zingales (2016), Zeume (2017), and Bennedsen and Zeume (2017)). We focus on the reputational sanctions attached to the punishment of wrongdoing by stock-market regulators. Armour, Mayer, and Polo (2017) find that the announcement of punishment of listed firms because of wrongdoing has far-reaching implications for firm value – the size of the negative effect of punishment announcement on firms value is higher than the pecuniary size of the direct punishment itself. They interpret the additional negative effect of punishment announcements as a reputational sanction effect. In this paper, we build on the far-reaching effects of reputational sanctions to study the *indirect* effects of sanctions on *non-punished* peer firms. The finding that sanctioning firms for wrongdoing has an

effect on the behavior of non-punished peer firms suggests that the fear of reputational sanctions is a relevant governance mechanism when other well-studied mechanisms are ineffective. This governance mechanism is inexpensive relative to the direct monitoring of firms by activist shareholders or the direct investigation of listed firms by the market authority. Researchers have also studied the effect of monitoring on the side of courts, as opposed to the market authority for listed firms (e.g., see Grinstein and Rossi (2016)).

Our paper also contributes to the literature on the effects of salience on decision-making. Theories exist to explain 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 settings to test for the effects of such characteristics on individual decision-making (e.g., see Benjamin et al. (2010), D'Acunto (2016b), and D'Acunto (2017)). In corporate finance, Dessaint and Matray (2017) find that managers accumulate cash holdings to insure their firms against disaster risk after observing the effects of a natural disaster on firms closeby, which they argue increases managers' expected probability of disasters through salience of disaster risk.

We also relate to the large body of work on corporate governance mechanisms in the presence of blockholders 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, 2016a). Managers might appropriate or destroy shareholder value if internal governance mechanisms, such as board oversight, are ineffective (Hermalin and Weisbach, 2017). External mechanisms like governance through trading might also be ineffective, especially if the government is a majority shareholder and does not care about fluctuations in stock prices (Edmans and Manso, 2011). 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. To safeguard the rights of minority shareholders, stock-market regulators monitor listed firms and sanction wrongdoing, producing both direct and a 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 its 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 salience of the direct and reputational damage managers *could* face in case of punishment.

Finally, we contribute to the body of research on governance and corporate outcome distortions in settings with large governmental ownership of productive resources (Shleifer (1998), Bortolotti and Faccio (2009)) and in which political connections are valuable to firms (Faccio, 2006).

II Institutional Setting

In this section, we discuss two important features of our institutional setting. First, we describe the process through which State Owned Enterprises (SOEs) emerged in China. The origin of the difference between SOEs and non-SOEs is crucial to our analysis, because as we describe in Section V, our empirical strategy compares the reaction of SOEs and non-SOEs to the same shocks. 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 State Owned Enterprises and Business Groups in China

The Chinese government imposed the transition from a Communist economic system to a market-economy system in two stages, starting with the first set of reforms in 1978. Although in the first phase of market reforms the government maintained a strict direct control over the economy for decades, it also promoted a gradual, experimental, and pragmatic approach to improve the performance of corporations, which were (Lin (2009) and Xu (2011)). To maintain control over economic activity while allowing for private ownership, the government developed a system that has been labeled "networked hierarchy."

The networked hierarchy consists of vertically-integrated corporate groups that are organized by the State-Owned Assets Supervision and Administration Commission of the

State Council (SASAC). The hierarchical aspect of business groups obtains two forms. First, the vertical integration of firms along the production chain provides subsidiaries at the top of the production chain substantial power on the corporate policies implemented by subsidiaries at the bottom of the production chain. Second, the coordination of production activities on the part of SASAC reflects to the top-down character of policy formulation and transmission typical of an authoritarian political regime.

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, but upstream sectors in the networked hierarchy were still organized as government-controlled monopolies through State Owned Enterprises (SOEs). In the early 2000s, SEOs also started to be gradually privatized. Between 2001 and 2004, the number of SOEs in China decreased by 48 percent. 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). Throughout the second stage of economic reform, surviving SOEs reinforced their monopoly power in upstream sectors, which are generally in nontradable or regulated sectors. Importantly, surviving SOEs were still protected from foreign competitions following the WTO entry. In contrast, non-SOEs face fierce competition in the downstream sectors, which are tradable and 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 while 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.

B Guranteed Loans

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 of strategic

importance 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 has been widely recognized by regulators, practitioners, and academics, loans to SOEs by the major Chinese banks account for the largest part of the nonperforming loans (NPL) in China.

Unlike other countries, where governments use guarantees to finance small firms or to support homeownership, the role of guarantor in individual loans falls to individual firms in China. Jiang, Lee, and Yue (2011) describe in detail the mechanisms through which Chinese corporation tunnel resources to related private parties through inter-corporate loans. 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. Since 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. A quarter of loans in China's banking system was then backed by guarantees (McMahon, 2014).

China's guarantee chain can cause the risk of guaranteed loans to spread over the networked hierarchy formed by related parties in certain regions in the scenario in which a public company services as a guarantor, shareholder wealth is at risk. On August, 2003, CSRC issued a notice to regulate guaranteed loans provided by public firms. According to the notice, firms should follow the following criteria when guaranteeing for their related parties. First, the amount of guarantee provided by a public firm cannot excess 50% of its net worth. Second, public firms are not allowed to provide guarantees for borrowers whose leverage ratio excesses 70%. Third, public firms cannot guarantee related companies or natural persons in which they hold less than 50% shares. Last, the guarantee should be approved by at least two thirds of directors in the board meeting or approved in the shareholder meeting.

III Data

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

A Data Sources

Firm-level Information: Listed Firms. Our main source is the *China Stock Market and Accounting Research* (CSMAR) database, which contains information about all Chinese listed firms in terms of balance sheet and other accounting variables, ownership structure, outstanding bank loans, and financial fraud events sanctioned by the market authority.

We use the information in CSMAR to construct all the accounting-based observables we use in the analysis for our sample of listed firms. We collect financial data from the CSMAR, including the establishment and IPO years, total assets, total and long-term liabilities, fixed assets, cash, operating sales, net income, distress identification (coded as "ST" firms), and cross-listing information, and number of employees. CSMAR also calculates variables such as Tobin's Q.

Starting from 2003, CSMAR discloses identities for public firms' controlling shareholders and ultimate owners. It also indicates whether the nature of controlling shareholder, or ultimate owner, is SOEs or non-SOEs. However, CSMAR does not distinguish between companies and State Asset Management Company within SOEs or between legal entities and natural persons within non-SOEs. For this reasons, we manually read the names of shareholders to further verify their identities and double-check their government or private nature.

We extract information about public firms' city location through company addresses in the IPO filling. Excluding the two special administrative regions (i.e., Hong Kong and Macau), the constitution of China provides for six 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 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.

Firm-level Information: Related Parties. 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 about related parties, which are private firms. The first is Orbis Asia-Pacific. Orbis collects companies filed accounts to Chinese Administration of Industry and Commerce, National Tax Bureau, and National Bureau of Statistics of China (NBSC). It includes 26 million active companies in mainland China. We extract company financial statements from Orbis on the sample period of 2005-2014. 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 state-owned firms, and non-state firms with revenues above 5 million yuan (about US$ 600,000). We extract the name list from the CSMAR related party transaction database. We create an algorithm to match the names of related parties with private firms included by two databases.

Loan Guarantee Fraud Events. We identify all the fraud events related to loan guarantees for private related parties of listed firms the Chinese market authority punished over time from the CSRC's *Enforcement Action Research Database*, which is part of CSMAR. 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 source, which include CSRC public announcements, information firms under investigations make public, and newspaper articles. The time period we access for our analysis is from 1997 to the end of 2014.

This database provides detailed and standardized information regarding fraud events, such as the date in which a punishment for a firm committing fraud is announced, the market authority that announced the fraud event, the time period during which fraud was committed, the reasons for punishment, the extent of the punishment, as well as a detailed description of the activities in which the listed company engaged, which violated stock-market transparency regulation.

Although anecdotal evidence shows that 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 hand collected corporate fraud events which involved illegal activity in relation to the provision of loan guarantees from listed firms to private

related parties. Specifically, we classify fraud events as related to loan guarantees either if the fraud database cites loan guarantee misconduct as at least one the reasons for punishment or if the description of the fraudolent activities includes the word "guarantee". Throughout our sample period (1997-2014), we obtained a list of 285 corporate fraud events involving irregular loan guarantees in which public firms and their related parties are involved.

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 is a prefecture, the earlier was the first punishment event for wrongdoing related to related-party loan guarantees of a local listed firm in the prefecture. We observe substantial spatial variation in the timing of the first punishments, which range throughout our sample period from 2001 to 2014. Moreover, no substantial spatial clustering of the timing of first punishments is detectable in the map, which suggests that concerns about spatial correlation across observations in neighboring prefectures are not relevant in our context.

Loan Guarantees to Related Parties. To track the direction and amount of guarantees either provided or received by public firms, we rely on highly disaggregated related-party transaction data. The disclosure of related party transactions became mandatory for all Chinese public firms starting in 2004.

We consider both the total gross amount of loan guarantees as well as the net amount of loan guarantees, 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.

Loan-level Data. 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 about loan characteristics based on company announcements for the period from 1996 to 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, and whether the loan was guaranteed by a third party and the purpose of the loan.

B Summary Statistics

The firms we use in the analysis are distributed throughout China, which guarantees that our results do not rely on peculiar areas and prefectures. The bottom map of Figure 1 describes the spatial distribution of the firms in our sample – the darker is a prefecture, the higher the number of firms in the prefecture that enter our sample. In particular, the map shows that the firm in our sample are not concentrated only in the largest Chinese urban conglomerates – e.g, Shanghai and Beijing – or only in special economic zones like Shenzhen, or even only in coastal areas.

Table 1 reports the 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 of Table 1 refers to the our main sample of Chinese listed firms that are headquartered in a region in which at least one listed company was punished by the market authority 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 region was punished by the market authority. About 42% of our observations refer to years after the punishment events. *SOE* is a dummy variable that equals 1 if the firm is a State Owned Enterprise, which is the case for half of your firm×year observations.

Our main object of interest is the extent of loan guarantees listed firms extend to their related parties, for which we report two alternative definitions. *Total Provided Guarantees* are the overall amount of loan guarantees listed firms extend to related parties, whereas *Net Provided Guarantees* are 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 – this variable obtains negative values when a listed firm is a net receiver of guarantees from related parties.

We then provide statistics for the financials and the other firm-level variables we observe, which we use in the analysis. We winsorize financial variables at the 1-99 percent

levels to ensure that outlier observations do not affect our results.

Total factor productivity (TFP) is measured following the procedure described by Olley and Pakes (1996), who provide a semi-parametric method to estimate TFP which controls for simultaneity and selection biases in OLS estimation. *CEO Duality* is a dummy variable that equals 1 if the CEO of the company is also the head of the board of directors, which reduces the independence of the board. We see that 13% of our observations have dual CEOs, whereas 28% of our observations refer to years in which firms are covered by at least one analyst. 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 use the scaled change in fixed assets as our proxy for investment as opposed to capital expenditures, because we cannot observe the balance sheet information we would need to compute capital expenditures due to the different reporting requirements of Chinese listed firms with respect to US or European firms. As far as financials are concerned, *Leverage*, measured as financial leverage over total end-of-previous-year assets is on average low (6%) with more than 90% of the sample having a leverage below 20%. We also use the share of cash-like instruments over total end-of-previous-year assets, which is 16% on average. Finally, Tobin's Q is larger than 1 for both the mean and average firm in the sample.

Panel B of Table 1 refers to the individual bank loans we observe for firms in our main sample. Notably, we observe more loans in the periods after the punishment of a regional peer compared 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 firms in the sample that are SOE ($31\% < 50\%$). The firm-level characteristics weighted at the level of bank loans do not seem to display any other substantial departure for the main sample of firm×year observations.

Finally, 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. Note that we have proportionally more related parties in the period after peer punishments than before, compared to the main sample of listed firms ($61\% > 42\%$), which suggests that the number of private firms to which listed firms are related has increased over time. We also see that SOEs are less likely to have private related parties compared to SOE's representation in 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 average and standard deviation of the distributions.

IV Hypothesis Development

Several governance mechanisms the literature has studied extensively might be ineffective for firms in which the government is a majority shareholder. Internal governance mechanisms might be muted if the composition of the board is not independent. This is likely to be the case for Chinese SOEs with CEO duality, in which the CEO of the company is also the president of the board of directors. At the same time, external governance mechanisms such as the threat of trading on the part of private blockholders might also be ineffective, because the majority shareholder does not care for the volatility of stock prices in the medium run, and the CEO can accuse private blockholders of short-termism and speculative behavior.

For these reasons, an effective governance mechanism in SOEs should affect managers' incentives to engage in wrongdoing directly. A prominent channel to affect managerial incentives is managers' own career concerns. If managers perceive that the probability of being punished for wrongdoing and the reputational losses for wrongdoing are large, then they will be more likely to abstain from conducting wrongdoing.

We argue that a channel that affects managers' perception of the probability of being punished conditional on wrongdoing as well as the extent of expected losses in case of wrongdoing is the salience of these dimensions whenever managers observe them happening to firms similar to the ones they run.

This effect of salience of the likelihood and effects of punishment should affect the behavior on SOE CEOs more than non-SOE CEOs for at least two reasons. First, as we argue above, other governance mechanisms that reduce non-SOEs managers' incentives to engage in wrongdoing are muted for SOE managers. Even if peer punishment increased the salience of the likelihood and effects 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, SOE CEOs might expect their firms are on average less likely to be punished

by the market authority than other firms, because the government has influence on the market authority and might hinder punishment. In the data, we do confirm that SOEs are on average less likely to be punished than non-SOEs, and conditional on punishment, SOEs benefit from higher "regulatory tolerance," in the sense that punished SOEs had perpetrated wrongdoing for more time than punished non-SOEs. Punishment of peer firms thus is likely to increase the salience of the likelihood of punishment and extent of losses due to punishment more for SOE CEOs than for non-SOE CEOs.

In our empirical analysis we compare the behavior of SOE firms and non-SOE firms that operate in the same region and industry, after one of their peer firms is punished for wrongdoing compared to before. At the same time, it is important to stress that being an SOE or non-SOE firm is not a randomly assign status in our setting.

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

Hypothesis 1: *After punishment of a peer firm, SOE peers are more likely to reduce wrongdoing than non-SOE peers in the same region and industry.*

If the reaction to peers' wrongdoing reflects a broader concern by SOE CEOs of improving the public's perception that they do not engage in unethical behavior, we should observe that SOE CEOs are also more willing to allow stricter monitoring on the part of private shareholder and the public as an costly signal that they are not engaging in wrongdoing. As far as private shareholders are concerned, in our data we can observe a crucial aspect of listed firms' governance structure which previous research has related to high-quality internal governance, that is, the absence of CEO duality. CEO duality means that the firm's CEO is also the president of the board of directors, and hence has a strong influence in board decisions and monitoring effectiveness. Absence of CEO duality suggests that the board of directors of a listed company is more independent and less prone to managerial influence. As far as scrutiny from the broader public is concerned, one way we can observe in our data in which SOE CEOs could increase independent public scrutiny into their firms would be to push to obtain analyst coverage for their stock. We therefore bring to the data the following hypothesis:

Hypothesis 2: *After punishment of a peer firm, SOE peers are more likely to stop CEO duality and to obtain analyst coverage than non-SOE peers in the same region and*

industry.

The third prediction we bring to the data concerns the real effects of reducing wrongdoing in firms. If engaging in activities like tunneling resources from listed firms to private parents and subsidiaries was not detrimental to shareholders, we should find no effects of reducing wrongdoing on a company's investment. If this was the case the external governance mechanism we study would be irrelevant, as long as unethical behavior should be sanctioned only if it destroys shareholders value.

To the contrary, if reducing wrongdoing decreases CEOs' engaging in a wasteful use of the firm's resources which destroys shareholder value, we should observe that after the punishment of a peer SOE CEOs are more likely to cut on excessive investment, and to increase the efficiency of their use of labor and capital resources. In our setting, we do observe firm-level investment in fixed assets and we can measure firm-level total factor productivity, which we use to bring to the data our third hypothesis:

Hypothesis 3: *After punishment of a peer firm, SOE peers are more likely to decrease investment and increase total factor productivity than non-SOE peers in the same region and industry.*

The real outcomes we consider above are based on accounting-based dimensions that are reported by the firm's management, and hence might be prone to manipulation on the side of the management. We can obtain an independent assessment on whether the positive real effects of reducing wrongdoing are actual by observing the behavior of market participants around the punishment of peers, and hence after observing SEO CEOs reaction to such punishment events. If the positive real effects of reducing wrongdoing exist, we should detect them when comparing the cumulative abnormal returns of SOE stocks compared to non-SOE stocks after the punishment of a peer. We therefore propose the following hypothesis:

Hypothesis 4: *After punishment of a peer firm, the cumulative abnormal returns of SOE peers' stocks are higher in the medium run than the cumulative abnormal returns of non-SOE peers' stocks in the same region and industry.*

The last hypothesis we bring to the data aims to verify that cut in the tunneling

of resources from listed SOEs to their parents and subsidiaries in the form of loan guarantees does have sizable effects on the parents and subsidiaries' ability to access lending markets and to invest. If this was not the case, we would be concerned that SOE CEOs might cut loan guarantees but at the same time use alternative and more obscure ways to tunnel resources to related parties, and hence the expected benefits to private shareholders from a cut in loan guarantees might be ephemeral. To verify that the cut in loan guarantees to related parties does have real effects on private related parties, we therefore test the following hypothesis:

Hypothesis 5: *After punishment of a peer firm, the amount of credit related parties obtain as well as their investment decrease more for the related parties of SOE peers than for the related parties on non-SOE peers.*

V Empirical Strategy

To test the hypotheses described above empirically, we propose a difference-in-differences strategy. We compare a set of yearly outcomes measured at the firm level before and after the first time a peer firm is punished, and across listed SOE and listed non-SOE firms. A peer firm is a firm whose headquarters are the same region as the listed SOE and listed non-SOEs. The double difference we aim to assess is therefore as follows:

$$\begin{aligned}&(Outcome_{SOE,r,after} - Outcome_{SOE,r,before})\\&- (Outcome_{non-SOE,r,after} - Outcome_{non-SOE,r,before})\end{aligned}$$

where r indicates the region in which the SOE and non-SOE peers are headquartered, which defines peer status in our setting.

In order to implement this strategy, we will estimate a set of linear specifications which only exploit variation in outcome and control variables within firms and within years. Absorbing any systematic variation across firms allows to exclude that firm-level time-invariant characteristics explain the differential reaction to peer firms' punishment. Absorbing any systematic variation across firms observed in the same year allows us to exclude that time-varying economy-wide shocks all firms face might trigger the differential

reactions to the peer punishment we test in the data. We will therefore estimate linear specifications of the following type:

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

where the coefficient β captures the double difference defined above after partialling out firm-level characteristics (X) as well as firm and year fixed effects (η_i and η_t). Note that no firms in our sample change their main industry affiliation throughout the sample period, and hence 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 peer listed firm.

A Parallel-Trends Assumption

The validity of our difference-in-differences strategy relies on the assumption that non-SOE listed firms headquartered in region r represent a valid counterfactual for the behavior of SOE listed firms headquartered in the same region after the market authority imposes the first punishment of a listed firm in region r. This *parallel-trends* assumption states that 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. At most, to assess the plausibility of the assumption we need to interpret the estimate of coefficient β in equation (1) causally, we can test formally 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:

$$\begin{aligned} Outcome_{i,r,t} = \alpha + \sum_t \beta_t SOE_{i,r} \times Year_t \\ + \gamma_1 SOE_{i,r} + \gamma_2 \sum_t Year_t + X'\delta + \eta_i + \epsilon_{i,,r,t}, \end{aligned} \tag{2}$$

where $\sum_t \beta_t SOE_{i,r} \times Year_t$ 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 region r, and the other variables are defined as in equation (1).[3]

The null hypothesis that pre-trends are parallel across treatment and control group consists thus in assuming that each of the estimated coefficients β_t in equation (2) equals zero. We therefore estimate equation (2) 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 around the estimates. 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 that the trends on all our outcomes are parallel before the punishment events across listed SOEs and listed non-SOEs headquartered in the same region as the punished listed firm. Note that 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. This is because as we move further away from the punishment date we lose the observations that refer to listed firms in regions in which a peer was punished early in the sample, and hence for which we do not observe information for many years before the punishment date.

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 reassure us when assuming that listed non-SOE might represent a viable counterfactual for the behavior of listed SOEs in the same region had the punishment events not happened.

[3]Note that we wrote the full set of fixed effects ηt of equation (1) as $\sum_t Year_t$ in equation (2) to maintain symmetry the with the interaction term in the specification.

VI Reaction to Peers' Punishment: SOEs vs. non-SOEs

In this section, we describe the results for testing the hypotheses described in Section IV empirically.

A Loan Guarantees to Related Parties

Baseline Results. We first consider Hypothesis 1, which argues that after the punishment of a peer, SOEs are more likely to reduce wrongdoing than non-SOEs operating in the same region and industry. In our setting, we define wrongdoing as the extent of loan guarantees that listed firms extend to private parents and subsidiaries, i.e. their private related parties. We consider this form of wrongdoing, because in the data we observe the market authority's punishment of excessive extension of loan guarantees from listed firms to their private related parties as a form of tunneling of resources at the expense of the minority shareholders of listed firms.

Specifically, we estimate the following linear equation by ordinary least squares:

$$\begin{aligned} Loan\ Guarantees_{i,r,t} = \alpha + \beta SOE_{i,r} \times After\ Peer\ Punishment_{r,t} + \gamma_1 SOE_{i,r} \\ + \gamma_2 After\ Peer\ Punishment_{r,t} + X'\delta + \eta_i + \eta_t + \epsilon_{i,r,t}, \end{aligned} \tag{3}$$

where $Loan\ Guarantees_{i,k,r,t}$ is the amount of loan guarantees extended by firm i in region r in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,k,r}$ 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_t represent full sets of firm and year fixed effects, respectively. For the sake of statistical inference, we cluster standard errors at the level of the region (r) to allow for correlation of unknown form across the residuals of listed firms headquartered in the same region.

Firm fixed effects absorb time-invariant systematic differences in the extent of loan guarantees across firms, whereas year fixed effects absorb time-varying shocks to the extent of loan guarantees listed firms extend to their related parties, which affect all firms in the sample similarly in the same year. We also propose results for a specification that restricts the variation we exploit further, by allowing for systematic differences in time-varying shocks to loan guarantees across Chinese regions. These specifications thus replace the set of year fixed effects (η_t) with a full set of region×year fixed effects (η_{rt}). Note that this set of fixed effects absorbs the variation in the dummy variable $After\ Peer\ Punishment_{r,t}$ completely.

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

Note that our setting does not provide clearcut predictions for coefficients γ_1 and γ_2. The null hypothesis that $\gamma_1 = 0$ states that that, on average, considering both the pre- and post-peer-punishment periods, SOE peers 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 used to extend to related parties before the punishment of a peer firm.

Table 2 reports the results for estimating equation (3). 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 that the estimated coefficient $\hat{\beta}$ is negative, and we can reject the null 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 (3) we consider, including the most restrictive specifications that absorb all time-varying shocks to loan guarantees that affect firms in the same region (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 industry and year after the first listed firm is punished in their regions compared to before.

As far as the coefficients associated with the two dummies are concerned, we do not detect any systematic pattern in the estimations. We find that $\hat{\gamma_1}$ 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 regional peer, the loan guarantees non-SOE 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 by SOEs compared to non-SOEs after the peer's punishment ranges between 1.4 percentage points (column (2) of Table 2) to 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).

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 two different groups of firms. On the one hand, the control group includes listed firms in the years in which no punishment has occurred in the cities in which they operate, which allows us to exploit the staggered timing of the occurrence of punishments across China for identification. On the other hand, the control group also includes listed firms in cities that have never experienced any punishment throughout the sample period, which one might worry differ systematically from the cities that ever experience a punishment event in ways that might related with our outcomes. In Panel A of Table 3 of the Online Appendix, we show that the results are materially unaffected if we restrict the sample to firms in cities that experience at least one punishment during the sample period.

To address the concern that our results might be driven by the timing of punishment of a few large commercial cities, in Panel B of Table 3 we show that the results are similar if we exclude the most important Chinese commercial cities – that is, Beijing, Shanghai, and Shenzhen.

One might be concerned that the SOE status of the firms in our sample changes dramatically during the sample period, when the Chinese government proceeded to privatize several SOEs. We show that this concern is unlikely to be material for our results in Panel C of Table 3. The results do not change substantially if we do not allow the SOE status of a firm to change over time, but we fix it 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. We weigh observations based on the total number of firms in the location in which the observed firm is headquartered to assess whether our results are driven mainly by large urban conglomerations or by less concentrated areas. We find that not only our results stay statistically significant, but the size of the estimated effects is larger than in the baseline analysis of Table 2, which suggests that large urban conglomerates are important to drive the baseline results.

In Panel E of Table 3, we exclude all firm-year observations for firms that accessed a location only after the first punishment happened in the location, and we verify that this segment of firms is immaterial to explaining our baseline results.

Finally, in Panel F of Table 3 we propose a placebo test whereby we assign placebo first dates of punishment to Chinese regions randomly – in the sample of city*years, we use a random generator to match each city 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 region after the placebo date of first punishment of a local peer.

B Salience of Peer's Punishment and Size of the Effect

The mechanism that underlies our hypothesis suggests that the sobering effect of a peer's punishment should be stronger the more salient is the punishment to unpunished peers. Ideally, we would test whether the extent of the reaction of firms to local peers' punishments is higher when the punishment events are more salient, as opposed to when the punishment events are less salient.

We construct two proxies for the salience of punishment events. The first proxy exploits the media coverage of peers' punishment. We collect all pieces of news in national and local newspapers in the 30 days before and after the investigation window of the

punished peer from the *China Knowledge Resource Integrated Database*. For each piece of news, we categorize whether it discusses the punished firm or not by checking whether the title and/or the body of the news include the name of the punished firm. For each period around a punishment event, we then construct the ratio of the number of news that cover the punishment over the total number of news published over the period. The rationale for this measure is that the higher is the media coverage of a punishment event, the more salient the punishment is likely to be 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 at times in which other important events are covered by the media and distract peers, or in case the central government restricts access to information regarding the punished firms. In all these cases, the punishment event should be less salient to peers compared to cases in which the share of news covering the punishment event is high.

The second proxy for salience of the punishment events relies on the returns of punished firms around the announcement. Intuitively, events that cause a larger reaction of the punished firm's stock prices should be more salient than events that cause a smaller reaction, because the extent of fraud for which the firm is punished is large. This proxy is potentially correlated with the media-coverage proxy, but does not necessarily capture the same variation.

In the left panel of Table 4, we define as salient a punishment event that was in the top 10th percentile of the distribution of the ratio of news covering the punishment over the total number of news in China in the 30 days around the investigation window of the punished firm. In the right panel of Table 4, we define as salient a punishment event that causes a drop in the abnormal stock returns of the punished firm of more than 20% in the 30-day window around the announcement. We then use two separate dummies for the event date: $After\ Punishment--Salient$ equals 1 after a local firm is punished and the firms' abnormal returns dropped by more than 20% at announcement, and zero otherwise. $After\ Punishment--Non\ Salient$ captures the remaining peer punishment events.

Consistent with the salience interpretation, using any of our two proxies we find that the effect of peers' punishment on SOEs is substantially larger when the punishment event

was more salient, compared to when the punishment event was less salient. Note that non-SOEs do not react to any of the type of events, which is consistent with our interpretation of the baseline results – non-SOEs do not react because other governance mechanisms are in place and discipline their behavior even absent salience of peers' punishment.

(Untabulated) results 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 over the total number of news downloads to construct our measure of media coverage of the punishment event. This alternative proxy addresses the concern that peers might not read all the news that are published. Moreover, we find similar results if we change the threshold for the negative CARs of the punished firms or the extension of the event window when constructing our second measure of salience.

C Governance Outcomes: CEO Duality and Analyst Coverage

As discussed in Section IV, 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 that their companies do not engage in wrongdoing. We consider two such costly signals, which we can observe in our data, that is, the likelihood the firm eliminates CEO duality and the likelihood the firm becomes covered by at least one analyst.

We estimate the following specification using a linear probability model:

$$\begin{aligned} Governance\ Outcome_{i,r,t} = \alpha + \beta SOE_{i,r} \times After\ Peer\ Punishment_{r,t} + \gamma_1 SOE_{i,r} \\ + \gamma_2 After\ Peer\ Punishment_{r,t} + X'\delta + \eta_i + \eta_t + \epsilon_{i,k,r,t}, \end{aligned} \tag{4}$$

where $Governance\ Outcome_{i,r,t}$ is either a dummy variable that equals 1 if the firm displays CEO duality, or a dummy variable that equals 1 if at least one analyst covers the firm. All the other variables are defined as in equation (3). Based on Hypothesis 2, we expect that $\beta < 0$ when considering CEO duality – governance improves as the firm moves from CEO duality to a more independent board – and that $\beta > 0$ when considering whether at least one analyst covers the firm – public scrutiny on the firm is higher is the firm is covered by analysts.

Table 5 reports the results for estimating equation (4). In columns (1)-(3), the outcome is the dummy for CEO duality. As predicted in Hypothesis 2, we reject the null

that the coefficient $\beta = 0$ at conventional levels of statistical significance. We find that SOE firms are about 5.6 percentage points less likely to display CEO duality after the first firm is punished in their region compared to 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. Similar to our results for the extent of loan guarantees to related private parties, we fail to reject the null that either γ_1 or γ_2 are equal to 0 at any plausible level of significance in most specifications.

In columns (4)-(6) of Table 5, the outcome is the dummy for whether at least one analyst covers the firm. Even in this case, we can reject the null that $\beta = 0$, and consistent with our interpretation, we find that after the peer's punishment SOEs are more likely to become covered by analysts, compared to before the punishment and to non-SOEs. We can reject the null that this coefficient is zero at standard levels of significance when absorbing year and firm fixed effects. In the most restrictive specification that absorbs firm and region×year fixed effects, we do not reject the null of a zero coefficient below the 10% level of significance, although the size of the estimated coefficient is not negligible. The size of the effect ranges between about 12% and 38% of the average share of firms covered by at least one analyst.

D Real Outcomes: Investment and TFP

In the last part of our difference-in-differences analysis, we consider the effect of a peer's punishment on firm-level real outcomes. In particular, we estimate the following specification by ordinary least squares:

$$\begin{aligned} Real\ Outcome_{i,r,t} = \alpha + \beta SOE_{i,r} \times After\ Peer\ Punishment_{r,t} \\ + \gamma_1 SOE_{i,r} + \gamma_2 After\ Peer\ Punishment_{r,t} + X'\delta + \eta_i + \eta_t + \epsilon_{i,r,t}, \end{aligned} \tag{5}$$

where $Real\ Outcome_{i,r,t}$ is either the investment scaled by total assets or the total factor productivity(TFP) of firm i in region r in year t. All the other variables are defined as in equation (4).

Table 6 reports the results for estimating equation (5). In columns (1)-(3), the outcome variable is the growth of firm i's investment from year t-1 to year t, scaled by the

total amount of assets as of year t-1. We find that SOE firms decrease investment growth after the first peer is punished in their location, compared to before and to non-SOE firms. Moreover, non-SOE firms appear to not change their investment growth on average, as captured by the small size and change in sign of the estimated coefficient $\hat{\gamma_2}$. In terms of magnitude of the effect, the differential drop in investment growth for SOEs after the per punishment is about 1 percentage point, which corresponds to 17% of a standard deviation of investment in the running sample.

The drop in investment by SOEs might improve shareholder value by eliminating inefficient investment and wasteful management of resources, or might reduce shareholder value if the SOE's management had invested in positive net present value projects. As a rough proxy for the efficiency of the use of firm-level resources, we compute firms' TFP as discussed in Section III and use it as an alternative outcome when estimating equation (5). We cannot compute TFP for about 8% of our sample, which reduces the statistical power of tests in these specifications.

In columns (4)-(6) of Table 6, we find evidence that SOEs' TFP increased after the first peer punishment in their location, compared to before and to non-SOE firms in the same location. In the specification that includes firm and region×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%. We believe that the loss in statistical power due to the impossibility to compute TFP for a fraction of our sample might contribute to this non-result, and overall we believe that the evidence in columns (4)-(6) suggests that indeed SOEs' TFP increases on average after the first peer punishment compared to before and compared to non-SOEs. 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.

E Firm Value: Evidence from Event Studies

The evidence so far suggests that, after the first punishment for wrongdoing in inter-corporate loan guarantees in a region, the management of listed SOEs in the region reduce the amount of loan guarantees they extend to their related private parties; they improve the governance of the firm, e.g. they are more likely to eliminate CEO duality and obtain analyst coverage for the firm; and, they reduce investment and increase the

firm's TFP, which suggests that the investment the SOE had in place before the peer's punishment made an inefficient use of the firm's resources.

At the same time, the evidence so far is not enough to conclude that 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 shareholder 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 cumulative abnormal returns of listed SOEs and listed non-SOEs around the punishment dates. Figure 3 plots the average cumulative market-adjusted returns for the peer SOE and non-SOE listed firms around the date the first peer firm in each location was punished for wrongdoing in terms of loan guarantees to related parties.

Two patterns are worth of notice. First, we see that the cumulative abnormal returns of SOEs and non-SOEs follow trends that appear parallel at least up to the 5 days before the punishment of a peer firm in their location. These parallel trends in cumulative abnormal returns resemble the parallel trends of the outcome variables we consider in the regression analysis for the years before firms' punishment, which we documented in Section V. Cumulative abnormal returns appear to be flip sign and be statistically not different from zero for both groups of firms. We only detect slightly diverging trends in the 5 days before the announcement of peers' punishments. These pre-announcement diverging trends in the very few days before the event date might reflect information leakage about the upcoming punishment announcements.

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

Overall, the event study results suggest that the change in outcomes for SOE firms we discussed above are paralleled by significant and stable positive cumulative abnormal returns after the punishment, which corroborates the interpretation that the reaction of

SOE managers' to a peer's punishment produces actions that overall increase shareholder value and hence the value to minority shareholders.

VII Effects of Peers' Reaction on Related Parties

Our results so far do not rule out that the managers of SOE firms might engage in substitution across wrongdoing activities. 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 resources to related parties at the expenses of private shareholders.

Note that in the precious 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 cumulative abnormal returns of their stocks –increase, 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 private shareholders. Moreover, whereas our interpretation of the reaction to peer punishment is consistent with the fact that SOE firms react but non-SOE firms do not react, it is not clear why SOEs but not non-SOEs would move to more opaque forms of wrongdoing after a peer's punishment.

To assess the extent to which managers of SOEs might engage in more opaque and less detectable forms of wrongdoing in the form of transactions with related parties, in the last part of the empirical analysis we assess the effects of a peer's punishment on outcomes observed at the level of the *related parties* of listed SOE and non-SOE firms. If managers of SOEs were substituting loan guarantees with other forms of tunneling of resources to related parties, 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 effect of the listed SOEs' peer punishment on the listed SOEs' related parties would suggest that, even if the SOE management substituted loan guarantees with more opaque forms of transfers, on net related parties did suffer a cut in available resources after the punishment of the peer.

Our empirical strategy for related parties is similar to strategy we employed to assess the effects of peers' punishment on listed SOEs outcomes. In particular, we estimate the

following specification by ordinary least squares:

$$\begin{aligned} \textit{Related Party Outcome}_{i,r,t} = \alpha + \beta SOE_{i,r} \times \textit{After Peer Punishment}_{r,t} + \gamma_1 SOE_{i,r} \\ + \gamma_2 \textit{After Peer Punishment}_{r,t} + X'\delta + \eta_i + \eta_t + \epsilon_{i,r,t}, \end{aligned} \tag{6}$$

where $\textit{Related Party Outcome}_{i,r,t}$ 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. All the other variables are defined as in equation (3).

Table 7 reports the results for estimating equation (6). 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 that the borrowing of related parties based on guaranteed loans drops significantly after the SOEs' peers are punished, both statistically and economically, because the size of the marginal effect is about one half of the average total amount of guaranteed credit over total assets.

The decrease of 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 7, we consider the growth of related parties' investment from year t-1 to year t scaled by assets in year t-1 as the outcome variable, and we find that after the peers' punishment, related parties of SOEs decrease investment growth substantially. This result suggests that 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.

VIII Conclusions

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

We consider Chinese SOEs, which should be more prone to react to the

salience-of-punishment mechanism, because they are less constrained by traditional governance mechanisms that restrict the behavior of listed non-SOEs. We find that after a local peer headquartered in the same prefectures 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. Consistent with the salience-of-punishment channel, the effects are stronger for listed SOEs whose peers' punishment was more salient.

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 as 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? What are the psychological channels through which the salience-of-punishment mechanism operates, e.g. are 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 on these questions.

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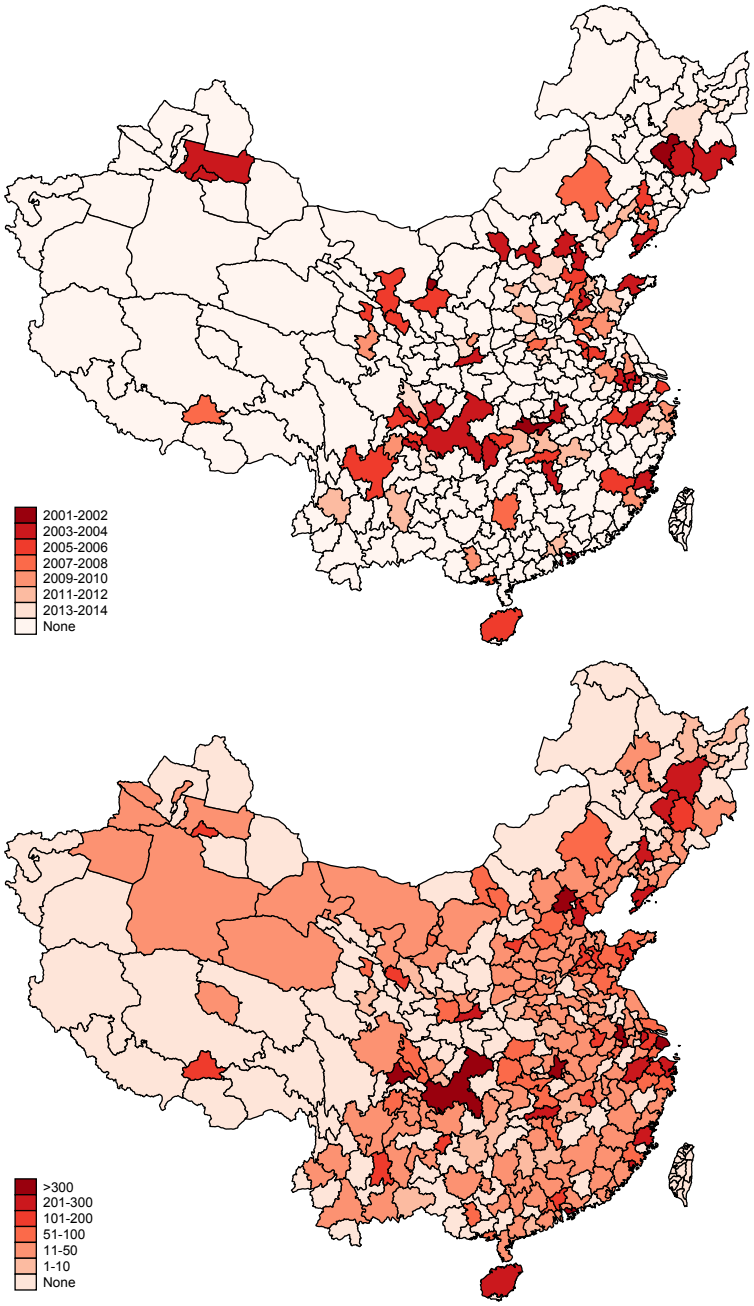
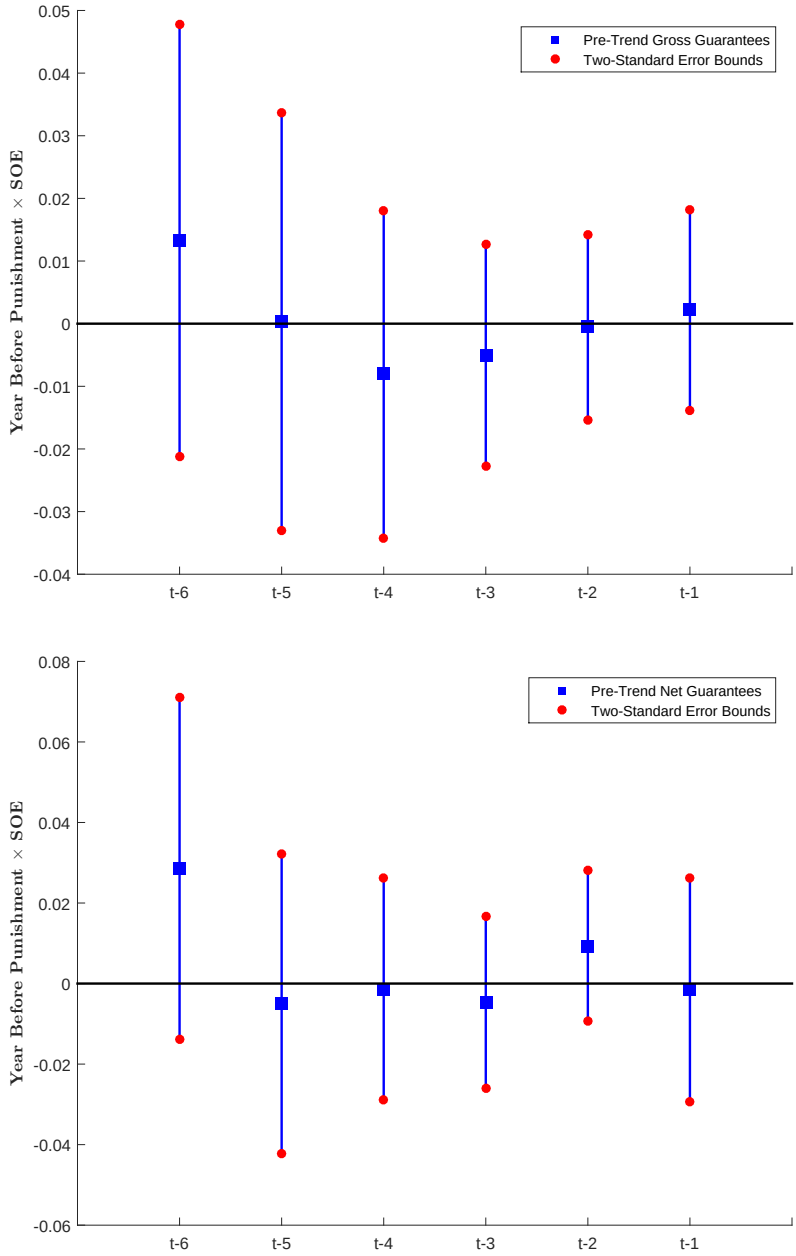
Figure 1: **Time of First Punishment and Number of Firms at the Local Level**



This figure plots the time of the first punishment in the top panel and the number of firms in the bottom panel at the province level. The sample period is 1997 to 2014.

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

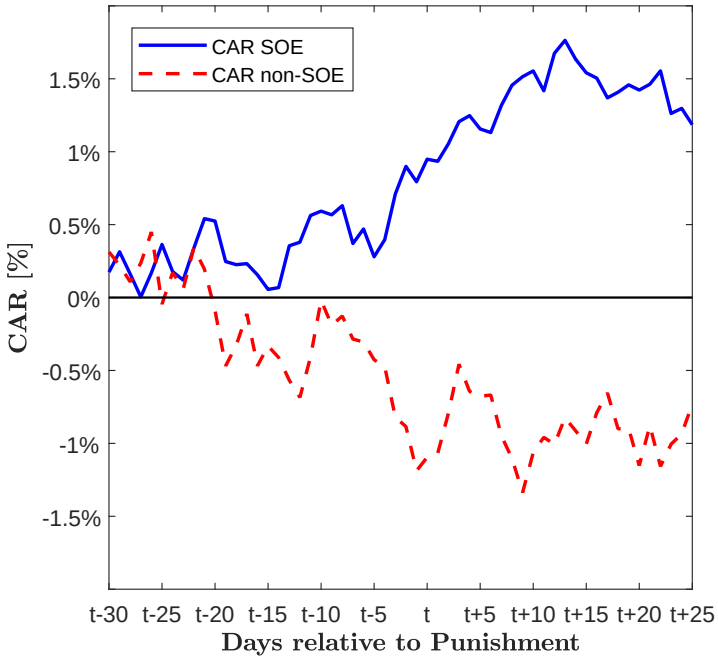


This figure plots the estimates of β_t from the following linear equation

$$Outcome_{i,r,t} = \alpha + \sum_t \beta_t SOE_{i,r} \times Year_t + \gamma_1 SOE_{i,r} + \gamma_2 \sum_t Year_t + X'\delta + \eta_i + \epsilon_{i,,r,t}$$

where $\sum_t \beta_t SOE_{i,r} \times Year_t$ 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 region r, 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 Punishment: SOEs vs. non-SOEs**



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

Table 1: **Descriptive Statistics**

This table reports 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 the our main sample of Chinese listed firms that are headquartered in a region in which at least one listed company was punished by the market authority 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 1-99 percent levels to ensure that outlier observations do not affect our results. 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 Provided Guarantees	14,068	0.04	0.10	0.00	0.00	0.13
Net Provided Guarantees	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	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
Leverage	13,933	0.06	0.09	0.00	0.02	0.18
Cash	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 / Lagged 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	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 Related Parties)	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	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: **Inter-corporate Loan Guarantees After Peer Punishment: SOEs vs. non-SOEs**

This table reports estimates of β from the following linear equation

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

where $Loan\ Guarantees_{i,k,r,t}$ is the amount of loan guarantees extended by firm i in region r in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,k,r}$ 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_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the region (r). 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	0.0199***	0.0036		0.0115	0.0078	
	(3.34)	(0.59)		(1.40)	(0.83)	
After Punishment × SOE	−0.0175***	−0.0141***	−0.0242***	−0.0223***	−0.0151**	−0.0243**
	(−3.34)	(−2.67)	(−2.87)	(−2.87)	(−2.00)	(−2.14)
SOE	−0.0079*	0.0107**	0.0169**	−0.0019	0.0090	0.0157**
	(−1.66)	(1.99)	(2.14)	(−0.34)	(1.53)	(2.10)
Total Assets	0.0341***	0.0144***	0.0123**	0.0278***	0.0163***	0.0167***
	(9.22)	(3.38)	(2.19)	(7.05)	(3.51)	(2.73)
Leverage	0.0029	0.0172	0.0264	−0.0601**	−0.0520*	−0.0402
	(0.16)	(0.97)	(1.17)	(−2.12)	(−1.82)	(−1.17)
Cash	−0.0237***	−0.0180*	−0.0104	−0.0061	0.0034	0.0235
	(−2.63)	(−1.85)	(−0.75)	(−0.42)	(0.23)	(1.26)
Tobin's Q	0.0028***	0.0000	0.0005	0.0054***	0.0013	0.0008
	(3.34)	(−0.01)	(0.32)	(4.37)	(0.90)	(0.44)
Constant	−0.6978***	−0.2238**	−0.2317*	−0.6169***	−0.3357***	−0.3646***
	(−8.70)	(−2.36)	(−1.92)	(−7.29)	(−3.26)	(−2.82)
Firm Fixed Effect	X	X	X	X	X	X
Year Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.296	0.317	0.323	0.287	0.297	0.324

t-statistics in parentheses

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

Table 3: **Inter-corporate Loan Guarantees After Peer Punishment: SOEs vs. non-SOEs – Robustness**

This table reports estimates of β from the following linear equation

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

where all variables are defined as in Table 2. We cluster standard errors at the level of the region (r). 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	−0.0175***	−0.0141***	−0.0242***	−0.0223***	−0.0151**	−0.0243**
	(−3.34)	(−2.67)	(−2.87)	(−2.87)	(−2.00)	(−2.14)
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.296	0.317	0.323	0.287	0.297	0.324
	Panel B. Excluding Beijing, Shanghai, Shenzhen					
After Punishment × SOE	−0.0197***	−0.0173***	−0.0290***	−0.0250***	−0.0165*	−0.0270**
	(−3.09)	(−2.73)	(−2.69)	(−2.74)	(−1.79)	(−1.99)
Observations	10,375	10,375	10,375	10,375	10,375	10,375
Adjusted R^2	0.298	0.318	0.314	0.287	0.299	0.324
	Panel C. Fixing SOE status at Peer Punishment					
After Punishment × SOE	−0.0189**	−0.0172**	−0.0167*	−0.0242**	−0.0193**	−0.0192*
	(−2.21)	(−2.06)	(−1.78)	(−2.55)	(−2.16)	(−1.80)
Observations	13,133	13,133	13,133	13,133	13,133	13,133
Adjusted R^2	0.280	0.306	0.297	0.277	0.289	0.304
	Panel D. Weighted Least Squares (w=N. local firms)					
After Punishment × SOE	−0.0175***	−0.0140***	−0.0219***	−0.0242**	−0.0150***	−0.0254**
	(−4.76)	(−3.84)	(−4.96)	(−4.43)	(−3.02)	(−3.65)
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.296	0.317	0.344	0.287	0.297	0.339
	Panel E. Full Set Interactions Controls					
After Punishment × SOE	−0.0201***	−0.0160***	−0.0236***	−0.0217***	−0.0152**	−0.0251**
	(−3.67)	(−2.93)	(−2.62)	(−4.43)	(−1.99)	(−2.17)
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.296	0.317	0.344	0.287	0.297	0.339
	Panel F. Placebo Test: Random Punishment Date					
After Punishment × SOE	−0.0105	−0.0105*	−0.0089	−0.0137	−0.0109	−0.0052
	(−1.64)	(−1.77)	(−0.85)	(−1.64)	(−1.32)	(−0.47)
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.294	0.317	0.321	0.286	0.297	0.323
Controls Table 2	X	X	X	X	X	X
Firm Fixed Effect	X	X	X	X	X	X
Year Fixed Effect		X			X	
City-Year Fixed Effect			X			X

Table 4: **Inter-corporate Loan Guarantees After Peer Punishment: The Role of Salience**

This table reports estimates of β from the following linear equation

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

where $Loan\ Guarantees_{i,k,r,t}$ is the amount of loan guarantees extended by firm i in region r in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,k,r}$ 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; in the left panel, $After\ Peer\ Punishment - -Salient_{r,t}$ 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 the ratio between the number of news that discuss the punishment over the total number of news in the 2 months before and after the investigation window is in the top 10% of the distribution; in the right panel, $After\ Peer\ Punishment - -Salient_{r,t}$ 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 the CARs of the punished firm dropped by more than 20% in the 30 days around the punishment announcement; 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_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the region (r). 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.

	Top 10 – Punishment News/Total News			CARs punished ≤-20%		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment–Salient	0.0202	0.0186		0.0293	0.0299*	
	(1.13)	(1.16)		(1.57)	(1.66)	
After Punishment–Non Salient	0.0096	0.0055		0.0115	0.0078	
	(1.13)	(0.55)		(1.40)	(0.83)	
After Punishment–Salient	−0.0380***	−0.0280**	−0.0415**	−0.0411***	−0.0324**	−0.0409**
× SOE	(−2.77)	(−2.07)	(−2.36)	(−3.12)	(−2.51)	(−2.95)
After Punishment–Non Salient	−0.0184**	−0.0120	−0.0201*	−0.0147*	−0.0083	−0.0173
× SOE	(−2.29)	(−1.51)	(−1.68)	(−1.87)	(−1.06)	(−1.36)
SOE	−0.0020	0.0089	0.0158**	−0.0013	0.0096	0.0168**
	(−0.35)	(1.52)	(2.12)	(−0.22)	(1.61)	(2.75)
Total Assets	0.0278***	0.0163***	0.0167***	0.0279***	0.0166***	0.0169**
	(7.01)	(3.49)	(2.74)	(7.01)	(3.53)	(2.75)
Leverage	−0.0608**	−0.0528*	−0.0408	−0.0598**	−0.0517*	−0.0400
	(−2.15)	(−1.86)	(−1.19)	(−2.09)	(−1.79)	(−1.16)
Cash	−0.0055	0.0039	0.0237	−0.0058	0.0038	0.0235
	(−0.38)	(0.27)	(1.26)	(−0.40)	(0.26)	(1.25)
Tobin's Q	0.0054***	0.0013	0.0008	0.0054***	0.0013	0.0008
	(4.38)	(0.90)	(0.45)	(4.43)	(0.90)	(0.49)
Constant	−0.6158***	−0.3355***	−0.3644***	−0.6198***	−0.3418***	−0.3679**
	(−7.26)	(−3.24)	(−2.82)	(−7.27)	(−3.29)	(−2.83)
Firm Fixed Effect	X	X	X	X	X	X
Year Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	12,969	12,969	12,969	12,969	12,969	12,969
Adjusted R^2	0.297	0.318	0.323	0.288	0.298	0.325

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

Table 5: **Governance After Peer Punishment: SOEs vs. non-SOEs**

This table reports estimates of β from the following linear equation

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

where $Governance\ Outcome_{i,r,t}$ is either a dummy variable that equals 1 if firm i in region r in year t displays CEO duality, or a dummy variable that equals 1 if at least one analyst covers the firm; $SOE_{i,k,r}$ 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_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the region (r). Columns (1)-(3) report results for CEO duality, whereas columns (4)-(6) report results for analyst coverage. The sample period is 1997 to 2014.

	CEO Duality			Analyst Coverage		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0360**	0.0285		0.0754**	−0.0550	
	(2.14)	(1.54)		(1.97)	(−1.55)	
After Punishment × SOE	−0.0404**	−0.0253	−0.0557**	0.1079***	0.0427*	0.0351
	(−2.40)	(−1.52)	(−2.01)	(4.45)	(1.93)	(1.30)
SOE	0.0128	0.0288*	0.0495*	−0.1084***	−0.0322	−0.0212
	(0.86)	(1.80)	(1.94)	(−5.15)	(−1.53)	(−0.74)
Total Assets	−0.0118	−0.0307**	−0.0363**	0.1406***	0.0806***	0.0787***
	(−1.36)	(−2.31)	(−2.04)	(13.68)	(8.36)	(6.25)
Leverage	−0.0593	−0.0265	0.0104	0.1297	0.1640**	0.1208
	(−1.03)	(−0.45)	(0.13)	(1.53)	(2.22)	(1.42)
Cash	−0.0455	−0.0165	0.0154	0.0761	0.0995*	0.0941
	(−0.83)	(−0.32)	(0.22)	(1.38)	(1.89)	(1.36)
Tobin's Q	0.0049	0.0008	−0.0016	0.0031	0.0241***	
	(1.31)	(0.18)	(−0.32)	(0.87)	(5.54)	(3.65)
Constant	0.3691*	0.8506***	0.9569**	−2.7650***	−1.4358***	−1.5504***
	(1.95)	(2.79)	(2.42)	(−12.07)	(−6.55)	(−5.74)
Firm Fixed Effect	X	X	X	X	X	X
Year Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	12,622	12,622	12,622	12,969	12,969	12,969
Adjusted R^2	0.367	0.374	0.384	0.494	0.551	0.564

t-statistics in parentheses

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

Table 6: **Investment and Tfp After Peer Punishment: SOEs vs. non-SOEs**

This table reports estimates of β from the following linear equation

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

where $Real\ Outcome_{i,r,t}$ is either the investment scaled by total assets or the total factor productivity(TFP) of firm i in region r in year t; $SOE_{i,k,r}$ 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_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the region (r). 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	−0.0005	0.0031		0.1272**	−0.0485	
	(−0.15)	(0.87)		(2.25)	(−0.81)	
After Punishment × SOE	−0.0102***	−0.0084**	−0.0110**	0.2053***	0.1175*	0.1539
	(−3.07)	(−2.51)	(−2.13)	(3.40)	(1.94)	(1.54)
SOE	0.0093***	0.0085***	0.0096*	−0.0795*	0.0140	−0.0009
	(3.11)	(2.66)	(1.93)	(−1.78)	(0.32)	(−0.01)
Total Assets	−0.0052***	−0.0028	−0.0028	0.9096***	0.8328***	0.8369***
	(−3.37)	(−1.42)	(−1.21)	(34.95)	(25.11)	(21.53)
Leverage	0.0491***	0.0486***	0.0583***	0.1550	0.2272	0.1946
	(3.66)	(3.70)	(3.67)	(0.72)	(1.09)	(0.68)
Cash	0.0461***	0.0374***	0.0317**	0.5833***	0.6492***	0.5256***
	(4.17)	(3.33)	(2.29)	(4.87)	(5.71)	(3.44)
Tobin's Q	−0.0002	0.0015*	0.0008	0.0982***	0.1389***	0.1510***
	(−0.27)	(1.73)	(0.65)	(10.94)	(12.36)	(11.38)
Constant	0.1102***	0.0547	0.0587	−14.7068***	−12.9860***	−13.1787***
	(3.30)	(1.26)	(1.12)	(−26.29)	(−17.40)	(−16.09)
Firm Fixed Effect	X	X	X	X	X	X
Year Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	9,153	9,153	9,153	11,643	11,643	11,643
Adjusted R^2	0.068	0.081	0.156	0.885	0.889	0.894

t-statistics in parentheses

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

Table 7: **Related Party Borrowing and Investment After Peer Punishment: SOEs vs. non-SOEs**

This table reports estimates of β from the following linear equation

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

where Related Party Outcome$_{i,r,t}$ 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 region r in year t; 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_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the region (r). 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	0.0022		0.0255	0.0335	0.0485
	(0.60)	(0.26)		(1.23)	(1.57)	(1.22)
After Punishment × SOE	−0.0472***	−0.0440**	−0.0673**	−0.0240*	−0.0233*	−0.0278
	(−2.63)	(−2.50)	(−2.16)	(−1.75)	(−1.72)	(−1.20)
SOE	0.0304*	0.0390**	0.0576*	0.0185	0.0217*	0.0236
	(1.88)	(2.33)	(1.97)	(1.54)	(1.81)	(1.09)
Total Assets	−0.0142***	−0.0184***	−0.0205*	−0.0060	−0.0045	0.0027
	(−3.72)	(−4.33)	(−1.72)	(−0.83)	(−0.48)	(0.22)
Leverage	0.1159**	0.1295***	0.0201	0.0703	0.0351	0.0084
	(2.29)	(2.61)	(0.16)	(0.88)	(0.44)	(0.07)
Cash	−0.0156	−0.0066	−0.0137	0.1424**	0.1172**	0.0887
	(−0.35)	(−0.15)	(−0.11)	(2.30)	(1.99)	(1.22)
Tobin's Q	0.0101**	0.0114**	0.0097	0.0139***	0.0118**	0.0136**
	(2.49)	(2.09)	(1.25)	(3.35)	(2.52)	(2.21)
Constant	0.3664***	0.4190***	0.5150**	0.1362	0.0944	−0.0547
	(4.10)	(4.39)	(2.02)	(0.82)	(0.43)	(−0.20)
Firm Fixed Effect	X	X	X	X	X	X
Year Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	2,509	2,509	2,509	10,645	10,645	10,645
Adjusted R^2	0.025	0.048	0.572	0.018	0.027	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

Table A.1: **Inter-corporate Loan Guarantees After Peer Punishment: SOEs vs. non-SOEs – excl. Beijing, Shanghai, and Shenzhen**

This table reports estimates of β from the following linear equation

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

where $Loan\ Guarantees_{i,k,r,t}$ is the amount of loan guarantees extended by firm i in region r in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,k,r}$ 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_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the region (r). Columns (1)-(3) report results for the overall amount of loan guarantees, whereas columns (4)-(6) report results for net guarantees. We exclude Beijing, Shanghai, and Shenzhen from the analysis. The sample period is 1997 to 2014.

	Total Provided Guarantees			Net Provided Guarantees		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0226***	0.0075		0.0135	0.0109	
	(2.96)	(1.05)		(1.40)	(1.19)	
After Punishment × SOE	−0.0197***	−0.0173***	−0.0290***	−0.0250***	−0.0165*	−0.0270**
	(−3.09)	(−2.73)	(−2.69)	(−2.74)	(−1.79)	(−1.99)
SOE	−0.0075	0.0118*	0.0183**	−0.0003	0.0109	0.0193**
	(−1.45)	(1.94)	(2.00)	(−0.04)	(1.63)	(2.21)
Total Assets	0.0378***	0.0180***	0.0173***	0.0302***	0.0187***	0.0212***
	(10.52)	(4.14)	(2.82)	(6.98)	(3.48)	(2.91)
Leverage	−0.0079	0.0072	0.0140	−0.0644**	−0.0554*	−0.0419
	(−0.40)	(0.37)	(0.52)	(−2.00)	(−1.70)	(−0.98)
Cash	−0.0223*	−0.0152	−0.0064	−0.0097	0.0013	0.0265
	(−1.95)	(−1.22)	(−0.33)	(−0.54)	(0.07)	(1.00)
Tobin's Q	0.0029***	−0.0002	0.0007	0.0058***	0.0011	0.0004
	(2.75)	(−0.14)	(0.36)	(4.11)	(0.59)	(0.17)
Constant	−0.7745***	−0.3017***	−0.3384**	−0.6681***	−0.3868***	−0.4766***
	(−9.94)	(−3.12)	(−2.50)	(−7.25)	(−3.26)	(−2.99)
Firm Fixed Effect	X	X	X	X	X	
Year Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	10,375	10,375	10,375	10,375	10,375	10,375
Adjusted R^2	0.298	0.318	0.314	0.287	0.299	0.324

t-statistics in parentheses

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

Table A.2: **Inter-corporate Loan Guarantees After Peer Punishment: SOEs vs. non-SOEs – Only Regions with Punishments**

This table reports estimates of β from the following linear equation

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

where $Loan\ Guarantees_{i,k,r,t}$ is the amount of loan guarantees extended by firm i in region r in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,k,r}$ 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_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the region (r). Columns (1)-(3) report results for the overall amount of loan guarantees, whereas columns (4)-(6) report results for net guarantees. We only keep cities which ever experienced a punishment. The sample period is 1997 to 2014.

	Total Provided Guarantees			Net Provided Guarantees		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0308***	0.0096		0.0183*	0.0106	
	(4.81)	(1.43)		(1.98)	(0.97)	
After Punishment × SOE	−0.0271***	−0.0207***	−0.0214**	−0.0318***	−0.0230**	−0.0232*
	(−4.48)	(−3.25)	(−2.62)	(−3.34)	(−2.43)	(−1.96)
SOE	0.0014	0.0144**	0.0134*	0.0093	0.0174**	0.0147*
	(0.25)	(2.26)	(1.75)	(1.19)	(2.20)	(1.71)
Total Assets	0.0293***	0.0124**	0.0126**	0.0275***	0.0165***	0.0164**
	(6.44)	(2.40)	(2.11)	(6.11)	(3.18)	(2.61)
Leverage	0.0092	0.0174	0.0157	−0.0561*	−0.0558*	−0.0587*
	(0.44)	(0.89)	(0.72)	(−1.71)	(−1.71)	(−1.73)
Cash	−0.0227**	−0.0194*	−0.0203	−0.0124	−0.0043	0.0052
	(−2.57)	(−1.94)	(−1.49)	(−0.82)	(−0.28)	(0.29)
Tobin's Q	0.0032***	0.0004	0.0006	0.0051***	0.0012	0.0011
	(3.13)	(0.28)	(0.36)	(3.62)	(0.75)	(0.63)
Constant	−0.6062***	−0.1866	−0.2302*	−0.6126***	−0.3357***	−0.3405**
	(−6.17)	(−1.62)	(−1.82)	(−6.39)	(−2.93)	(−2.60)
Firm Fixed Effect	X	X	X	X	X	X
Year Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	9,418	9,418	9,418	9,418	9,418	9,418
Adjusted R^2	0.292	0.310	0.320	0.275	0.284	0.305

t-statistics in parentheses

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

Table A.3: **Inter-corporate Loan Guarantees After Peer Punishment: SOEs vs. non-SOEs – Constant Firm Status**

This table reports estimates of β from the following linear equation

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

where $Loan\ Guarantees_{i,k,r,t}$ is the amount of loan guarantees extended by firm i in region r in year t to any private parent or subsidiary scaled by the previous end-of-the-fiscal-year assets; $SOE_{i,k,r}$ 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_t represent full sets of firm and year fixed effects, respectively. We cluster standard errors at the level of the region (r). Columns (1)-(3) report results for the overall amount of loan guarantees, whereas columns (4)-(6) report results for net guarantees. We fix the firm status at the time of the first punishment of a peer firm. The sample period is 1997 to 2014.

	Total Provided Guarantees			Net Provided Guarantees		
	(1)	(2)	(3)	(4)	(5)	(6)
After Punishment	0.0267***	0.0075		0.0168*	0.0115	
	(3.46)	(0.97)		(1.73)	(1.11)	
After Punishment × SOE	−0.0189**	−0.0172**	−0.0167*	−0.0242**	−0.0193**	−0.0192*
	(−2.21)	(−2.06)	(−1.78)	(−2.55)	(−2.16)	(−1.80)
Total Assets	0.0351***	0.0124***	0.0108*	0.0284***	0.0151***	0.0154**
	(10.11)	(2.97)	(1.93)	(7.58)	(3.31)	(2.59)
Leverage	−0.0014	0.0170	0.0178	−0.0584**	−0.0482*	−0.0408
	(−0.08)	(0.96)	(0.77)	(−2.06)	(−1.66)	(−1.15)
Cash	−0.0253***	−0.0191*	−0.0105	−0.0109	−0.0016	0.0216
	(−2.72)	(−1.90)	(−0.74)	(−0.74)	(−0.11)	(1.11)
Tobin's Q	0.0026***	−0.0007	−0.0003	0.0052***	0.0010	0.0001
	(3.43)	(−0.65)	(−0.18)	(4.44)	(0.71)	(0.06)
Constant	−0.7226***	−0.1776*	−0.2103*	−0.6273***	−0.3068***	−0.3537***
	(−9.77)	(−1.90)	(−1.67)	(−7.86)	(−3.02)	(−2.64)
Firm Fixed Effect	X	X	X	X	X	X
Year Fixed Effect		X			X	
City-Year Fixed Effect			X			X
Observations	9,418	9,418	9,418	9,418	9,418	9,418
Adjusted R^2	0.292	0.310	0.320	0.275	0.284	0.305
Observations	13,133	13,133	13,133	13,133	13,133	13,133
Adjusted R^2	0.280	0.306	0.297	0.277	0.289	0.304

t-statistics in parentheses

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