The mortality cost of political connections

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Abstract

We study the relationship between the political connections of Chinese firms and workplace fatalities. In our preferred specification we find that the worker death rate for connected companies is two to three times that of unconnected firms (depending on the sample employed), a pattern that holds for within-firm estimations. The connections-mortality relationship is attenuated in provinces where safety regulators' promotion is contingent on meeting safety targets. Further analyses indicate that connections enable firms to avoid (potentially costly) compliance measures, rather than using connections to avoid regulatory response after accidents occur. In the absence of fatalities, connected firms receive fewer reports of major violations for safety or environmental compliance, whereas in years of fatal accidents the rate of reported violations is identical. Moreover, fatal accidents produce negative returns at connected companies and are associated with the subsequent departure of well-connected executives, overall indicating little protection from regulatory backlash. Our findings emphasize the social costs of political connections, and suggest that appropriate regulatory incentives may be useful in mitigating these costs.

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There exists a longstanding debate on the social impact of rent-seeking by politically connected firms. The overall effect hinges to a large extent on whether political ties allow businesses to circumvent needlessly onerous bureaucratic constraints or to get around regulations that are socially beneficial but result in lower firm profits. Yet there is little empirical evidence to inform our views on this tradeoff. In this paper, we examine the link between firms' political ties and workplace fatalities in China, thus highlighting the cost side of this tradeoff; we also examine the effect of regulator incentives in mitigating these costs.

There have been many anecdotal accounts of substandard working conditions across a range of industries in China. As some indication of the scale of the problem, China's worker death rate is over 20 times that of the United Kingdom (Wang, 2011). This poor safety record is generally attributed to weak enforcement rather than the absence of adequate laws and regulations (Phillips, 2010). For example, a scathing assessment by the International Labour Organization in 2012 stated that firms in China's construction industry "do not meet international standards, national laws and regulations due to a lack of prevention, illegal operations, poor preventive and protective measures, lack of provision of personal protection equipment and effective and regular safety inspections, audits and training."[1] Bribery is often implicated in cases of poor safety compliance: following an explosion at a Nanjing chemical factory that killed 12 workers, the *Wall Street Journal* suggested that "corruption and collusion between...factory owners and local inspectors limit the likelihood that laws protecting workers will be enforced." This view is echoed by statements from the national ministry charged with production safety, whose spokesman stated as recently as 2013 that "protection from local government and corruption...are behind many major deadly accidents." Others have taken the contrary view - politicians plausibly put more weight on worker welfare relative to profits than businessmen, so that political ties could lead to *better* treatment of workers at politically connected firms (see Dong et.al. (2014) for recent evidence from China).

We use data from publicly traded Chinese firms during 2008 - 2013 to examine the empirical relationship between firms' political connections and workplace safety. We begin by documenting a robust positive correlation between the political ties of executives and worker deaths. We define a company to be politically connected if its CEO or another "C-suite" executive previously held a high-level government post; the number of of workplace deaths per employee at connected firms is two to three times higher than at unconnected firms (depending on the sample we employ). This pattern holds in regression specifications, both in the cross-section and also in a firm-year panel with firm fixed-effects, where the connection-fatality relationship is identified from executive turnover. We consider several alternative hypotheses that could account for this pattern, such as differential rates of death underre-

[1] See http://www.ilo.org/dyn/normlex/en/f?p=NORMLEXPUB:13100:0::NO::P13100_COMMENT_ID:3085598, last downloaded September 5, 2014.

porting for connected versus unconnected firms or differences in management talent, and also whether the selection of connected executives could bias our results.

Consistent with our thesis that connections insulate firms from regulatory compliance, we find that despite their relatively poor safety records there has never been a public report of a major workplace safety audit at a connected company (the rate is just under 5 percent at comparable unconnected firms). We also show that connected firms are fined for pollution less often, reinforcing the broader view that connections help firms circumvent costly regulations.

Such insulation from oversight may account for the relatively high profitability that we document among connected firms in our sample. We delve more deeply into the link between connections, lax safety compliance, and profitability by examining the effect of fatal accidents on future profits. We find that the share prices of connected firms fall by as much as 9 percent (relative to unconnected firms) in the 30 trading days following a fatal accident; in the year following such accidents, the profit premium of connected firms disappears, indicating that investors' expectations are validated by subsequent accounting profits. Further, we show that in the year following worker fatalities, firms are significantly less likely to employ connected executives, indicating one direct channel through which fatal accidents reduce or eliminate connected firms' profit premium.

Overall, we argue that our full set of results are best explained by the use of political relationships to (profitably) circumvent safety oversight and regulation, and that this protection is lost in the wake of accidents that result in worker deaths.

Finally, we explore the effect of strengthened incentives for regulators on worker safety at connected firms by examining the passage of laws which made officials' promotion contingent on meeting safety targets. Exploiting the different timing of passage of so-called "No safety, no promotion" (NSNP) laws across provinces, we show that stronger regulator incentives are associated with lower worker death rates, primarily among connected companies. This relationship holds when we include firm fixed effects, province-year fixed effects, and province-specific time trends to control for unobserved trends or shocks to firms or provinces. We also find a drop in pollution fines for connected firms following the passage of NSNP laws, consistent with substitution amongst forms of rent-seeking.

We present a Becker-style audit model that is useful for organizing and interpreting our full set of results. In our model, regulators trade off the costs of safety monitoring against the career costs of workplace fatalities, while firms weigh the cost of safety compliance versus the fines (and future oversight) that come with accidents and/or the detection of workplace safety violations. We add the following assumptions to the standard model: the cost to the regulator of monitoring connected firms is higher, and that connected firms lose their ability to shirk on safety compliance after fatalities occur. Our simple model yields a set of predictions that are useful in organizing our empirical results: (a) well-connected firms have more accidents; (b) in the absence of workplace fatalities, connected firms have fewer audits; (c) connected firms are more profitable; (d) the effect of raising the regulator's (career) cost from workplace fatalities may have a much greater impact on safety compliance at well-connected firms (owing to a 'bang-bang' shift in enforcement equilibrium); (e) expected profits at connected firms decline following accidents.

Our work contributes most directly to the literature on the consequences of government regulation. Many of the empirical contributions in this area have emphasized the negative impact of onerous regulations on economic growth (see, for example, Djankov et al (2002) on entry regulation, and Bourlès et al (2010) for product market regulation). An exception is Bertrand et al (2007), which finds that driver's license applicants in India obtain their licenses more quickly when given a financial incentive to do so (and hence are likely to get licenses through bribery). Furthermore, drivers that likely obtained licenses through bribery subsequently performed poorly on driving tests. By focusing on worker deaths, we look more directly at the welfare consequences of regulation; further, we focus on high-level connections rather than petty corruption.

Our study also contributes to the literature on the social costs and benefits of political rent-seeking by firms, and corruption more broadly. The theoretical foundations of this field were laid by Leff (1964), Becker and Stigler (1974), and Shleifer and Vishny (1993), which consider the circumstances where bribery and corruption will be most costly (Olken and Pande (2012) provide a recent survey). Mauro's (1995) cross-country analysis of corruption and growth was the first of many studies over the past couple of decades to explore the correlates of corruption empirically (see Fisman and Svensson (2007) and Bai et al (2013) for within-country analyses). This literature has often used the political connections of businesses - the measure we employ in this paper - as a marker for favorable treatment by government officials.[2]

While prior research has established the implications of political ties for firm profits, our study is one of a small number that illustrates the social costs. Earlier research on the social costs has generally emphasized the welfare consequences of directing government resources to politically favored firms, either through under-production or the efficiency loss of channeling business to less productive enterprises (see in particular Cingano and Pinotti (2013) and Amore and Bennedsen (2013) for two recent examples). Ours is the first to focus on a particularly tangible form of social cost - worker deaths - and to emphasize the costs of regulatory non-compliance rather than resource misallocation. While we focus on

[2]For examples of event study analyses on the value of personal connections, see Fisman (2001), Faccio (2006), and Rocholl et al (2009); for event study evidence from China, see Fan et al (2008). Khwaja and Mian (2005) study the value of connections in Pakistan with a focus on credit access, while both Olken (2007) and Rocholl et al (forthcoming) emphasize the procurement channel. Borisov et al (2013) provides evidence on lobbying expenditures as an alternative to personal ties as a source of political connections; Dinc (2005) emphasizes the critical role of politicians' incentives in governing the behavior of connected firms.

worker deaths as one particularly evocative example of social cost, it is likely representative of connected firms' ability to evade regulation - at society's expense - in other realms. In China, the head of the Food and Drug Administration was executed in 2007 for certifying that products were safe in exchange for bribes, while bribery was also implicated in the infant formula scandal the following year (Huang, 2012). Collusion between firms and the local bureaucracy is similarly blamed for the country's pollution problems, allowing businesses to evade emissions regulations across a range of industries (Morgan, 2013).

Our paper relates to the concurrent work of Jia and Nie (2013), who investigate the impact of government decentralization on worker deaths in Chinese coal mines. We view our study as complementary to theirs: whereas Jia and Nie focus on shifts in political structure and the resultant effect on collusion between state-owned enterprises and local governments, we study the firm-level impact of political connections of private businesses (indeed, we find that our results are unaffected by controlling for state ownership of the enterprises in our sample, highlighting the fact that the two papers focus on independent channels). We also emphasize that our work exploits firm-level data, and examines a wider array of industries.

Finally, our work also contributes to the related field that examines the propensity of companies to act in a socially responsible manner and the financial implications of doing so. Much of this research has focused on the abnormal returns earned by firms with high or low measures of social responsibility (see Elfenbein, Margolis and Walsh (2007) for a survey, Harrison and Kacperczyk (2009) for a recent analysis of social responsibility and long-run returns, and Hamilton (1995) for an event study of environmental responsibility and market returns). A separate stream of research focuses on social responsibility as an outcome variable. Papers in this area have examined correlates of corporate goodness such as financing constraints (Hong et.al. (2011)), product market competition (Baron, 2001), and information asymmetries (Elfenbein et al, 2012). In this paper, we relate political connections both to the level of company responsibility in the form of worker safety, and also to financial performance following worker deaths.

The rest of this paper is organized as follows: Section 1 describes the institutional background and the data, including a discussion of workplace safety oversight in China and also discussion of the channels through which political ties are thought to influence safety practices. Section 2 provides a simple theoretical framework for organizing our subsequent empirical results; Section 3 presents the main results and robustness checks, and Section 4 concludes.

1 Data and institutional background

1.1 Workplace safety oversight in China

Chinese worker safety is overseen by a patchwork of governmental authorities and regulations.[3] The earliest significant nationwide rules governing worker safety focused on mining, with the enactment of the Law of the People's Republic of China on Safety in Mines by the National People's Congress in 1993. As with much legislation in China, the Mining Safety Law explicitly recognized that worker welfare had to be balanced with the needs of economic development: "[The law] is formulated for the purpose of ensuring safety in production in mines, preventing accidents and protecting personal safety of workers and staff at mines and promoting the development of the mining industry." However, the details of how such high-level directives were to be put into practice were left to lower-level administrative bodies.

Subsequent national regulation, enacted in 1996, targeted coal mining in particular and specified conditions for implementation - such as the training of foremen in effective safety practices - which had to be satisfied for a mine to obtain an operating license. In 2002, nationwide rules that covered all industries were put into effect. This came with the Law of the People's Republic of China on Work Safety, which codified worker safety as a set of statutory rights. The law was comprised of 97 provisions covering - among other things - safety measurement, supervision and management of production safety, rescue work and investigations following accidents, and also laid out the resultant legal liabilities. This law was supplemented by the Administrative Penalties for the Violation of Production Safety Laws, passed in 2003 and revised in 2007. It specified the administrative punishments for safety violations, including modest fines (up to 100,000 RMB for an individual and 300,000 RMB for a firm; throughout our sample, the renminbi traded in the range of 6.2 to 7.2 RMB to a US dollar), and, more substantively, license suspensions and mine closures.

Since 2001, the enforcement of workplace safety regulation has been overseen by the State Administration of Work Safety (SAWS). This was intended to decouple safety oversight from industrial and mining production ministries. Previously, for example, the Ministry of Coal Industry had the potentially conflicting responsibilities of increasing output as well as maintaining safety standards. Since 2005, SAWS has had full ministry status, reporting directly to the State Council, the highest political body in China. In addition to sanctions for non-compliant firms, the central government has also mandated since 2006 (2004 for coal) that firms put aside annual reserves to cover safety costs. For the industries we study, these funds amount to a substantial fraction of firm revenues. For construction, the requisite safety fund is 2 percent of costs. In other industries, mandated safety funds are a non-linear function

[3] See Homer (2009) for a detailed history of safety regulation in China focused on - but not limited to - the mining industry.

of revenues or output. For chemical firms, which make up the largest industry in our sample, safety funds averaged about 0.7 percent of revenues, while for coal mining the average was just over 1 percent of revenues.

Despite the many regulations on the books and the significant power vested in SAWS, China's workplace safety record remains poor by international standards due to ineffective enforcement. According to SAWS, there were more than 83,000 deaths in workplace accidents in 2009, or roughly one out of every 10,000 workers (China Safe Production Yearbook, 2009). One reason is that, despite the increased ability to set regulations that came with the 2002 Work Safety Law, enforcement is largely left to local safety boards. These may lack independence from local officials, who are evaluated and rewarded on their ability to attract investment and maintain economic growth.[4] The effectiveness of safety compliance may be further weakened by corruption at the local level, as suggested by Wang (2006), Johnson (2006), Epstein (2009) and Sebag-Montefiore Clarissa (2012) among others. In this spirit, we assess the extent to which political ties may have attenuated safety compliance.

1.2 No safety, no promotion laws

To take advantage of local differences in the extent of safety enforcement, we analyze the effects of one significant shift in the incentives of local regulatory authorities. Prior to 2004, provincial officials were evaluated and promoted based on economic growth (Li and Zhou, 2005). Since 2004, China has followed a safety production targeting system, whereby the central government assigns a 'death ceiling' to each province, and the provincial government is responsible for allocating the 'quota' among municipalities (Peng, 2004). In response, provinces began adopting "No safety, no promotion" (NSNP) policies that made promotion of safety regulators and other local government officials contingent on meeting the safety target set for their region by the provincial government. Guodong was the first province to adopt this policy in 2005, while others followed suit only much later. As of mid-2014, 11 provincial authorities, out of a total of 31, still had not adopted this approach to providing incentives to safety regulators.[5] In Appendix Table A1, we list for each province the dates of the passage and implementation of NSNP policies. We will exploit this difference across provinces in officials' safety incentives to assess whether well-connected firms are less likely to have worker fatalities in locations where local government officials are evaluated in part based on worker safety. Further, as Table A1 makes clear, a number of laws were passed during our sample

[4] See Homer (2009), Li and Zhou (2005), Wang (2006), People's Daily (2005), and editorial comments in the Journal of New Safety (2005) for further details on the administration and enforcement of safety codes.

[5] Note that some provinces had enacted earlier policies on production safety. However, no punishments were specified for officials that failed to meet safety quotas. For example, Qinghai Province passed "Rules on monitoring production safety in Qinghai Province" on Aug 27, 2005 (effective Nov 1, 2005), but without specifying punishment for failing to meet quotas. We view provinces under such regimes as equivalent to those without safety interventions.

period of 2008-2013, making it possible to use within-firm changes in *NSNP* laws to identify the effect of the regulatory environment. Table A1 additionally provides, for each province, the number of firm-year observations and number of worker deaths, along with population and income per capita for 2005.

Passage of NSNP laws is, of course, non-random. In unreported analyses, we find that the fraction of provincial GDP from coal mining is a strong positive predictor of NSNP laws, but after this is accounted for, no other province-level attribute (including construction output, log income per capita, population, worker deaths per capita, or corruption cases per capita) is correlated with the passage of such laws. When we control for provincial coal output in our analysis below, our results are unaffected. Also, the fact that we will exploit the timing of passage of NSNP laws to identify their effect further lessens the concern of non-random assignment of regulator incentives.

1.3 Safety compliance and political connections

Our reference to the use of political connections as a cause of worker deaths in the introduction can be traced to a number of widely discussed mechanisms. Many of these are outlined in a research report published by the China Labor Bulletin in 2006, and begin at the point of entry into industry and also for expansion to new locales.[6] To operate in hazardous industries such as coal mining and chemicals, a firm must obtain a license which requires dozens of approvals from individual government offices. Higher-level connections can facilitate rapid approval, potentially without the requisite safeguards in place. In construction, firms may subcontract to low-cost (and unqualified) providers, or employ relatively low-cost unqualified workers. These are cost-cutting measures that have been tied to bribery of safety regulators, most notably in the arrest of the chief safety officer of Bangbu City in 2013.[7]

While we do not observe individual share ownership in the companies we study, connected firms often provide ownership stakes to local officials, who are then motivated to promote profits over safety. Further, once operation begins, connected firms are less likely to be shut down in response to observed safety violations - a fact we further verify in our own data. Finally, as emphasized by Sun (2005) in his assessment of the coal industry in Chenzhou, powerfully connected mining companies may deter local officials from monitoring them at all.

This deference to well-connected firms may not continue in the aftermath of fatal accidents, which generally demand a response. Indeed, as Kato and Long (2006a and 2006b) note, management turnover is common following poor financial performance and as a result of

[6] Information on the Chinese Labor Bulletin's may be accessed at http://www.clb.org.hk; the specific report we cite may be accessed directly (in Chinese) at http://www.usc.cuhk.edu.hk/PaperCollection/Details.aspx?id=5630

[7] See http://www.ccdi.gov.cn/lzjy/yajs/201307/t20130715_6183.html for a report of this case (last accessed September 4, 2014).

serious operating or financial problems of any kind. This is consistent with our findings in Section 3.1.3 on the post-accident profitability and high management turnover following worker fatalities. Whether this differentially affects connected firms is ambiguous. Cao et al (2011) suggest that well-connected individuals may be insulated from the consequences of poor performance, implying a muted impact of worker deaths on connected firms. One the other hand, Wang and Qian (2011) and others have argued that, since firms run by former public officials have greater public exposure, extreme events like worker deaths may demand a *greater* response for connected firms. We will examine this question empirically in the results that follow.

1.4 Data

Our sample includes all publicly traded Chinese firms from the following industries (China industry classifications in parentheses): Coal mining (industry code B01, 25 firms); Petroleum and Natural Gas Extraction (B03, 2 firms); Ferrous Metals Mining and Dressing (B05, 2 firms); Nonferrous Metals Mining and Dressing (B07, 8 firms); Petroleum Refining and Coking (C41, 5 firms); Raw Chemical Materials and Chemical Products (C43, 124 firms); Smelting and Pressing of Ferrous Metals (C65, 34 firms); Smelting and Pressing of Nonferrous Metals (C67, 41 firms); and Construction (E01, 35 firms).[8] These industries are the regulatory focus of the State Administration of Work Safety, which is both an indication of the relative importance of workplace safety and also facilitates the collection of reliable firm-level worker mortality data, as discussed in greater detail below.

To collect worker death statistics, we began by examining firms' Corporate Social Responsibility (CSR) reports, which large publicly traded firms started to provide beginning in 2008. These reports often disclose any accidents involving worker fatalities. We also reviewed firms' annual reports, semi-annual reports, quarterly reports and other announcements: while some firms failed to provide CSR reports, all publicly traded firms are required by the Chinese SEC to disclose workplace accidents that result in fatalities in some manner. To cross-check the figures obtained from companies' own disclosures, we utilized data from the official website of the State Administration of Work Safety (SAWS) in China[9] and websites of its local branches, which provide an accident searching function. According to the "Notice on improving accident information report from the State Administration of Workplace Safety," beginning in 2008, all firms in China were required to report workplace deaths to the local branches of SAWS in a timely manner, which in turn were meant to be publicly disclosed. Prior to 2008, firms were also required to report such information; however, due to the modest penalties for

[8] The initial sample included 125 chemical firms, but one chemical firm was dropped because it had zero sales in two of the sample years, and went bankrupt in 2012.

[9] http://media.chinasafety.gov.cn:8090/iSystem/shigumain.jsp

non-reporting prior to 2008, these earlier data are very incomplete and thought to be of poor quality. The lack of significant penalties similarly affected the disclosure of safety information in company reports. Owing to these shifts in reporting that took place in 2008, we focus on the sample period 2008-2013; In our Data Appendix, we provide further details comparing company and government data sources. (As an indication of the enormous shift in reporting, the fraction of firms reporting at least one workplace fatality increased eight-fold between 2007 and 2008, as more stringent disclosure rules went into effect.)

For the construction industry, firms are not required to report workplace fatalities to the public in their CSR or other corporate reports, since construction workers are typically contract rather than formal employees. We obtain data for this industry through a proprietary accident-level dataset from the Ministry of Housing and Urban-Rural Development of China (MHUDC). Construction firms are required to report any accident involving fatalities to the MHUDC and have been required to do so since 2008. These accident-level data were matched by name to our sample of listed firms. 35 Construction companies contribute a total of 123 firm-year observations to our panel dataset and a total of 243 deaths.

For 57 firm-year observations from 16 firms, death information was not found in government websites or company reports. In these cases, we supplement our data using a media search based on the WiseSearch (www.wisers.com) database. WiseSearch, located in Hong Kong, has broad coverage of newspaper reporting in China, including stories from more than 2,000 newspapers, covering over 80 percent of province-level newspapers, city newspapers and all national financial newspapers. These 57 firm-year observations contribute 11 deaths to our full-sample total of 1332, a reported death rate that is far lower than that of firm-year observations from official or company sources.

Finally, we note that for accidents involving three or more worker deaths - defined by SAWS as "severe" - there is a high level of consistency between CSR and corporate reports and those provided by SAWS, and in no case was a severe accident reported in the media but not in official sources.

Our final dataset is comprised of 1475 firm-year observations, with a total of 1332 worker fatalities. This represents an annual worker death rate for the overall sample of about 0.65 workers per ten thousand, somewhat below the national average of 0.93. While this may seem puzzling given our focus on safety-regulated industries, it is a natural result of sampling only from publicly traded companies.

We define four measures of worker mortality: $Deaths_{cy}$, total worker fatalities at company c during calendar year y; $DeathRate_{cy} = 1,000 * Deaths_{cy}/Employment_{cy}$, our favored measure, gives worker mortality scaled by employment; $I(Deaths_{cy} > 0)$, an indicator variable denoting whether there was at least one fatality at firm c in year y; and an indicator for firm-years when at least one severe accident (three or more fatalities) occurs, $Severe_{cy}$. Owing to several extreme observations for *DeathRate* we winsorize this variable at the 1 percent level.

For the full sample, at least one fatality occurs in 13.5 percent of firm-year observations. In Figure 1, we show the frequency distribution of *Deaths* for these 199 non-zero observations. While there are some very high fatality firm-year observations concentrated in mining and construction (all but one 15+ fatality observations are in these industries), most observations are clustered in the single digits.

In 284 accidents that led to a total of 1146 deaths (out of the total of 1332 in our firm-year panel data), we were able to pinpoint the precise date of the accident. The discrepancy owes to the fact that in some cases CSR or other corporate reports provided only annual aggregates or failed to provide exact dates; in a subset of these cases where only aggregate information was provided, we were unable to determine the precise date of an accident through media accounts. We focus on the 284 accidents with precise event dates in our market reaction analysis below.

Our measure of political connections is based on the previous employment histories of top executives at each firm. We define a company to be connected if a senior manager held a high-level government post prior to joining the firm. Information on top executive teams is provided by companies' annual reports, based on year-end employment. To construct this measure, we collected the resumes of senior managers for our sample of firms from Wind Information, the largest vendor of raw financial data in China. We focus on the backgrounds of the Chairman, Vice-Chairman, CEO, and Vice-CEOs at each company, positions roughly equivalent to "C-suite" executives at an American firm. These resumes provide employment histories, including past positions in government. We define a firm-year indicator variable, $Connected_{cy}$, to be equal to one if a senior executive on December 31 of year y-1 at firm c previously served as mayor or vice-mayor ("Tingju Ji" in Chinese) in the city where the company is located, or as a provincial or central government official of the same or higher rank.[10] In practice, over 90 percent of our connected executives came from the municipal bureaucracy, so we lack the statistical power to examine the heterogeneous effects of connections based on the level of government.

We make a couple of observations about the timing of data on connections and accidents. First, we measure connections at the beginning of each calendar year; the possibility of mid-year executive turnover introduces classical measurement error in *Connected*, most plausibly biasing our results downward. Second, the fact that we use beginning-of-year observations on political connections - that necessarily precede accidents - will be particularly important in the specifications that include firm fixed-effects, since it ensures that a new executive will have been in place for at least a short period of time prior to the occurrence of fatal accidents

[10] Lower-level bureaucrats that run smaller sectors or departments within a municipality ("Ke Ji" in Chinese) would not necessarily outrank workplace safety authorities.

that we may attribute to his employment at the firm.

Firms are not required to disclose workplace safety violations uncovered by regulators. As a measure of safety enforcement - albeit an imperfect one - we collected safety violation information using a media search of the WiseSearch (www.wisers.com) database.[11] We define $SafetyViol_{cy}$ as an indicator variable denoting whether company c was reported for a major safety violation in year y. This skews our list of violations toward relatively severe ones, and also toward large and visible ones. As a second proxy of regulatory enforcement (which simultaneously captures the extent of compliance), we collected a list of fines for environmental violations for our sample firms from the Institute of Public & Environmental Affairs, a non-profit organization located in Beijing.[12] We define an indicator variable $Pollute_{cy}$ to denote a year y where firm c was fined for environmental non-compliance. For 87 firm-year observations (5.6 percent of the sample) during 2008-2011, the Institute of Public and Environmental Affairs listed no information in violations, so for regressions involving *Pollute* our sample is comprised of 1388 observations. (Our results are insensitive to coding these as zeros.)

During our sample period, the government held significant ownership stakes in a number of publicly traded firms, including many in our sample. Plausibly, the composition of a company's top executive team is correlated with ownership status, so it is important to control for government holdings. We define $StateOwnership$ to be the fraction of equity held by government entities of all levels (municipal, state, and central) either directly or through state owned enterprises. These data are taken from CSMAR at GTA, a Shenzhen-based financial data vendor.

To examine the effects of regulator incentives, we define the variable $NSNP_{py}$ to be a province-year variable denoting whether province p had passed a "No safety, no promotion" law in year y. We define $NSNP_{py}$ as beginning in the first full year that a law was in effect. (Our results are insensitive to defining *NSNP* instead on the basis of the fraction of days that a law was in effect during its year of passage.)

Finally, we collected data to construct control variables, including stock price, return on assets (ROA), total assets, and investment (defined as the ratio of capital expenditure to total assets) from CSMAR at GTA. We obtain total employment figures from Resset, a Beijing-based financial data vendor. For total assets, sales, and employment, we use figures from the beginning of year y, while we employ lagged values for the flow variables ROA and investment. The

[11] We first use a combination of company name (including both the abbreviated name and the full name) and some key Chinese words that refer to safety violations, specifically: Anquan Shengchan, Xingzheng Chufa, Fakuan, Tingchan, Zhengdun, Tingye, Zhenggai, Jinggao) to download all possible newspaper reports. This produced 6265 stories which were then processed by research assistants at Nanjing University to manually code information on safety violations, since some downloaded documents turned out to be "false positives."

[12] This organization has devoted considerable effort to collecting environmental violation information from a range of sources, mainly media reports and from its cooperation with different branches of Ministry of Environmental Protection of the People's Republic of China. The list may be found at http://www.ipe.org.cn/about/about.aspx (last accessed on September 1, 2014).

asset and employment figures are used to construct $CapInt_{cy} = log(Assets_{cy}/Employees_{cy})$, a measure of firm c's capital intensity in year y.

Table 1A presents summary statistics for the firm-year panel data. We further show these summary statistics for the full sample split into politically connected and unconnected firm-year observations in Table 1B. Of particular note, politically connected firms have, on average, .084 worker deaths per 1,000 employees each year, whereas the comparable figure for unconnected firms is 0.024 deaths per 1,000 employees. The worker death rate for unconnected firms is thus somwhat below the national average of 0.093 per 1,000 in 2005, based on statistics from the China Safety Production Yearbook, while that of connected firms is several times higher than the national average. Politically connected firms are also larger, as measured by employees or assets, an issue that we will take up in detail below. State ownership is also correlated with political connections - the mean level of state ownership is nearly 80 percent higher for $Connected = 1$ firms. Connected firms are more profitable, as measured by return on assets, consistent with the prior literature on the value of political ties. Finally, connected firms have significantly higher rates of major safety violations, but as we will see shortly this is simply due to the fact that fatalities - which are far higher among connected firms - tend to trigger reports of safety violations.

In Table 1C, we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. For this set of firms, which we define as the $Var(Connected) > 0$ sample, differences between connected and unconnected firms are generated by executive turnover. Here we find the two groups are much more balanced on observables - connected and unconnected firms are of similar size (based on employment, assets, and sales) and have comparable rates of investment (though state-controlled firms still employ connected firms a higher fraction of the time). We still find, however, that connected firms are more profitable, though the (unconditional) difference is significant only at the 10 percent level. Further, the differences in death rates remain large and highly significant in this subsample, a pattern that will be reflected in our within-firm regression results below. There is no significant difference in workplace violations between connected and unconnected firms, though we show below that this gap is considerably wider for firm-year observations where no fatalities occur.

Before proceeding to the model that we will use to organize our results, we provide a graphical overview of the main patterns in our data in Figures 2A and 2B, using the full sample and the subsample of companies that have within-firm variation in *Connected* respectively. For ease of exposition, we group together industries with fewer than 50 firm-year observations. The patterns are fairly similar across the two figures. With the exception of smelting non-ferrous materials, where fatalities are virtually non-existent, worker mortality is higher in connected versus unconnected firms. This difference is smallest for coal mining. One possible reason for this is that given the relative isolation of coal mines, under-reporting of fatalities is a more of a concern. This explanation is borne out in media reports of firms - particularly those with ties to local governments - attempting to coerce victims' families into maintaining silence (see, for example, "Graft in China Covers Up Toll of Coal Mines," *New York Times*, April 10, 2009).

2 Theoretical framework

To provide a basic framework for our analysis, we present a simple Becker-style audit model that captures many of the stylized facts that we document in our empirical analysis. The reader focused exclusively on our empirics may wish to proceed directly to Section 3.

We employ a two period model of firm i's production with fixed (non-safety) costs (C_i) and revenues (R_i). At the beginning of period 1, firms make the decision of whether to hire a connected executive and whether to shirk on safety expenditures, which are set at a level s_i mandated by regulators (and known to all parties). Production then takes place for two periods.

To begin, we make the extreme assumption that safety regulators' incentives are such that unconnected firms *always* comply with requisite safety expenditures, while connected firms never do. We will turn to the question of regulator incentives below, and endogenize the compliance decision. The pay of unconnected executives is normalized to zero, and we model connected executives as taking a share α of profits, which they earn as a result of the cost savings they generate. We further simplify the analysis by modeling the probability of a fatal accident as occuring with probability p for firms that fail to make safety expenditures and zero for those that do. The cost of a fatal accident is a fine F, and we further assume that following worker fatalities connected individuals lose their influence (and hence are also of no value to firms).

We allow for firms to have differing costs of safety compliance, but drop subscripts for R and C to focus on heterogeneity based on safety costs. This set of assumptions yields the following two-period profits for connected versus unconnected firms:

$$E(\pi_i) = \begin{cases} 2(R - C - s_i), \; if\; Connected_i = 0 \\ (1-\alpha)(R-C) + p(R - C - s_i - F) + (1-p)\left[(1-\alpha)(R-C) - pF\right], \; if\; Connected_i = 1 \end{cases} \tag{1}$$

For unconnected firms, this expression is straightforward to interpret: profits are revenues less total costs (non-safety and safety) in each period. For connected firms, the first term is operating profits in period 1. If an accident occurs (with probability p), the firm loses its connection, earning $R - C - s_i$ in the second period, and also paying a fine F. If no accident occurs, the firm maintains its connection, again earning $(1-\alpha)(R-C)$, and with probability p must pay a fine F if an accident occurs in period 2.

A firm will hire a connected executive if $E(\pi_i|Connected = 1) - E(\pi_i|Connected = 0) \geq 0$, i.e.,

$$s_i \geq (R - C)\alpha + pF \tag{2}$$

This reductionist description of political connections has a number of properties – both assumptions and predictions of the model – that we can evaluate in our empirical analyses:

1. Based on the assumption that it is more costly to monitor connected firms, it follows immediately that connected firms shirk on safety at higher rates than unconnected firms.

2. Also by assumption, connected firms are more profitable, given that the value created by avoiding safety expenditures is split between the firm and the connected executive himself.

3. Connections are more prevalent among firms that face higher safety costs since condition (1) is satisfied for a larger set of parameter values $\{C, R, \alpha\}$.

4. An accident at a connected firm leads to a decline in expected profits, as a result of the fine incurred and loss of savings on safety costs in the second period.

We now incorporate regulator incentives into our model that will allow for partial auditing of both connected and unconnected firms. This will help to provide an intuition for differences in the death rates and audit outcomes of connected versus unconnected firms, and how this difference is affected by increased costs (to the regulator) of non-compliance. Our approach is in the spirit of a standard Becker (1974) deterrence model (and also related to public finance models of tax auditing; see Andreoni et al (1998) for an overview), where an interior solution requires mixed strategies by both auditors and firms. In our model, we represent the probability that a firm of type i chooses to shirk on safety expenditures by r_i and the probability of an audit by q_i.

We assume that a regulator faces an audit cost a_i for auditing firm i. We will denote the audit cost as a_{nc} if $Connected_i = 0$ and a_c if $Connected_i = 1$, and assume that $a_{nc} < a_c$ due, for example, to the influence cost of auditing a company run by a politically powerful individual. The audit cost must be weighed against the career consequences of worker deaths that may occur in the absence of safety compliance. We assume that both the decision by the firm to shirk and the decision by the regulator to conduct an audit are made at the beginning of period 1 (and apply to both periods), and denote the career cost to the regulator of a fatal accident in any period by D_i. Retaining p as the probability of accident given shirking, in a mixed strategy equilibrium the regulator's decision to audit is dictated by the equality $r_i\left[pD_i + (1-p)pD_i\right] = a_i$, since the probability of a fatal accident occuring in period 1 is p while the probability in period 2 (conditional on no accident in period 1) is $(1-p)p$. This yields the following condition on the probability that firm i shirks:

$$r_i \leq \frac{a_i}{p(2-p)D_i} \tag{3}$$

The expression holds with strict equality for mixed strategies. With a sufficiently high (or D sufficiently low), we obtain a corner solution where all firms of type i shirk and regulators audit with probability zero.

For an unconnected firm, the mixed strategy equilibrium requires that the audit rate q_i satisfy the condition that expected profits from shirking and compliance are equal, that is:

$$q_i\left[2(R-C-s_i)-F\right]+(1-q_i)\left[(R-C)+p(R-C-s_i-F)+(1-p)\left[(R-C)-pF\right]\right] = 2(R-C-s_i)$$

The first bracketed term shows expected profits in the event that the firm is audited (identical to the profits under compliance less a fine F) while the second term shows expected profits if no audit occurs (similar to the expression for *Connected* firms in (1) above). This yields the following condition on q_i :

$$\frac{1}{q_i} = 1 + \frac{F}{(s_i - pF)(2-p)} \tag{4}$$

Since $q_i \in (0,1)$ a mixed strategy equilibrium may hold only with sufficiently high safety costs such that $s_i > pF$. Assuming that this condition holds, expression (4) has a number of intuitive properties: the audit rate q_i is decreasing in the fine F, as firms will require less frequent auditing if the consequences of getting caught are more severe; higher safety cost s_i results in a higher audit rate, as firms will be more tempted to shirk to avoid this cost; and a higher accident probability reduces the probability of audit as firms will be more likely to face penalties even in the absence of an audit.

For connected firms, we first consider the case where the audit cost a_c for connected firms is such that $a_i \geq p(2-p)D_i$: this leads to a corner solution where connected firms shirk and unconnected firms are in a mixed strategy equilibrium with audit probability and shirking rate defined by (3) and (4).

If the cost of fatalities to the auditor D increase such that connected firms are no longer at a corner solution, then the model is no longer consistent with different audit rates for connected versus unconnected firms, nor is it consistent with the higher profitability of connected firms (or higher pay for connected executives). To see why this is the case, observe that for a mixed strategy to be supported, a firm must be indifferent between shirking and compliance. But a connection generates value only when a firm decides to shirk (since its audit rate will be lower). Hence, in this second case we expect to see a convergence in audit rates *and* in profitability of connected and unconnected firms. Further, once profits of connected and unconnected firms converge, connected executives can no longer extract a higher share of profits.

Our model makes thus the following additional predictions that we may examine in the data:

1. If the cost of auditing connected firms,a_c, is initially very high (sufficient to generate a zero audit corner solution), connected firms will receive very few fines, despite having higher accident probabilities. .

2. If the implementation of NSNP laws led to a large increase in D, we may observe a large drop in worker deaths at connected firms and a convergence with the worker death rate at unconnected firms. This is the result of a shift among connected firms from corner solution to mixed strategy equilibrium.

3. Following implementation of NSNP laws, profits at connected firms will converge with the profits of unconnected firms.

4. Following implementation of NSNP laws, fewer firms will employ connected executives (and the pay of connected executives will decline), since connections no longer generate higher firm profits.

As we will show in our empirical results, the latter two predictions are not borne out in our data: firms continue to hire connected executives after the implementation of NSNP laws, and connected firms continue to earn a premium relative to unconnected firms in provinces that institute stronger safety incentives. These patterns can be reconciled, however, with a richer model that allows for "substitution effects" in how firms deploy their connections. We present evidence in favor of such substitution effects in our empirical analysis below, as fines for environmental violations go down *just among connected firms* following the passage of NSNP laws. That is, instead of using connections to shirk on safety, firms use connections to shirk on pollution abatement.

3 Empirical Results

3.1 Political connections, worker deaths, and firm profits

3.1.1 Connections and worker deaths

We begin by extending our analysis of the patterns revealed in Figure 2, relating *Connected* to worker fatalities, in a regression framework. Our baseline specification is as follows:

$$\begin{aligned} Deaths_{cy} \quad = \quad & \beta_0 + \beta_1 \times Connected_{cy} + \beta_2 \times StateOwnership_{cy} \\ & +\beta_3 \times CapInt_{cy} + \beta_4 \times ROA_{cy-1} \\ & +\beta_5 \times log(Sales)_{cy} + \beta_6 \times Investment_{cy-1} + \varepsilon_{cy} \end{aligned} \tag{5}$$

We include fixed effects for year, industry, and province in most specifications, and will further augment these controls with firm fixed-effects in some cases. In most of our specifications, we will assume a normally distributed disturbance term. While we begin by showing results with various monotonic transformations of *Deaths* as the outcome variable, for most of our analyses we will focus on $DeathRate_{cy}$ (workplace deaths per 1,000 employees) as our measure of workplace fatalities. Finally, to deal with concerns of unobserved differences between connected and unconnected firms, we also present results using the sample of companies that have within-firm variation in *Connected*, where the effect of connections is identified from executive turnover; we denote this set of firms as the $Var(Connected) > 0$ sample.

Before turning to standard OLS specifications, we first show our main results in Table 2 using a count model for *Deaths*, assuming a negative binomial distribution (a Poisson distribution is a poor fit for the death distribution in the data). In all specifications in all tables, we cluster standard errors by firm. We begin with a specification with no covariates. The coefficient on *Connected* is very large in magnitude - its coefficient of 1.953 implies a death incidence rate for connected companies that is just over 700 percent ($e^{1.953}$) of the baseline, significant at the 1 percent level. Adding controls in column 2 reduces the coefficient on *Connected* to 1.7754 (a 578 percent increase relative to baseline, significant at the 1 percent level), while the further addition of province, year, and industry fixed effects has little impact on the coefficient (column 3). In column 4 we include firm fixed-effects. This demanding specification limits our sample to those firms with at least one non-zero value of *Deaths* (otherwise, the fixed effect in our negative binomial estimation is undefined). This yields a smaller effect, albeit one that is still statistically significant and large in magnitude, implying that the death incidence rate for connected firms is 216 percent of the baseline. When we employ the $Var(Connected) > 0$ sample, the coefficient falls marginally to 1.52 (a 458 percent increase relative to baseline), significant at the 1 percent level, while adding firm fixed effects to this specification reduces the coefficient on *Connected* to 0.855. It is noteworthy that no other coefficient, apart from our size proxy of $log(Sales)$, is consistently a significant predictor of *Deaths*.

We next present a set of OLS specifications in Table 3, first using $log(1 + Deaths)$ as the dependent variable. In column 1 we present results using the full set of controls in Equation (5), as well as industry, year, and province fixed effects. The coefficient of 0.282 is signficant at the 1 percent level; the coefficient is virtually identical when we limit the sample to $Var(Connected) > 0$ firms (column 2) and increases to 0.37 with the inclusion of firm fixed effects (column 3). In columns 4-6 we show results based on our preferred outcome variable of *DeathRate* (deaths per 1,000 employees). The coefficient on *Connected* is approximately 0.05 across all specifications. Given the average *DeathRate* among $Connected = 0$ firms of 0.024, this implies a worker death rate that is three times higher at connected firms. For the $Var(Connected) > 0$ sample, the mean *DeathRate* among unconnected firms is 0.039, so the coefficient on *Connected* implies a more than two-fold difference in worker death rate. Finally, in columns 7-9 we use $I(Deaths_{cy} > 0)$, an indicator variable denoting at least one fatality at firm c in year y, as the dependent variable. The coefficient on *Connected* is in the range of 0.1 across all specifications, though not significant at conventional levels for the specification using the $Var(Connected) > 0$ sample without firm fixed effects (p=0.12). In Appendix Table A2, we provide an alternative approach to dealing with the differing attributes of connected versus unconnected firms based on propensity score matching. This yields a difference in death rate that is comparable to what we find in our OLS regressions. Finally, in Appendix Table A3 we add industry-specific time trends to each of our specifications; the coefficient on *Connected* is virtually unchanged across all specifications, indicating that differential industry trends are not responsible for our results.[13]

3.1.2 Connections and regulatory enforcement

In our simple audit model, it is regulators' high cost of monitoring connected firms that leads to more fatalities. As we explain in Section 1, firms are not required to disclose safety violations, so we use cases of major safety investigations by the government reported in the media as our measure of enforcement actions. We are interested, in particular, in differences in reported violations in years when no fatalities occur, since workplace deaths themselves may trigger an audit. In Table 4, we provide the crosstab of *Connected* and $I(Deaths_{cy} > 0)$, showing the average safety violation rate, *SafetyViol*, for the $Var(Connected) > 0$ sample (violation rates are uniformly lower in the full sample, since *SafetyViol* is correlated with firm size). There are two striking patterns. First, in the absence of worker fatalities, there is no public report of a safety violation when a firm employs a connected executive. In the absence of a connection, the rate is 4.6 percent. Owing to the small number of non-zero

[13] Our results are also unchanged if we include controls for leverage or stock price volatility.

observations, standard regression techniques are inappropriate for assessing this difference. We use Fisher's exact test to assess the difference, which generates a p-value of 0.11. We observe a very different pattern in firm-years when fatalities occur, when the rates of reported safety violation are virtually identical for connected and unconnected firms (16 versus 15.8 percent).

We also consider a second measure of the extent of monitoring based on whether a firm was fined in year y for environmental noncompliance, $Pollute_{cy}$. As with safety violations, we focus on the $Var(Connected) > 0$ sample because the strong correlation between fines and size raises particular concerns of balance in the connected and unconnected samples. The raw difference in *Pollute* between connected and unconnected firms is -0.099 (0.170 versus 0.071, p-value for the difference in means is 0.02).[14] In Table 5, we show that this relationship remains significant across various regression specifications. Column 1 shows the simple bivariate correlation between *Connected* and *Pollute*. The relationship is virtually unchanged with the addition of controls for size, capital intensity, and so forth in column 2, while the further addition of year, industry, and province fixed effects (column 3) reduces the magnitude of the coefficient on *Connected* to -0.071, signficant only at the 10 percent level. But this, in a sense, may over-control for firm attributes - connections may be clustered in industries (or provinces) where environmental or safety violations are common, and the inclusion of fixed effects absorbs this relevant between-industry or province variation.

3.1.3 Connections and firm profits

A final piece of corroborating evidence - already suggested by a comparison in the raw data - is that connected firms enjoy higher profitability as a result of their protection from regulatory oversight. In the first two columns of Table 6, we show that this pattern holds for both the full and $Var(Connected) > 0$ samples. For the full sample, we find that connections are associated with a 1.7 percentage point increase in *ROA* (significant at the 1 percent level), while for the $Var(Connected) > 0$ sample the effect is a 1.1 percentage point increase (about 20 percent), significant at the 10 percent level..

In columns 3-4, we more directly tie firm profitability to our model of safety monitoring by looking at profits in $y + 1$ as a function of both connections and its interaction with the occurrence of worker fatalities in year y. The effect is *ex ante* ambiguous. In the organizing framework we present above, if connected individuals lose their ability to circumvent costly safety compliance after fatalities occur, then an accident should lead to a relatively large profit decline among connected firms (and potentially the departure of connected executives, whose connections are no longer valuable). A plausible alternative is that connections serve

[14] There is no difference between the two groups for the full sample, resulting from the fact that unconnected firms are much smaller on average than connected firms.

to insulate firms from regulatory backlash following worker deaths, in which case one would expect the value of well-connected firms to fare relatively well after an accident. (Similarly, given the high accident rates at connected firms, a fatality represents a lesser surprise to the market, and its effect should thus have been largely incorporated into market prices.)

The data favor the 'loss of privilege' view of connections. For the full sample, the coefficient on the interaction term, $Connected * I(Deaths > 0)$, is negative (significant at the 1 percent level) and of approximately the same magnitude as the direct effect of *Connected*. For the $Var(Connected) > 0$ sample, the interaction term is negative and nearly twice the size of the direct effect of *Connected*, indicating that if an accident occurs at a connected company in year y, in expectation profits will be lower in $y + 1$ than if it had been unconnected in year y. Our results in the previous section further argue against connections serving to protect firms from regulatory backlash following worker deaths. While we observed zero reports of workplace safety investigations (*SafetyViol*) at connected firms in the absence of worker deaths, in years where fatal accidents occur *SafetyViol* is identical for connected and unconnected firms, suggesting that connections do not insulate firms from post-accident regulatory responses.

As an alternative approach to studying the impact of accidents on profitability, we also look at market reaction to news of fatal accidents (which are, by definition, unexpected). We define the accident date as $t = 1$ and examine the subsequent change in share price as this information is incorporated by investors. In most cases the formal announcement of worker deaths comes much later than the accident itself, so it is critical in our analyses to allow for a fairly long event window. Further, for a number of cases, multiple fatal accidents occur within a single month of one another for a single firm. These instances are clustered in the construction industry, which has a very high accident rate: the 37 construction firms in the sample generated 143 of the 284 identifiable accidents in the dataset. To prevent double-counting of returns that follow multiple temporally adjacent accidents, we proceed as follows: proceeding forward from each company's first accident in the sample, we drop accidents that occurred within 30 trading days of the initial accident. We then proceed to the next accident (that has not been dropped) - by definition more than 30 trading days following the first - and repeat the process. There are 41 instances of accidents clustered within 30 trading days, 33 in construction, and leads to the deletion of 64 observations (sometimes more than two fatal accidents occurred within 30 trading days). We also define a variable to account for the full set of fatalities taking place at company c within a month following each initial accident a, $MonthlyDeaths_{ca}$. Finally, we omit the 10 observations where trading was suspended following the accident or insufficient pre-event data were available to calculate abnormal returns.

In Figure 3, we present cumulative abnormal returns (CARs), estimated using a standard four-factor model, i.e., Fama-French three factors and also the Carhart momentum factor, over event windows of up to 30 trading days following each accident date. Since the rate of fatal accidents is much higher for connected firms, the event study sample has a higher fraction of *Connected* = 1 observations (24.8 percent of the total) relative to our firm-year sample.

We show CARs for the full sample and also for the sample divided based on *Connected*. For the full sample, there is only a very small negative market response following an accident. For the subsample of *Connected* = 0 firms, the average CARS from [1,1] to [1,30] are slightly positive, though in no case significantly different from zero. By contrast, for the *Connected* = 1 subsample we observe a steady decline in firm value from the date of an accident. There is good reason to expect this delay in investor response: in most cases, the fatalities were not disclosed by the company at the time the accidents occurred, nor were they reported in the media outlets covered by WiseSearch, so news may be slow to reach the market.

The regression results in Table 7 echo the patterns observed in Figure 3 - politically connected firms suffer relatively large declines in market value following worker deaths, with the decline increasing over the 30 trading days following each accident. Over a three day window following an accident, connected firms decline by about 1.4 percent more than unconnected firms do. This difference widens to more than 8 percent after 30 trading days. The coefficient on *Connected* is significant at least at the 10 percent level across all windows. We note additionally that total deaths in the 30 trading days after the accident is also negatively correlated with returns, though the coefficient on $log(MonthlyDeaths_{ca})$ does not approach statistical significance in any event window.

In our model, we assume that connections lose their value after fatal accidents, and firms no longer benefit from employing them. More generally, it is very common in China for high-level firings to take place following negative company news of any kind (see Yan (2004) among many other media reports for a reference), further reinforcing the prediction that connected executives (and any residual value they generate) will depart following a fatal accident.

For a company that is politically connected in year $y-1$, we find that the likelihood that the company is still connected in year y is 19 percentage points lower if a fatal accident occurred during $y-1$ (64.0 percent versus 44.7 percent). In Table 8, we examine the relationship between worker deaths and the loss of political connections in a regression framework using the following specification, which captures the relationship between fatalities in year y-1 and political connections at the beginning of year y:

$$
\begin{aligned}
Connected_{cy} \quad = \quad & \beta_0 + \beta_1 \times I(Deaths_{cy-1} > 0) + \beta_2 \times Connected_{cy-1} + \beta_3 \times CapInt_{cy-1} \\
& +\beta_4 \times ROA_{cy-1} + \beta_5 \times log(Sales)_{cy-1} \\
& +\beta_6 \times Investment_{cy-1} + \varepsilon_{cy}
\end{aligned} \tag{6}
$$

For the full sample, the coefficient on $I(Deaths_{cy-1} > 0)$ in column 1 is -0.073 (significant at the 1 percent level) indicating that a firm is 7 percentage points less likely to be connected in the year following a fatal accident. This sample includes many firms that are never connected during 2008-2013; in column 2 we focus on the $Var(Connected > 0)$ sample, where the coefficient on $I(Deaths_{cy-1} > 0)$ is -0.248, and is virtually identical when we add firm fixed effects in column 3. [15]

In closing this section, it is important to note that while fatal accidents often trigger the departure of connected individuals, executives themselves tend to fare well in the managerial labor market after accidents occur. In the 26 instances in our sample where political connections are lost in the year following a fatal accident, we find that in 19 cases the connected individuals went on to take other high-level positions at other firms (in two cases at the parent company to the listed firm). In four cases the connected manager retired completely (though these executives were already in their late 50s) and in three cases the connected managers went on to work at semi-governmental agencies.[16] The fact that connected executives do well in the managerial labor market after presiding over workplace disasters can help to reconcile the relative lack of concern for safety by connected managers with the fact that it often leads to their dismissal.

In summary, our analysis of the link between worker safety and firm profits indicates that, while connected firms are more profitable overall, this profit premium is lost following fatal accidents. These patterns are broadly consistent with the framework presented in Section 2, where connections are profit-enhancing in expectation, but lead to relatively low profits following a negative worker safety realization.

[15] We find, in results not reported, that worker deaths are also correlated with the departure of other (unconnected) executives. However, this relationship is statistically very weak (p-value=0.29), and controlling for turnover of unconnected executives has no effect on the results we report in Table 8.

[16] These patterns are consistent with other accounts of the careers of connected executives forced to depart from companies in the wake of scandals: they often reappear a short while later in high-level positions at other firms. One extreme example is that of Zhixing Gao, a former politician who, in 2001, served as vice-CEO of Chenjiashan Coal when an accident at one of the company's mines killed 38 workers. He went on to serve as CEO of Huangling Coal, where an explosion killed 24 miners in 2004. A number of such cases have been documented by Chu (2008).

3.2 Main results - discussion

The fact that our results in Table 3 survive the inclusion of firm fixed-effects raises the issue of whether executives can reasonably produce such rapid changes in operational outcomes. We begin by noting that the relatively rapid impact of top executives on firm-level outcomes is in line with the earlier literature on CEO turnover. Bertrand and Schoar (2003), for example, show that individual CEOs bring idiosyncratic financial and operational "styles" to how they run a company, a result that has been reaffirmed in subsequent work. Morten Bennedsen, Pérez-González, Nielsen and Wolfenzon (2007) further find large performance changes in a difference-in-differences analysis of CEO transitions, consistent with the view that the impact of leadership changes may be felt very quickly. Bloom et al (2013) provide an indication of how such rapid change is possible: in their study, Indian textile firms randomly assigned to be taught 'better management' by consultants improved their total factor productivity by over 20 percent, as a result of higher output and more efficient use of inventories.

The scale of government-mandated safety funds indicate the level of savings that firms might (relatively quickly) achieve by shirking on compliance. As noted in Section 1.1, construction firms are mandated to hold 2 percent of construction project costs for safety compliance, coal mining firms an average of over 1 percent of revenues, and an average of 0.7 percent of revenues in chemicals, the largest industry in our sample. If connections allow firms to reduce these expenditures substantially, it may allow for a relatively rapid increase in profitability, albeit at a greater risk to workers. (While firms maintain political ties for multiple reasons, it is nonetheless noteworthy that construction and coal are also the two sectors with the highest proportion of firms employing connected executives, employing connected executives in 20 and 11 percent of firm-year observations respectively.)

These figures also give some indication of whether the profit premium enjoyed by connected firms could plausibly be accounted for in significant part by savings on safety expenditures. Recall that return on assets is approximately 1 percentage point higher for connected firms in our $Var(Connected) > 0$ sample (the difference in return on sales is marginally higher, about 1.5 percentage points). If connections allow firms to shirk on a sizable fraction of their mandated safety funds, this alone may account for a reasonably large fraction of this *Connected* profit premium. And this is only on the basis of reduced costs, whereas lower compliance may also allow for increased output. (When we consider the relationship between $log(OperatingCosts)$ and connections, controlling for sales, we find that the cost margin dominates, though our estimates of the revenue effect of connections are extremely noisy, so we cannot rule out an impact on that margin as well.)

The higher profitability of connected firms is also helpful in refuting one of the most obvious alternative explanations for their poor safety records, namely that connected executives are simply incompetent. If this were the case, one might expect connected firms to earn lower profits as well. While this does not rule out the 'incompetence hypothesis' completely, the profit premium of connected firms does contradict the most straightforward form of it. We return to this question below in our robustness section, where we will also look at the educational qualifications of connected executives.

Finally, before proceeding to an analysis of the impact of regulator incentives on worker deaths, we comment on the limits in imposed by our focus on publicly traded firms, and our emphasis on worker deaths rather than worker safety more broadly. Both limitations are imposed by data availability - we have been unable to obtain data on connections at private firms, and firm-level data on injuries are unavailable even for publicly traded firms. As we note in the Data section, worker deaths are likely much higher at private companies overall. We do not, however, have a clear prediction as to whether the correlation between connections and worker deaths is likely to be stronger or weaker among private firms. To the extent that all publicly traded firms are more carefully scrutinized - regardless of their political ties - we would expect that our results here underestimate the effect of political connections in the broader sample of all Chinese firms. On the other hand, the high-level connections among executives in our sample firms are likely to be very rare among (smaller) private firms. This may bias upward our assessment of the positive link between connections and worker deaths.

Apart from questions of data availability, we choose to study worker deaths rather than injuries because it is a less gameable outcome - we will return to the issue of under-reporting of deaths (which does occur) in the robustness section below, but note at this point that it is generally more difficult to avoid disclosing deaths relative to injuries. There is good reason to expect that the two would be positively correlated, and indeed they are at the province-level. Using province-year data on worker deaths and workplace injury claims from the China Labour Statistical Yearbooks, we look at the death-injury 'elasticity' over the years 2006-2011.[17] In a log-log specification including both province and year fixed effects, the coefficient on province-level deaths is 0.20 (signficant at the 10 percent level) in predicting worker injuries, indicating a positive relationship between the two, even after accounting for country-wide changes in safety and province-level differences in mix of industry and so forth (of course, if we omit these controls the relationship is much, much stronger). Thus, while our focus in this paper is on the extreme outcome of fatalities, there is thus reason to believe that deaths may be related to the broader social cost of worker harm.

3.3 The effect of No Safety, No Promotion laws

We now examine whether the relationship between political connections and worker deaths is affected by the existence of "No safety, no promotion" (NSNP) reforms that limited the

[17] 2012 data were not available at the time of writing.

promotion opportunities of regulators who failed to meet safety targets. Plausibly, this made it costlier for regulators to turn a blind eye to unsafe working conditions at a firm. As we observed in our discussion of a standard audit model in Section 2, this may have a disproportionate impact on worker safety at connected firms if, prior to the passage of NSNP legislation, they were largely unmonitored. For previously unmonitored firms, strengthened regulator incentives can cause a relatively large change in firm behavior as auditor and (connected) firm switch from a corner (no-audit) solution to mixed strategies.

As a preliminary overview of the data, in Table 9 we show the average of *DeathRate* for company-year observations in provinces that, at some point during 2008-2013, pass a NSNP law, disaggregated by *NSNP* status and *Connected*. In the first column, we begin by simply looking at whether worker deaths are lower after the passage of *NSNP.* Overall, the death rate is nearly 50 percent lower post-NSNP. This pattern is hard to distinguish from the secular decline in worker deaths across the sample period: death rates dropped from 0.037 per 1,000 employees in 2008 to 0.011 in 2013. In columns 2 and 3, we disaggregate these figures by *Connected* status (again for the sample of provinces that pass NSNP laws during our sample period). Here, we find that connected firms experience a decline in *DeathRate* of 86 percent, as compared to just 30 percent for unconnected firms.

In Table 10 we show that this disproportionate impact of NSNP laws on connected firms holds in a regression framework. In column 1, we show results using the full sample and our standard controls (including industry, year, and province fixed effects). The coefficient on *Connected* is 0.079, about 65 percent higher than in the comparable specification in Table 3, Column 4 which provides the average effect of connections on worker deaths for both NSNP and non-NSNP firm-year observations. The coefficient on *Connected*NSNP* is negative, significant at the 1 percent level, and its value, -0.087, implies an effect of connections on worker deaths for $NSNP = 1$ observations of very close to zero. In column 2 we limit the sample to $Var(Connected) > 0$ firms and in column 3 we add firm fixed effects. The pattern is very similar across all specifications - a large, positive direct effect of connections, and an approximately offsetting effect from the interaction *Connected*NSNP.* In Appendix Table 3, we add a number of further robustness checks: we limit to sample to only provinces that pass NSNP laws during 2008-2013; include province-specific time-trends, province-year fixed effects, and interaction controls for $Connected * log(GDPpercapita)$ and $log(Sales) * NSNP$. In all of these additional specifications, the main results are virtually unchanged.

If the value of connected executives is realized through their ability to circumvent safety regulation, it follows that the profit premium of connected firms should decline after the passage of NSNP laws. To examine this possibility, we use *ROA* as the dependent variable in columns 4-6 of Table 10. The results are inconclusive: While the coefficient on *Connected*NSNP* is insignificant in all specifications, the standard error is large so we can say little about the impact of NSNP on profits. One reason that the impact of safety-focused incentives on profits might be muted is because firms shift to deploying their connections to other forms of rent-seeking. In columns 7-9, we use *Pollute* as the outcome variable. The patterns in this set of regressions indicate that connected firms' lower rate of fines for environmental violations documented in Table 5 comes entirely from *NSNP*=1 observations: the direct effect of *Connected* in these specifications is close to zero, while the coefficient on the interaction term indicates that for firms in $NSNP = 1$ provinces, the rate of environmental fines at connected firms is about 15 percentage points lower (though the difference is no longer statistically significant with the inclusion of firm fixed effects in column 9 (p-value of 0.12)).

Overall, our results in this section indicate a large reduction in worker fatalities for connected firms in provinces that pass NSNP laws; by contrast, there is virtually no effect of the laws on unconnected firms. Our analysis of the effect of NSNP laws on firm profits is too noisy to draw any conclusions; however, we also argue that there is evidence that the profit effects could be limited as firms shift to deploying their connections in other ways, notably in evading enforcement of environmental regulations.

3.4 Identification concerns

There are three primary challenges to identification. First, the difference in measured death rates between connected and unconnected firms may result from differential rates of underreporting. Second, prior political experience may be correlated with unobserved differences in managerial attributes. Third, the selection of connected executives into firms with high probability of worker deaths may result in an upward bias in our results if companies hire former politicians in anticipation of future worker deaths. We argue that, while our findings cannot completely rule out such alternatives, they are hard to reconcile with our full set of results.

Given that fatal accidents may lead to fines and invite greater scrutiny, firms may attempt to cover up or otherwise under-report worker deaths. For example, a *New York Times* article in 2009 describes the near-successful attempt by a mine owner in Hebei province to hide an accident that killed 35 miners in 2008. (Since the mining company involved was not publicly traded, it is not in our dataset. This is a critical distinction, since publicly listed firms are generally held to a higher degree of oversight and disclosure.) Concerns of underreporting may bias our results downward since, as the *Times* article suggests, it may be easier for well-connected companies to avoid disclosing work deaths. Our results are only biased upward if well-connected companies face greater media or regulatory scrutiny.

Our main approach to assessing whether our findings are the result of differences in disclosure for politically connected firms is to focus on severe ($Deaths \geq 3$) accidents that are more difficult - though not impossible - to hide from authorities and hence less subject to underreporting. We present these results in Table 11, which indicates - across a range of specifications - a robust, positive, and statistically significant correlation between *Connected* and severe accidents. Given the base rate of severe accidents for unconnected firms of 2.7 percent (4.9 percent in the $Var(Connected) > 0$ sample), the coefficient on *Connected* implies a rate of severe accidents that is more than three times higher for connected firms for both the full and $Var(Connected) > 0$ samples. (It is also worth observing that the gap in worker death rates between connected and unconnected firms is lower in mining industries where, based on media reports, underreporting is of greater concern. This suggests that, if anything, underreporting biases our results toward zero.)

We next consider whether our results can plausibly be explained by differences between connected and unconnected executives in their competence or interest in dealing with worker safety. It is unlikely that such unobserved differences are driven by a general lack of managerial acumen among connected managers. If this were the case, we would expect their firms to be poorly run more broadly and hence less profitable, whereas as previously noted we observe higher profits at connected companies in the data. Rather, we are concerned specifically with managers' ability to implement worker safety measures, and incentives affecting profit-safety tradeoffs. To explore the possible effect of the executives' competence in executing worker safety, we included age and industry experience of executives as additional controls in our regressions. We argue that age proxies for seniority and experience, while industry background captures an executive's familiarity with the industry's safety practices. These additional controls have little effect on our findings. Further, if connected executives are chosen for risk-taking or are otherwise aggressive in their management style, we would not expect firms under their control to be targeted with fewer fines or investigations for regulatory infractions, as we document in Section 3.1.2.

Connected executives could face additional incentives to shirk on regulations as a result of future political career concerns, to the extent that connected managers are more likely to seek public office in the future. Jia and Nie (2012), for example, emphasize the incentives of provincial governors to promote economic growth, even at the expense of worker safety. It is important to note that the existence of such career concerns still leads to a positive effect of *Connected* on worker mortality rates, which is broadly consistent with our thesis. In practice, though, we believe the data are more easily reconciled with political ties making it easier for firms to elude safety regulations, rather than generating direct incentives to reduce safety. A standard career concerns model, as described in Jia and Nie (2012), would lead managers with political aspirations to increase employment or output in the interests of economic growth, rather than boosting profits, as we find in our data. We find no evidence consistent with the career concerns hypothesis in the data: neither employment nor sales is correlated with connections in our $Var(Connected) > 0$ sample.

A final class of identification concerns relates to the selection of executives into firms with differing worker safety risk profiles. Firms may choose to hire connected executives to manage the fallout if they anticipate workplace deaths in the future, perhaps as a result of new projects or changes in safety policies; this would create an upward bias in our estimate of the effect of connections on worker deaths. However, this selection story would also predict a more muted impact of accidents on share price for connected firms, since this is exactly the scenario for which connected executives have been hired. This contradicts the evidence we report in Section 3.1.3.

While the discussion in this section cannot completely rule out alternative hypotheses or the presence of selection problems, we argue that the evidence is more easily reconciled with the use of political connections to avoid costly safety compliance.

4 Conclusion

In this paper, we document a robust positive correlation between the political ties of executives and worker fatalities at publicly traded Chinese companies. This pattern holds in the cross section and also for within-firm changes in political connections. Despite poor safety records, connected firms are targeted less often for safety investigations or fined for environmental non-compliance, and they are more profitable than their unconnected counterparts. The impact of "No safety, no promotion" laws indicate that strengthening regulators' incentives for oversight may help to weaken the ability of connected firms to avoid costly safety compliance.

Overall, we argue that our results are best explained by a simple audit model framework where well-connected companies using their political relationships to circumvent safety oversight and regulation. By documenting the impact of business-politics ties on a high-stakes social outcome - employee deaths - our results provide an important contribution to our understanding of the impact of political connections on social welfare, and may serve as a counterweight to the benign "efficient grease" view of corruption.

While our work has focused on China, it is possible - at least in theory - to perform a similar exercise in other countries where politically connected firms are prevalent. We may similarly examine the role of connections on other outcomes that are relevant to social welfare, like food safety and counterfeit production. We leave these directions for future work.

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Data Appendix

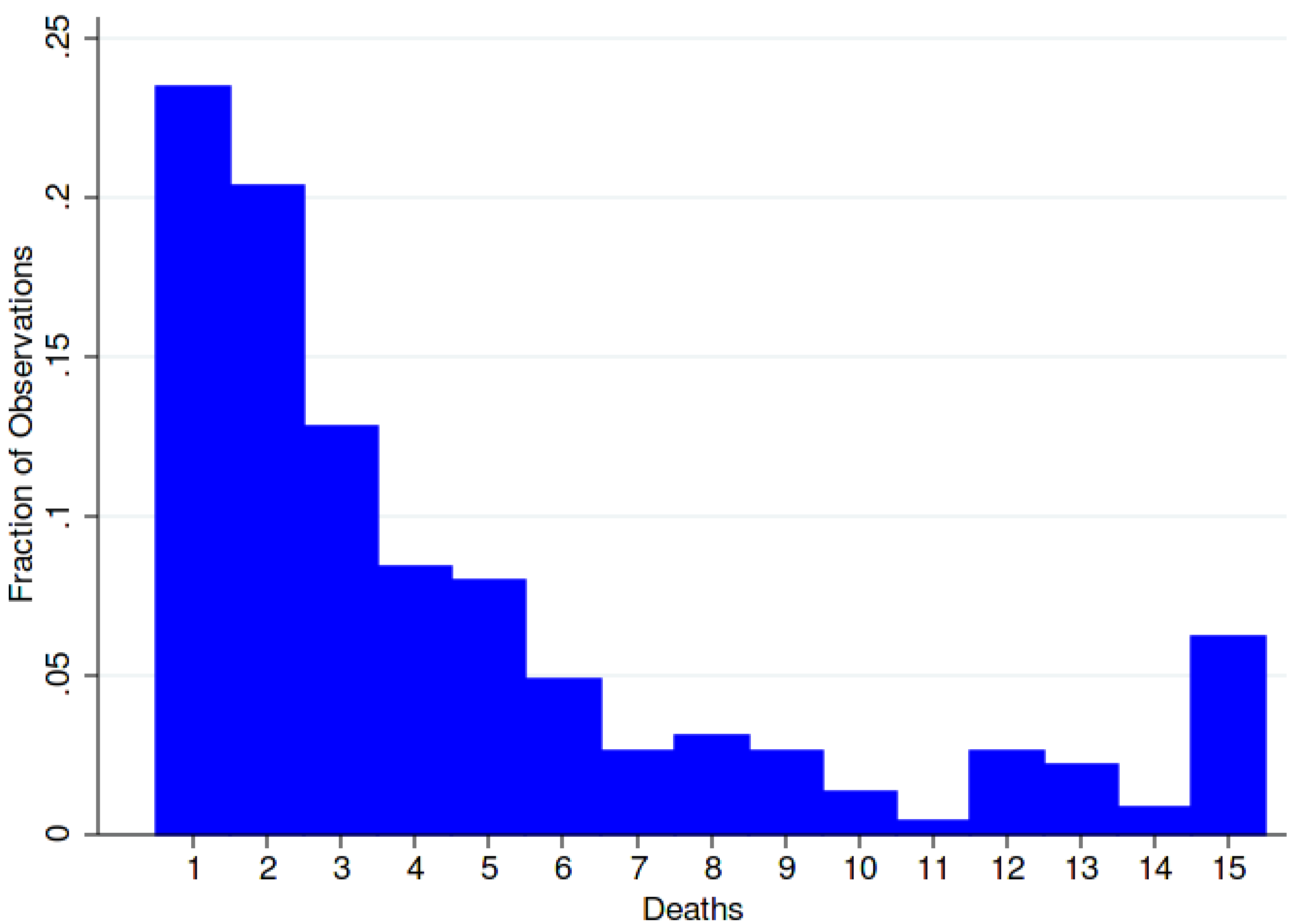
Our final dataset is comprised of 1475 firm-year observations (where we have 276 firms for the period of 2008 to 2013), with a total of 1332 worker fatalities. For the 193 firm-year observations for construction firms, we obtain detailed accident level information from a proprietary accident-level dataset obtained from the Ministry of Housing and Urban-Rural Development of China (MHUDC). Construction firms are required to report any accident involving fatalities to the MHUDC and have been required to do so since 2008. These accident-level data were matched by name to our sample of listed firms. 35 Construction companies contribute a total of 292 deaths (out of a total of 1332 deaths).

For the remaining 1282 (non-construction) firm-year observations , worker fatality information was available via company reports or the State Administration of Worker Safety (SAWS) website (http://media.chinasafety.gov.cn:8090/iSystem/shigumain.jsp) in 1225 cases. In 14 instances, the SAWS website failed to provide full accident-level information, while in 25 cases the company failed to provide details on worker fatalities.

Among the 1186 observations where data were available from both sources, the two lined up very closely, with slightly higher fatality figures based on company reports. Where discrepancies exist, we rely on the (higher) firm-reported figures. The total discrepancy between the two sources is 38 deaths across 14 firm-year observations. The 14 observations with no data from government sources contributed 18 deaths to our sample, while the 25 firm-year observations with no company data contributed 57 deaths.

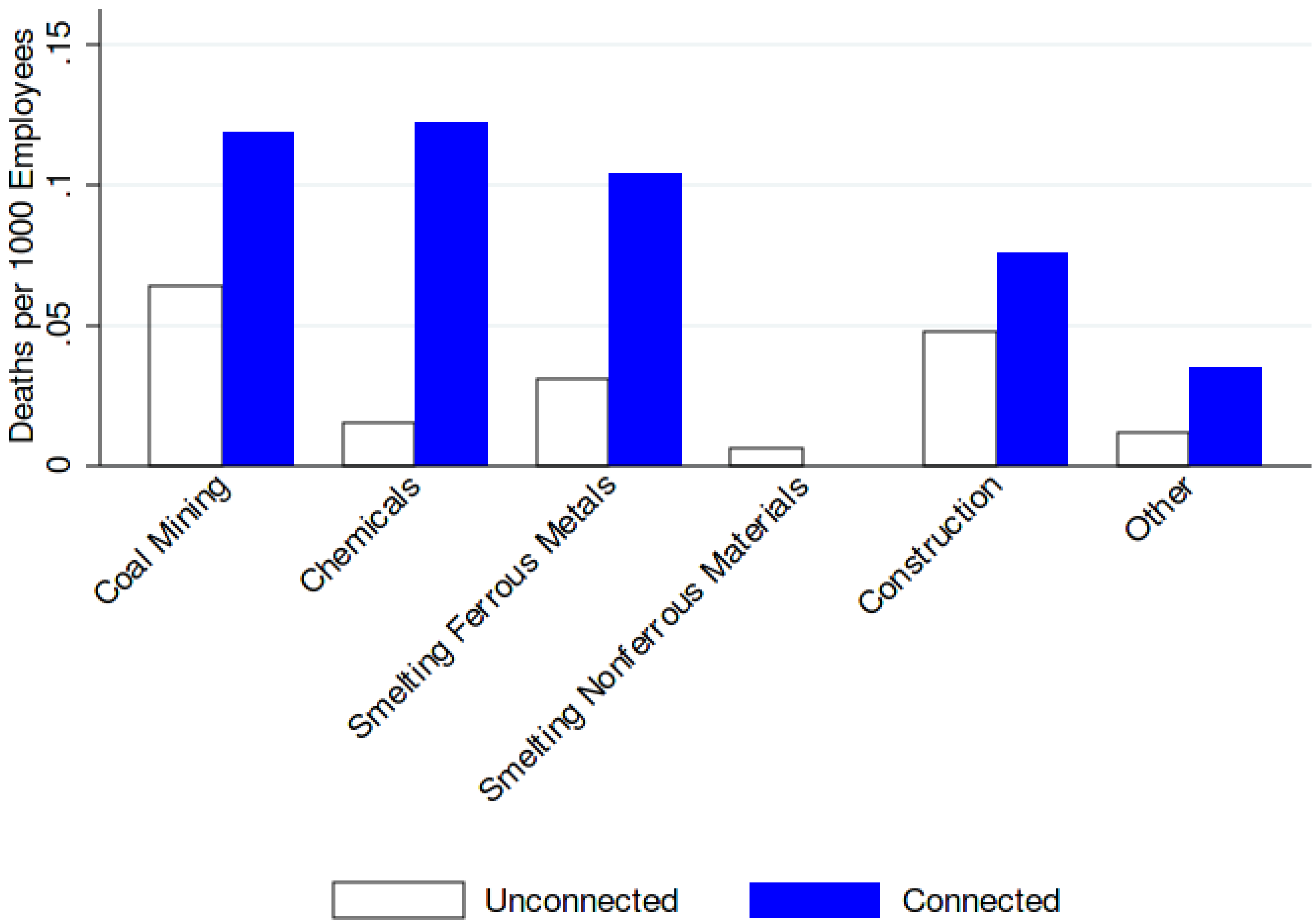
For the remaining 57 firm-year observations ($57 = 1475 - 193 - 1225$), across 16 firms, death information was not found in government websites or company reports. In these cases, we supplement our data using a media search based on the WiseSearch (www.wisers.com) database. These 57 firm-year observations contribute 11 deaths to our full-sample total of 1332, a reported death rate that is far lower than that of firm-year observations from official or company sources.

Figure 1: Frequency distribution of *Deaths*



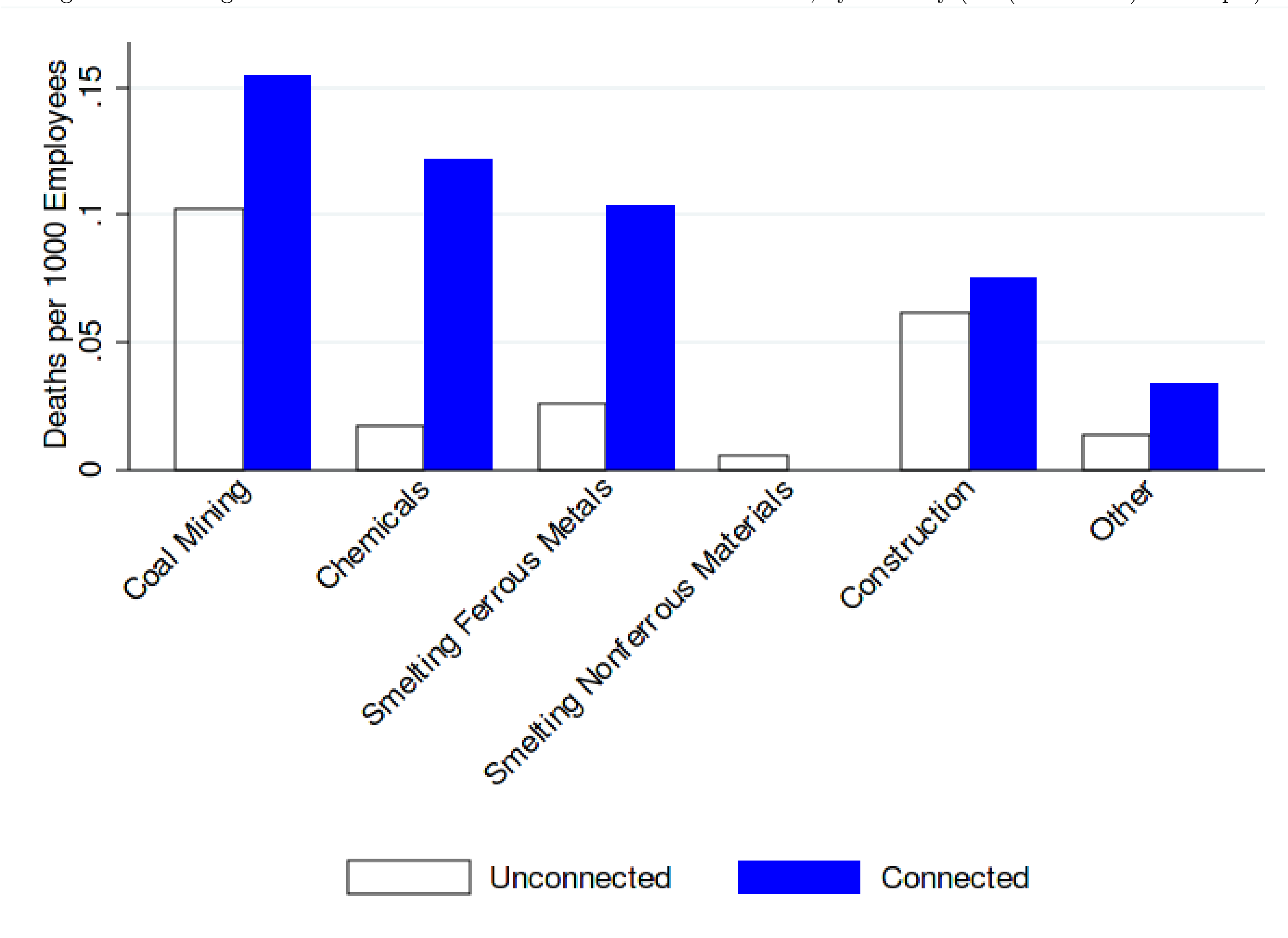
Notes: The sample used to construct this figure includes the 226 firm-year observations in which *Deaths* is positive. *Deaths* is firm-level worker fatalities per year. In this figure, the distribution of *Deaths* is censored at a maximum of 15.

Figure 2A: Average *DeathRate* at Connected versus Unconnected Firms, by Industry (full sample)



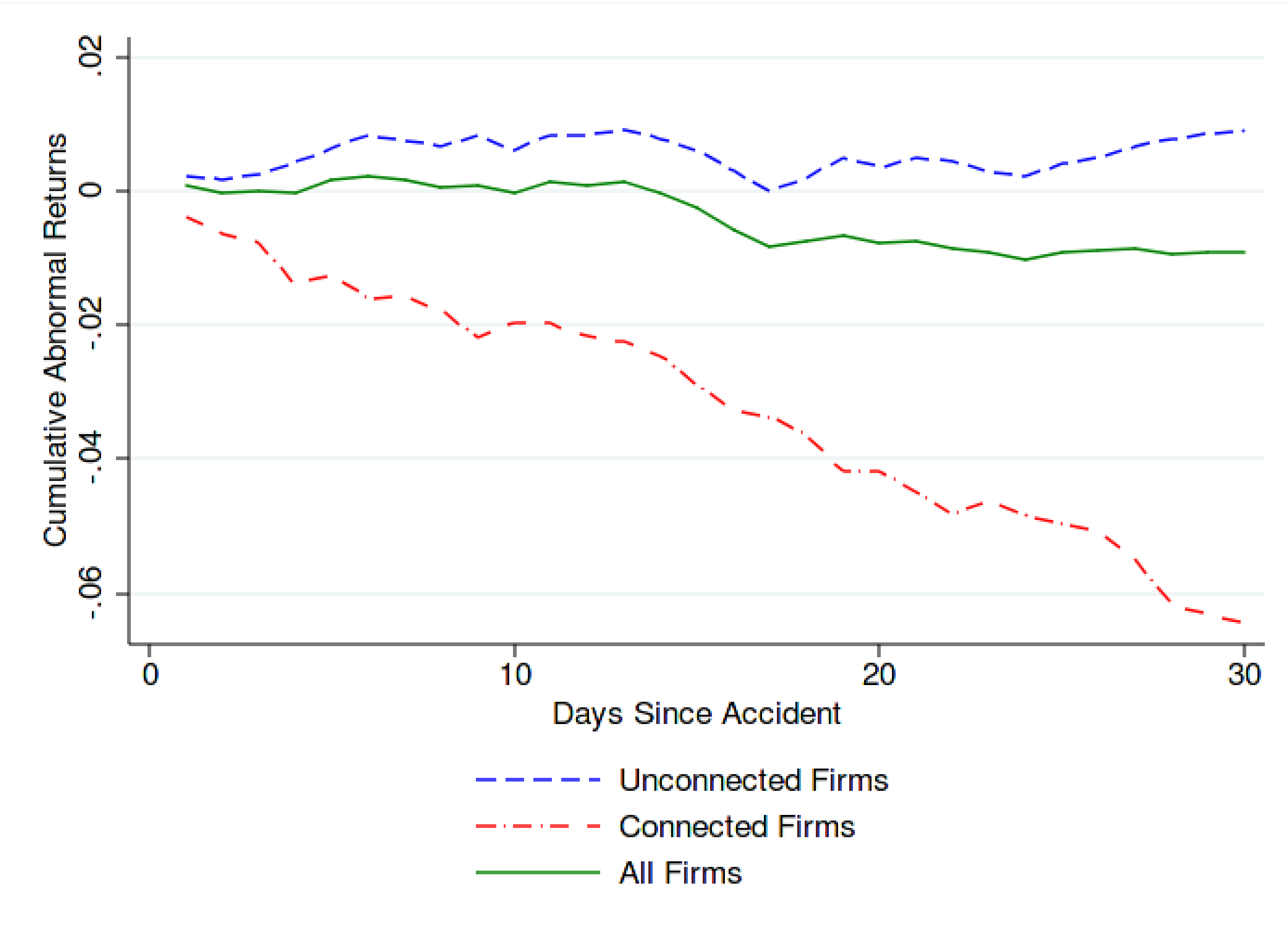
Notes: The figure is contructed using the full sample of 1475 firm-year observations. *DeathRate* is equal to 1000*(*Deaths/Employment*).

Figure 2B: Average *DeathRate* at Connected versus Unconnected Firms, by Industry (Var(Connected)>0 sample)



Notes: The figure is contructed using the Var(Connected)>0 sample of 325 firm-year observations. Var(Ct'd)>0 denotes that we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. *DeathRate* is equal to 1000*(*Deaths/Employment*).

Figure 3: Market reaction to fatal accidents, Connected versus Unconnected firms



Notes: Each data point represents the post-accident cumulative abnormal returns CARs[1,K], estimated using a standard Fama-French four-factor model, over the window of [1,K] where the accident date is set as 1. *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank.

Table 1A: Summary Statistics for firm-year panel data

Variable	Mean	SD	Median	Min	Max	Obs
log(1+Deaths)	0.211	0.612	0.000	0.000	5.094	1475
I(Deaths>0)	0.135	0.342	0.000	0.000	1.000	1475
DeathRate	0.029	0.090	0.000	0.000	0.366	1475
Severe	0.039	0.194	0.000	0.000	1.000	1475
Connected	0.087	0.283	0.000	0.000	1.000	1475
NSNP	0.411	0.492	0.000	0.000	1.000	1475
State Ownership	0.180	0.246	0.000	0.000	0.971	1475
log(Sales)	22.004	1.765	21.874	14.463	28.656	1475
CapInt	14.139	0.838	14.106	10.896	19.910	1475
ROA	0.039	0.066	0.033	-0.221	0.220	1475
log(Employment)	8.157	1.498	8.026	3.045	13.223	1475
Investment	0.078	0.065	0.065	-0.179	0.455	1475
SafetyViol	0.027	0.162	0.000	0.000	1.000	1475
Pollute	0.120	0.325	0.000	0.000	1.000	1388

Notes: All variables are at the firm-year level, and includes our full sample of 1475 firms. *Deaths* is firm-level worker fatalities per year; *I(Deaths>0)* is an indicator variable denoting at least one fatality in a firm-year observation; *DeathRate* is equal to 1000*(*Deaths/Employment*); *Severe* is an indicator variable denoting at least three fatalities from one accident in a firm-year observation; *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *StateOwnership* is the ratio of state shares to total outstanding shares; *CapInt* is log value of the ratio of total assets to total employees; *NSNP* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year; *SafetyViol* is an indicator variable denoting whether a firm was reported in the media for safety violations; *Pollute* indicates firm-year observations where the firm was fined for environmental violations.

Table 1B: Summary Statistics for firm-year panel data: politically connected vs. politically unconnected

Variable	PoliticallyConnected=0	PoliticallyConnected=1	Difference	(Std Error)
log(1+Deaths)	0.155	0.790	0.635***	(0.054)
I(Deaths>0)	0.110	0.395	0.285***	(0.031)
DeathRate	0.024	0.084	0.060***	(0.008)
Severe	0.027	0.163	0.135***	(0.018)
NSNP	0.417	0.349	-0.068	(0.045)
State Ownership	0.169	0.301	0.133***	(0.022)
log(Sales)	21.871	23.388	1.517***	(0.158)
CapInt	14.132	14.211	0.079	(0.077)
ROA	0.036	0.067	0.031***	(0.006)
log(Employment)	8.039	9.386	1.347***	(0.134)
Investment	0.077	0.091	0.014**	(0.006)
SafetyViol	0.022	0.078	0.055***	(0.015)
Pollute	0.119	0.123	0.004	(0.031)

Notes: All variables are at the firm-year level, and includes our full sample of 1475 firms. *Deaths* is firm-level worker fatalities per year; *I(Deaths>0)* is an indicator variable denoting at least one fatality in a firm-year observation; *DeathRate* is equal to 1000*(*Deaths/Employment*); *Severe* is an indicator variable denoting at least three fatalities from one accident in a firm-year observation; *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *StateOwnership* is the ratio of state shares to total outstanding shares; *CapInt* is log value of the ratio of total assets to total employees; *NSNP* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year; *SafetyViol* is an indicator variable denoting whether a firm was reported in the media for safety violations; *Pollute* indicates firm-year observations where the firm was fined for environmental violations.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 1C: Firm-year panel data: politically connected vs. politically unconnected firms, Var(Connected)>0 sample

Variable	PoliticallyConnected=0	PoliticallyConnected=1	Difference	(Std Error)
log(1+Deaths)	0.345	0.732	0.387***	(0.104)
I(Deaths>0)	0.224	0.373	0.148***	(0.053)
DeathRate	0.039	0.099	0.060***	(0.014)
Severe	0.049	0.176	0.127***	(0.033)
NSNP	0.422	0.441	0.020	(0.059)
State Ownership	0.220	0.287	0.066**	(0.032)
log(Sales)	22.629	22.772	0.143	(0.202)
CapInt	14.057	14.114	0.057	(0.116)
ROA	0.051	0.063	0.012*	(0.007)
log(Employment)	8.772	8.864	0.092	(0.184)
Investment	0.090	0.085	-0.005	(0.008)
SafetyViol	0.072	0.059	-0.013	(0.030)
Pollute	0.171	0.071	-0.099**	(0.042)

Notes: All variables are at the firm-year level, and includes the sample of firms where there is variation in *Connected* during our sample period (the Var(Connected)>0 sample), a total of 325 observations. *Deaths* is firm-level worker fatalities per year; *I(Deaths>0)* is an indicator variable denoting at least one fatality in a firm-year observation; *DeathRate* is equal to 1000*(*Deaths/Employment*); *Severe* is an indicator variable denoting at least three fatalities from one accident in a firm-year observation; *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *StateOwnership* is the ratio of state shares to total outstanding shares; *CapInt* is log value of the ratio of total assets to total employees; *NSNP* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year; *SafetyViol* is an indicator variable denoting whether a firm was reported in the media for safety violations; *Pollute* indicates firm-year observations where the firm was fined for environmental violations.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2: The impact of political connections on workplace deaths: negative binomial regression

	(1)	(2)	(3)	(4)	(5)	(6)
	Columns 1 - 6: Deaths					
Connected	1.953***	1.754***	1.697***	0.772***	1.521***	0.855***
	(0.393)	(0.318)	(0.328)	(0.212)	(0.398)	(0.222)
log(Sales)		0.665***	0.725***	0.134*	1.210***	0.329**
		(0.068)	(0.096)	(0.074)	(0.298)	(0.133)
ROA		4.626***	-1.259	0.267	1.514	2.731
		(1.675)	(2.094)	(1.709)	(5.451)	(2.936)
CapInt		-0.256**	0.093	-0.351**	-0.066	-0.372*
		(0.111)	(0.146)	(0.150)	(0.332)	(0.216)
StateOwnership		1.671***	1.238**	0.477	-0.401	0.680
		(0.429)	(0.588)	(0.368)	(1.027)	(0.610)
Investment		0.304	0.519	-2.610	3.551	-2.216
		(1.562)	(1.885)	(1.720)	(3.957)	(2.561)
Constant	-0.527***	-12.939***	-17.487***	0.210	-24.789***	-4.045
	(0.120)	(1.843)	(2.762)	(2.077)	(7.802)	(3.446)
ln(α)						
Constant	2.881***	2.278***	1.786***		1.278***	
	(0.090)	(0.099)	(0.103)		(0.153)	
Fixed Effects	-	-	Year; Industry; Province	Firm; Year	Year; Industry; Province	Firm; Year
Sample	Full	Full	Full	Full	Var(Ct'd)>0	Var(Ct'd)>0
Observations	1475	1475	1475	524	325	198
Log pseudolikelihood	-1095	-1014	-933	-506	-379	-223

Notes: All variables are at the firm-year level. *Deaths* is firm-level worker fatalities per year; *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *CapInt* is log value of the ratio of total assets to total employees; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares. Var(Ct'd)>0 denotes that we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. Robust standard errors are in parentheses; with the exception of columns 4 and 6 standard errors are clustered at the firm-level.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: The impact of political connections on workplace deaths: OLS regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Columns 1 - 3: log(1 + death)			Columns 4 - 6: DeathRate			Columns 7 - 9: I(Deaths> 0)		
Connected	0.282***	0.283**	0.370**	0.048***	0.050**	0.063***	0.109**	0.088	0.121**
	(0.106)	(0.138)	(0.150)	(0.017)	(0.020)	(0.021)	(0.049)	(0.056)	(0.060)
log(Sales)	0.096***	0.013	-0.181	0.006***	-0.009	-0.050**	0.052***	-0.046	-0.069
	(0.017)	(0.166)	(0.137)	(0.002)	(0.025)	(0.024)	(0.009)	(0.082)	(0.070)
ROA	-0.038	-0.318	1.555	-0.015	-0.075	0.219	-0.035	-0.192	0.808*
	(0.204)	(0.969)	(1.220)	(0.036)	(0.108)	(0.155)	(0.117)	(0.350)	(0.457)
CapInt	-0.064**	0.112	0.158	0.001	0.019	0.031	-0.038**	0.108	0.105**
	(0.030)	(0.134)	(0.109)	(0.004)	(0.020)	(0.022)	(0.016)	(0.068)	(0.050)
StateOwnership	0.149*	-0.007	0.363	0.013	-0.034	0.001	0.094**	-0.009	0.239**
	(0.083)	(0.213)	(0.248)	(0.012)	(0.026)	(0.034)	(0.047)	(0.115)	(0.113)
Investment	0.074	-0.556	0.484	-0.007	-0.127	0.008	0.051	-0.344	0.251
	(0.239)	(0.621)	(0.639)	(0.045)	(0.105)	(0.117)	(0.155)	(0.343)	(0.352)
Constant	-0.779*	-2.949	1.941	-0.074	-0.044	0.717	-0.302	-1.301	0.186
	(0.415)	(1.828)	(3.480)	(0.072)	(0.247)	(0.543)	(0.230)	(0.850)	(1.758)
Fixed Effects	Year; Industry; Province	Year; Industry; Province	Year; Firm	Year; Industry; Province	Year; Industry; Province	Year; Firm	Year; Industry; Province	Year; Industry; Province	Year; Firm
Sample	Full	Var(Ct'd)>0	Var(Ct'd)>0	Full	Var(Ct'd)>0	Var(Ct'd)>0	Full	Var(Ct'd)>0 0	Var(Ct'd)>0
Observations	1475	325	325	1475	325	325	1475	325	325
R-Squared	.28	.36	.493	.138	.322	.45	.268	.406	.512

Notes: All variables are at the firm-year level. *Deaths* is firm-level worker fatalities per year; *I(Deaths>0)* is an indicator variable denoting at least one fatality in a firm-year observation; *DeathRate* is equal to 1000*(*Deaths/Employment*); *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *CapInt* is log value of the ratio of total assets to total employees; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares. Var(Ct'd)>0 denotes that we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. Robust standard errors, clustered at the firm-level, are in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Crosstab of Connected and I(Deaths>0) for average safety violation rate (*SafetyViol*)

	Deaths=0	Deaths>0
Connected=0	0.046	0.160
Connected=1	0.000	0.158
Difference	0.046	0.002

Notes: Sample includes 325 firms with variation in *Connected* during 2008-2013; *Deaths* is firm-level worker fatalities per year; *I(Deaths>0)* is an indicator variable denoting at least one fatality in a firm-year observation; *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *SafetyViol* is an indicator variable denoting whether a firm was reported in the media for safety violations.

Table 5: The impact of political connections on environmental noncompliance

	(1)	(2)	(3)
	Columns 1 - 3: Pollute		
Connected	-0.099***	-0.093**	-0.071*
	(0.036)	(0.040)	(0.038)
log(Sales)		0.031*	0.007
		(0.017)	(0.018)
ROA		0.143	-0.152
		(0.439)	(0.276)
Investment		0.212	-0.130
		(0.332)	(0.257)
CapInt		-0.016	0.021
		(0.025)	(0.018)
StateOwnership		-0.115	0.199*
		(0.080)	(0.102)
Constant	0.171***	-0.315	-0.456
	(0.040)	(0.525)	(0.522)
Fixed Effects	-	-	Year; Industry; Province
Sample	Var(Ct'd)>0		
Observations	315	315	315
R-Squared	.0175	.0418	.398

Notes: All variables are at the firm-year level. *Pollute* indicates firm-year observations where the firm was fined for environmental violations; *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *CapInt* is log value of the ratio of total assets to total employees; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares. Var(Ct'd)>0 denotes that we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. Robust standard errors, clustered at the firm-level, are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Political connections, deaths and firm performance

	(1)	(2)	(3)	(4)
	Columns 1 - 2: ROA		Columns 3 - 4: ROA_F1	
Connected	0.017***	0.011*	0.015***	0.016**
	(0.006)	(0.006)	(0.006)	(0.007)
log(Sales)	0.004**	0.007	0.001	0.002
	(0.002)	(0.005)	(0.002)	(0.004)
Investment	0.146***	0.069	0.070**	0.029
	(0.035)	(0.074)	(0.030)	(0.044)
CapInt	0.008**	0.008	0.006**	0.007
	(0.004)	(0.010)	(0.003)	(0.007)
StateOwnership	-0.011	-0.001	-0.003	-0.020
	(0.009)	(0.027)	(0.008)	(0.017)
I(Deaths> 0)			0.005	0.005
			(0.004)	(0.008)
Connected * I(Deaths> 0)			-0.017**	-0.030***
			(0.008)	(0.010)
ROA			0.358***	0.155
			(0.057)	(0.095)
Constant	-0.106	-0.120	-0.050	0.004
	(0.065)	(0.160)	(0.050)	(0.139)
Fixed Effects	Year; Industry; Province			
Sample	Full	Var(Ct'd)>0	Full	Var(Ct'd)>0
Observations	1475	325	1199	268
R-Squared	.259	.477	.362	.601

Notes: All variables are at the firm-year level. *ROA_F1* refers to ROA in year t+1; *Deaths* is firm-level worker fatalities per year; *I(Deaths>0)* is an indicator variable denoting at least one fatality in a firm-year observation; *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *CapInt* is log value of the ratio of total assets to total employees; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares. Var(Ct'd)>0 denotes that we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. Robust standard errors, clustered at the firm-level, are in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: The impact of political connections on cumulative abnormal event returns (CARs): OLS regression

	(1) CARs[1,3]	(2) CARs[1,5]	(3) CARs[1,10]	(4) CARs[1,15]	(5) CARs[1,20]	(6) CARs[1,25]	(7) CARs[1,30]
Connected	-0.014**	-0.027***	-0.034*	-0.042**	-0.048*	-0.059*	-0.087**
	(0.007)	(0.010)	(0.018)	(0.018)	(0.027)	(0.032)	(0.036)
log(MonthlyDeaths)	-0.004	-0.003	-0.003	-0.007	-0.012	-0.008	-0.004
	(0.003)	(0.004)	(0.007)	(0.007)	(0.009)	(0.011)	(0.013)
log(Sales)	-0.002	-0.001	0.005	0.016**	0.009	0.012	0.008
	(0.005)	(0.007)	(0.007)	(0.007)	(0.011)	(0.014)	(0.014)
ROA	-0.059	-0.026	-0.147	-0.151	-0.185	-0.251	-0.196
	(0.083)	(0.107)	(0.121)	(0.128)	(0.193)	(0.225)	(0.240)
Constant	0.075	0.037	-0.109	-0.358**	-0.202	-0.279	-0.184
	(0.113)	(0.152)	(0.171)	(0.179)	(0.271)	(0.328)	(0.350)
Fixed Effects	Province, Industry & Year						
Observations	210	210	210	210	210	210	210
Adjusted R-Squared	.0671	.0946	.0649	.0635	.0752	.0362	.062

Notes: A standard OLS model is used to estimate the impact of political connections on CARs. CARs[1,K] is the cumulative abnormal event return, estimated using a standard Fama-French-Carhart four-factor model, over the window of [1,K] where the accident day is set as 1; *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; log(*MonthlyDeaths*) is the log value of the total deaths that take place within a month of each initial accident. Robust standard errors, clustered at the firm-level, are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: The effect of workplace deaths on political connections

	(1)	(2)	(3)
	Columns 1 - 3: Connected_F1		
I(Deaths> 0)	-0.073***	-0.248***	-0.244***
	(0.024)	(0.064)	(0.078)
Connected	0.401***	0.103*	0.046
	(0.049)	(0.057)	(0.064)
StateOwnership	0.047	0.310**	0.328
	(0.041)	(0.147)	(0.210)
log(Sales)	0.009	-0.047	-0.177
	(0.007)	(0.030)	(0.151)
CapInt	-0.008	-0.048	-0.157
	(0.012)	(0.039)	(0.118)
ROA	0.128	0.566	0.194
	(0.095)	(0.635)	(0.928)
Investment	0.137	-0.171	-0.285
	(0.123)	(0.507)	(0.586)
Constant	0.037	1.978***	6.434**
	(0.168)	(0.731)	(2.939)
Fixed Effects	Year; Industry; Province	Year; Industry; Province	Firm
Sample	Full	Var(Ct'd)>0	Var(Ct'd)>0
Observations	1199	268	268
R-Squared	.351	.244	.332

Notes: All variables are at the firm-year level. *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *Connected_F1* denotes *Connected* in year t+1; *Deaths* is firm-level worker fatalities per year; *I(Deaths>0)* is an indicator variable denoting at least one fatality in a firm-year observation; *CapInt* is log value of the ratio of total assets to total employees; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares. Var(Ct'd)>0 denotes that we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. Robust standard errors, clustered at the firm-level, are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Crosstab of Connected and NSNP for Deathrate

	All Firms	Unconnected	Connected
NSNP=0	0.035	0.025	0.166
NSNP=1	0.018	0.017	0.027
Difference		0.008	0.139

Notes: Sample includes the 564 observations from firms headquartered in provinces that implemented an NSNP law during 2009-2012; *Deaths* is firm-level worker fatalities per year; *DeathRate* is equal to 1000*(*Deaths/Employment*); *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *NSNP* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year.

Table 10: The differential impacts of "No Safety, No Promotion" on firm-level outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Columns 1 - 3: DeathRate			Columns 4 - 6: ROA			Columns 7 - 9: Pollute		
Connected	0.079***	0.093***	0.104***	0.013*	0.011	0.009	0.020	0.002	0.015
	(0.024)	(0.027)	(0.028)	(0.007)	(0.008)	(0.010)	(0.049)	(0.048)	(0.056)
NSNP	0.000	0.012	0.010	-0.004	-0.005	-0.006	0.008	0.047	0.025
	(0.008)	(0.028)	(0.029)	(0.006)	(0.011)	(0.013)	(0.031)	(0.079)	(0.083)
Connected * NSNP	-0.087***	-0.113***	-0.110***	0.010	0.001	-0.001	-0.150**	-0.189**	-0.143
	(0.029)	(0.033)	(0.034)	(0.012)	(0.011)	(0.013)	(0.066)	(0.086)	(0.091)
log(Sales)	0.005***	0.007	-0.046*	0.004**	0.007	0.035	0.039***	0.006	0.016
	(0.002)	(0.006)	(0.024)	(0.002)	(0.005)	(0.024)	(0.009)	(0.017)	(0.074)
CapInt	0.001	0.000	0.027	0.008**	0.008	-0.022	0.002	0.018	0.009
	(0.004)	(0.011)	(0.020)	(0.004)	(0.010)	(0.019)	(0.014)	(0.017)	(0.035)
StateOwnership	0.012	-0.032	0.001	-0.011	-0.001	-0.010	-0.063	0.190*	0.039
	(0.012)	(0.027)	(0.031)	(0.009)	(0.028)	(0.025)	(0.054)	(0.099)	(0.115)
ROA	-0.012	-0.082	0.209				-0.074	-0.141	-0.051
	(0.035)	(0.111)	(0.146)				(0.143)	(0.281)	(0.414)
Investment	-0.007	-0.113	0.005	0.146***	0.068	0.022	0.085	-0.133	-0.049
	(0.045)	(0.101)	(0.123)	(0.035)	(0.075)	(0.081)	(0.135)	(0.249)	(0.290)
Constant	-0.065	0.010	0.686	-0.107*	-0.116	-0.396	-0.761***	-0.421	-0.419
	(0.071)	(0.249)	(0.552)	(0.065)	(0.162)	(0.496)	(0.279)	(0.502)	(1.443)
Fixed Effects	Year; Industry; Province	Year; Industry; Province	Year; Firm Firm	Year; Industry; Province	Year; Industry; Province	Year; Firm Firm	Year; Industry; Province	Year; Industry; Province	Year; Firm Firm
Sample	Full	Var(Ct'd)>0	Var(Ct'd)>0	Full	Var(Ct'd)>0	Var(Ct'd)>0	Full	Var(Ct'd)>0	Var(Ct'd)>0
Observations	1475	325	325	1475	325	325	1388	315	315
R-Squared	.152	.361	.486	.26	.477	.623	.152	.412	.524

Notes: All variables are at the firm-year level. *Deaths* is firm-level worker fatalities per year; *DeathRate* is equal to 1000*(*Deaths/Employment*); *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *CapInt* is log value of the ratio of total assets to total employees; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares; *NSNP* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year; *Pollute* indicates firm-year observations where the firm was fined for environmental violations; Var(Ct'd)>0 denotes that we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. Robust standard errors, clustered at the firm-level, are in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: The impact of connections on severe accident rates: A test of under-reporting

	(1)	(2)	(3)
	Columns 1 - 3: Severe		
Connected	0.100***	0.144***	0.159***
	(0.038)	(0.052)	(0.057)
log(Sales)	0.020***	0.040***	-0.032
	(0.004)	(0.012)	(0.059)
ROA	0.027	-0.167	0.270
	(0.053)	(0.235)	(0.361)
Investment	0.006	0.055	0.253
	(0.072)	(0.281)	(0.350)
CapInt	-0.011	-0.026	0.019
	(0.008)	(0.022)	(0.052)
State Ownership	-0.005	-0.043	-0.006
	(0.025)	(0.064)	(0.081)
Constant	-0.232**	-0.364	0.487
	(0.111)	(0.463)	(1.519)
Fixed Effects	Year; Industry; Province	Year; Industry; Province	Year; Firm
Sample	Full	Var(Ct'd)>0	Var(Ct'd)>0
Observations	1475	325	325
R-Squared	.1	.186	.248

Notes: All variables are at the firm-year level. *Severe* is an indicator variable denoting at least three fatalities from one accident in a firm-year observation; *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *CapInt* is log value of the ratio of total assets to total employees; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares. Var(Ct'd)>0 denotes that we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. Robust standard errors, clustered at the firm-level, are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table A1: Instatement of "no safety, no promotion" laws by province

Province	Effective Date	Passage Date	Deaths	Firm-year obs	Population in 2005 (unit: 10,000)	GDP per capita in 2005 (unit: 1 RMB)
Guangdong	01-Feb-2005	01-Feb-2005	10	122	9194	24435.02
Heilongjiang	01-Jan-2007	28-Nov-2006	13	12	3820	14434.06
Guizhou	01-Jun-2006	01-Jun-2006	68	18	3730	5051.96
Ningxia	01-Nov-2006	27-Sep-2006	2	12	596	10239
Shanxi	29-Jan-2008	29-Jan-2008	246	63	3355	12495
Yunnan	18-Sep-2008	18-Sep-2008	19	51	4450	7835
Guangxi	01-Jan-2009	27-Dec-2008	5	24	4660	8787.729
Jilin	20-Jul-2009	08-Jan-2009	0	18	2716	13348
Hainan	02-Jun-2010	02-Jun-2010	0	6	828	10871
Jiangsu	23-Nov-2010	23-Nov-2010	15	104	7475	24560
Hebei	01-Jan-2011	08-Nov-2010	43	49	6851	14782.26
Henan	03-Jan-2011	03-Jan-2011	76	39	9380	11346.5
Hunan	22-Mar-2011	22-Mar-2011	2	54	6326	10426
Inner Mongolia	04-Mar-2011	04-Mar-2011	18	48	2386	16330.82
Sichuan	11-Apr-2011	11-Apr-2011	49	88	8212	9060
Chongqing	04-Jul-2011	04-Jul-2011	27	20	2798	10982
Jiangxi	03-May-2012	03-May-2012	7	18	4311	9440
Liaoning	01-Feb-2012	30-Nov-2011	23	54	4221	18983.2
Shandong	06-Apr-2012	06-Apr-2012	78	107	9248	20096.46
Shaanxi	04-Jan-2012	04-Jan-2012	0	29	3720	9899
Hubei	No safety quota	No safety quota	57	56	5710	11431
Qinghai	No safety quota	No safety quota	16	40	543	10044.74
Shanghai	No safety quota	No safety quota	43	57	1778	51474
Tianjin	No safety quota	No safety quota	2	6	1043	35783.19
Tibet	No safety quota	No safety quota	0	6	277	9114
Xinjiang	No safety quota	No safety quota	19	43	2010	13108
Zhejiang	No safety quota	No safety quota	20	108	4898	27702.69
Anhui	No safety quota	No safety quota	31	74	6120	8675.145
Beijing	No safety quota	No safety quota	410	90	1538	45443.69
Fujian	No safety quota	No safety quota	26	35	3535	18645.84
Gansu	No safety quota	No safety quota	7	24	2594	7476.529

Notes: Since 2004, China has followed a "safety production quota" system, whereby the central government assigns a "death quota" to each province, and the provincial government allocates the quota among municipalities. In response, provinces began adopting "no safety, no promotion" policies that made promotion of safety regulators and other local government officials contingent on meeting the safety production target set for their region by the provincial government. Guodoing was the first province to adopt this policy in 2005, while others followed suit only in recent years. Deaths is the number of worker deaths among firms in our sample. Firm-year obs is the number of observations that firms based in each province contribute to our sample. Population is province population in 2005. GDP P.C. is income per capita in 2005. Both population and income figures are obtained from CSMAR Regioual Economy Database.

Appendix Table A2: Propensity Score Tests

Dependent Variable	Matching Method	Number of Treatment Firms	Number of Control Firms	ATT	Std. Err.
DeathRate	Nearest-Neighbor Matching	129	91	0.292	0.129
DeathRate	Kernel Matching	129	1165	0.326	0.131
DeathRate	Radius Matching	123	1165	0.338	0.129

Notes: Stata codes come from Sascha Becker and Andrea Ichino (2002). ATT stands for "Average Treatment Effect on the Treated". Bootstrap replication number is set at 100 in each case. A logit model is used in the propensity score test, where firms are matched on these dimensions: log(Sales), ROA, log(Employment), Investment and SIC-1 digit industry. The Epanechinikov kernel is used in the kernel matching where the bandwidth is set at 0.04, smaller than the default value of 0.06.

Appendix Table A3: The effect of political connections on worker deaths, industry time trends included

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Columns 1 - 3: log(1 + death)			Columns 4 - 6: DeathRate			Columns 7 - 9: I(Deaths> 0)		
Connected	0.278***	0.285**	0.361**	0.047***	0.050**	0.062***	0.108**	0.093*	0.120**
	(0.105)	(0.138)	(0.153)	(0.016)	(0.020)	(0.021)	(0.047)	(0.054)	(0.058)
log(Sales)	0.102***	0.084	0.009	0.007***	0.001	-0.021	0.056***	-0.005	0.050
	(0.018)	(0.167)	(0.151)	(0.002)	(0.024)	(0.020)	(0.009)	(0.079)	(0.086)
ROA	-0.024	-0.157	1.713	-0.014	-0.062	0.232	-0.050	-0.164	0.809*
	(0.194)	(0.917)	(1.224)	(0.035)	(0.100)	(0.151)	(0.114)	(0.307)	(0.469)
CapInt	-0.062**	0.072	0.150	0.002	0.014	0.032	-0.037**	0.086	0.105*
	(0.030)	(0.137)	(0.106)	(0.004)	(0.019)	(0.021)	(0.016)	(0.065)	(0.056)
StateOwnership	0.151*	-0.084	0.207	0.011	-0.046	-0.021	0.070	-0.055	0.147
	(0.085)	(0.229)	(0.266)	(0.012)	(0.029)	(0.036)	(0.047)	(0.117)	(0.110)
Constant	-0.613	-3.236*	-237.989	-0.054	-0.070	-45.332**	-0.184	-1.365	-211.836***
	(0.433)	(1.897)	(166.483)	(0.075)	(0.254)	(19.176)	(0.232)	(0.868)	(57.131)
Fixed Effects	Year; Industry; Province	Year; Industry; Province	Year; Firm	Year; Industry; Province	Year; Industry; Province	Year; Firm	Year; Industry; Province	Year; Industry; Province	Year; Firm
Sample	Full	Var(Ct'd)>0	Var(Ct'd)>0	Full	Var(Ct'd)>0	Var(Ct'd)>0	Full	Var(Ct'd)>0	Var(Ct'd)>0
Observations	1475	325	325	1475	325	325	1475	325	325
R-Squared	.307	.4	.525	.157	.36	.482	.294	.461	.563

Notes: All variables are at the firm-year level. All specifications include industry-specific time trends. *Deaths* is firm-level worker fatalities per year; *DeathRate* is equal to 1000*(*Deaths/Employment*); *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *CapInt* is log value of the ratio of total assets to total employees; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares. *NSNP* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year; *Pollute* indicates firm-year observations where the firm was fined for environmental violations. Var(Ct'd)>0 denotes that we limit the sample to firms where there is variation in *Connected* over the sample period 2008-2013, due either to the arrival or departure of a connected executive. Robust standard errors, clustered at the firm-level, are in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table A4: The differential impacts of "No Safety, No Promotion": Robustness

	(1)	(2)	(3)	(4)	(5)
	Columns 1 - 5: DeathRate				
Connected	0.131***	0.097***	0.098***	0.093***	0.772**
	(0.044)	(0.028)	(0.032)	(0.027)	(0.316)
NSNP	0.080	0.050	0.000	-0.033	0.017
	(0.052)	(0.043)	(.)	(0.215)	(0.027)
Connected * NSNP	-0.157***	-0.132***	-0.151***	-0.114***	-0.127***
	(0.049)	(0.036)	(0.048)	(0.033)	(0.034)
log(Sales)	0.012	0.008	0.010	0.006	0.006
	(0.007)	(0.006)	(0.008)	(0.008)	(0.006)
CapInt	0.045**	0.004	0.009	-0.000	0.002
	(0.017)	(0.011)	(0.014)	(0.012)	(0.011)
StateOwnership	-0.082**	-0.061**	-0.055	-0.033	-0.034
	(0.037)	(0.026)	(0.041)	(0.027)	(0.026)
ROA	-0.023	-0.199**	-0.386*	-0.082	-0.077
	(0.128)	(0.099)	(0.203)	(0.111)	(0.110)
Investment	-0.133	-0.121	-0.191	-0.116	-0.113
	(0.137)	(0.118)	(0.158)	(0.102)	(0.100)
log(Sales) * NSNP				0.002	
				(0.010)	
Connected * log(GDP per Capita)					-0.065**
					(0.030)
Constant	-0.764***	159.468***	-0.141	0.025	-0.006
	(0.246)	(10.050)	(0.323)	(0.289)	(0.246)
Fixed Effects	Year; Industry; Province	Year; Industry; Province	Province X Year; Industry	Year; Indsutry; Province	Year; Industry; Province
Sample	Var(Ct'd)>0; Var(NSNP)>0	Var(Ct'd)>0	Var(Ct'd)>0	Var(Ct'd)>0	Var(Ct'd)>0
Observations	143	325	325	325	325
R-Squared	.38	.46	.668	.361	.379

Notes: All variables are at the firm-year level. *Deaths* is firm-level worker fatalities per year; *DeathRate* is equal to 1000*(*Deaths/Employment*); *Connected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor in the city ("Tingju Ji" in Chinese) where the firm is located or as a provincial or central government official of the same or higher rank; *CapInt* is log value of the ratio of total assets to total employees Robust standard errors, clustered at the firm-level, are in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.