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. The worker death rate for connected companies is five times that of unconnected firms; this result also holds when we exploit executive turnover to generate within-firm estimates. The connections-mortality relationship is attenuated in provinces where officials' promotion is contingent on meeting safety quotas. Fatal accidents produce negative returns at connected companies and are associated with the subsequent departure of well-connected executives. Our findings emphasize the social costs, as well as the firm-level benefits, of political connections.

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There exists a longstanding debate on the social impact of corruption. The effects hinge to some degree on whether political ties allow businesses to circumvent onerous bureaucratic constraints or force them to comply with regulations that are socially beneficial but result in lower firm profits. Yet there is little empirical evidence on the costs and benefits of corruption. In this paper, we examine the role of corruption in workplace fatalities in China, thus highlighting the cost side of this tradeoff.

There have been many journalistic accounts of substandard working conditions in Chinese industries ranging from electronics component production (as with the recent exposé of Apple's supplier Foxconn) to the coal industry, which is responsible for thousands of mining deaths each year. In 2003, the worker mortality rate per ton of coal extracted in Chinese mines was 100 times higher than that of American mines and thirty times higher than that of South African mines, which plausibly have similar labor conditions.[1] Media reports suggest that corruption - i.e., business-government ties for the benefit of corporate profits, contribute to the high mortality rate of Chinese coal miners. In 2005, the national minister of production safety, Li Yizhong, directly attributed the sector's poor safety record to corruption, saying, "Corruption is one major reason why accidents happen again and again…It is high time that we took a careful look at connections between local coal mine owners, local officials and the safety watchdog. These links have set up barriers against strict safety supervision."[2] By the end of 2011, in the coal mining industry alone, nearly 3000 government officials had been arrested for taking bribes or otherwise accepting favors from the companies they were meant to regulate.[3]

In this paper, we present three sets of empirical results. First, we document a positive relationship between political connections and worker mortality; second, we study how this relationship is affected by performance incentives for safety regulators; finally, we examine the implications of worker deaths for expected firm profits and in particular, how the impact on a firm's share price depends on its political ties.

We begin by showing a robust positive correlation between the political ties of executives and worker fatalities at publicly traded Chinese companies. We define a company to be politically connected if its CEO or another "C-suite" executive previously held a high-level government post, and we find that the rate of workplace deaths at connected firms is five times higher than at unconnected firms (1 versus 5 deaths per 1,000 workers annually) from 2008 to 2011. This pattern holds in regression specifications, both in the cross-section and also in a firm-year panel with fixed effects, where the connection-fatality relationship is identified from within-firm executive turnover. We consider several alternative hypotheses that could account for this pattern - such as differential rates of misreporting for connected versus

[1] "Coal mining: Most deadly job in China," *China Daily*, November 13, 2004.
[2] "Coal miners pay with their lives for cronyism in China," *Hindustan Times*, August 11, 2005.
[3] "Vice-CEO of Lu'an Group is investigated," *Daily Economic News,* Jan, 20, 2012.

unconnected firms or differences in management talent - and also whether the selection of connected executives could bias our results. We argue that our full set of results are best explained by the use of political relationships to circumvent safety oversight and regulation and that selection concerns are unlikely to be a dominant factor in explaining our findings.

We then go on to explore the effect of regulatory incentives on worker deaths. We find that worker mortality is lower in provinces where officials' promotion is contingent on meeting safety quotas. More importantly, we find that this effect is exclusively the result of lower mortality rates for connected companies, suggesting that government officials will be less prone to ignoring regulatory violations at well-connected firms when high-powered safety incentives are imposed on regulators.[4]

Finally, we analyze the implications of worker deaths for firm share prices, and how this relationship differs as a function of political connections. This is of particular interest as investor responses to worker deaths can help to adjudicate among potential mechanisms through which political ties affect worker safety. We perform an event study analysis of the 239 accidents where we are able to determine the precise accident date. While we find no significant impact of workplace fatalities on share price in the overall sample, event returns are negative for politically connected firms. Our analysis - while noisily estimated - suggests cumulative abnormal returns of -4.7 percent for connected firms relative to unconnected ones in the week following an accident, with the effect doubling to -9.7 percent over a 30 day trading window.[5] Investors' expectations are validated by subsequent changes in operating profits: in the year following an accident, return on assets is lower for well-connected firms, and largely unchanged for the rest of the sample. Further, a fatal accident predicts the departure of political connected executives by the end of the calendar year that the accident occurred, indicating that the expected loss of political connections - and their accompanying benefits - play a role in the negative market reaction.

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

[4]We note that the shortness of our panel - and its lack of overlap with most province-level changes in regulation - implies that these results are identified from cross-sectional differences in regulation across provinces and hence should be interpreted with some caution.

[5]The slow market response is due to the fact that worker deaths are generally not announced to the public immediately after they occur.

directly at the welfare consequences of regulation; further, we focus on high-level connections rather than petty corruption.

More broadly, our study contributes to the literature on the social costs and benefits of corruption. 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. 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. 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 (see, in particular, Fisman (2001) and Faccio (2006)). While prior research has established the implications of political ties for firm profits, our study illustrates the social costs. Finally, 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.

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 following: Section 1 describes the data and institutional background, Section 2 presents the main results and robustness checks, and Section 3 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.[6] 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, nationalwide 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.

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,

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

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.[7] 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.

To take advantage of local differences in the extent of safety enforcement, we analyze the effects of one significant shift in the safety incentives of local authorities. Prior to 2004, provincial officials were evaluated and promoted based on local economic growth (Li and Zhou, 2005). 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 is responsible for allocating the quota among municipalities (Peng, 2004). Province-level safety performance was widely publicized through quarterly reports in a national newspaper, the *People's Daily*. 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 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 recently. As of mid-2012, 11 provincial authorities, out of a total of 31, still had not adopted this approach to providing incentives to safety regulators.[8] Table 1 lists, for each province, the dates of the passage and implementation of "no safety, no promotion" policies. In unreported analyses, we implemented a Cox proportional hazard model to predict the passage of these laws. 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.

We find that the fraction of provincial GDP from coal mining is a strong positive predictor of "no safety, no promotion" laws, but after this is accounted for, no other province-level attribute (including construction output, log income per capita, population, 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. Thus, while relatively few provinces passed "no safety, no promotion" laws during our sample frame - and hence our identification is from the cross-section - there are no immediately obvious correlates of the

[7]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.

[8]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.

policy's passage that would undermine our identification.

1.2 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); Construction (E01, 35 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). 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 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 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 the State Administration of Work Safety 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 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 2008-2011.

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

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

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 49 firm-year observations from 13 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 49 firm-year observations contribute 4 deaths to our full-sample total of 1000, a reported death rate that is far lower than that of firm-year observations from official or company sources.

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

Our final dataset is comprised of 933 firm-year observations, with a total of 1000 worker fatalities. Given the sample average of 3442 employees per firm, this represents an annual worker mortality rate that is more than three times the national average of one in ten thousand - a natural result of our focus on high mortality industries. However, the death rate is plausibly far higher in these industries for privately owned companies, which we are unable to include in our sample owing to data availability.

We define four measures of worker mortality: $Deaths_{cy}$, total worker fatalities at company c during calendar year y; $DeathRate_{cy} = Deaths_{cy}/Employment_{cy}$, a measure of deaths 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, $I(Deaths_{cy} \geq 3)$.

In 239 accidents that led to a total of 789 deaths (out of the total of 1000 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 239 accidents with precise event dates in our market reaction analysis below.

Our measure of political connections is based on the previous employment history 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, 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, $PoliticallyConnected_{cy}$, 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 $PoliticallyConnected$, making it likely that our results are biased 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 that we may attribute to his employment at the firm.

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.

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 both total assets and employment, we use figures from the beginning of year y, while we employ lagged values for the flow variables ROA and investment.

Finally, we define $SafetyQuota_{py}$ to be a province-year variable denoting whether province p had passed a "no safety, no promotion" law in year y. Since many such laws went into effect mid-year, we define $SafetyQuota_{py}$ as the fraction of days during the year of passage when

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

the law was in effect. (For example, in Hainan province the law went into effect on June 2, 2010, the 153rd day of the year; thus for 2010, $SafetyQuota_{py} = (365 - 153)/365.$)

Table 2A presents the summary statistics for the firm-year panel data. We further show these summary statistics for the sample split into politically connected and unconnected firm-year observations in Table 2B. Of particular note, politically connected firms have, on average, 0.52 worker deaths per 1,000 employees each year, whereas the comparable figure for unconnected firms is 0.09 deaths per 1,000 employees. Politically connected firms are also larger, as measured by employees or assets, which will make it crucial to control carefully for size in the analysis that follows. State ownership is also correlated with political connections - the mean level of state ownership is 61 percent higher for $PoliticallyConnected = 1$ firms. Finally, connected firms are more profitable, as measured by return on assets. This is consistent with the prior literature on the value of political ties; while the raw difference in means is not statistically significant, we will examine profit differences in greater detail below, controlling for basic firm-year characteristics like province and industry.

2 Results

We begin with a graphical overview of our main results. In Figure 1, we split the sample based on connections in year y, $PoliticallyConnected_{cy}$, and show the average of deaths per employee, $DeathRate_{cy}$. We observe that the average number of worker fatalities is nearly 3 times higher at $PoliticallyConnected = 1$ firms in 2008, and in other years the difference is even greater.

One concern with assigning a causal interpretation to the cross-sectional relationship between $PoliticallyConnected$ and worker fatalities is that companies may hire well-connected executives precisely because they work in particularly hazardous locations or sub-industries, and a well-connected executive can help them circumvent costly safety protocols. While it is possible that workplace hazards vary at high frequency within a given company, to some degree these concerns may be alleviated by examining within-firm shifts in political connections and the resultant change in workplace deaths. In a number of cases, we observe within-firm shifts in political connectedness that allow for a pre-post comparison of workplace safety as a function of the arrival or departure of executives that previously held high-level government posts. In Table 3, we show the change in various measures of worker mortality in the year after a firm experiences a change in political connections, separately listing those changes associated with a politically connected executive's departure (column (1)) or arrival (column (2)). We find a symmetric change in worker deaths from the arrival and departure of former government officials. For example, deaths per 1,000 employees increase by nearly one with the arrival of a politically connected executive, while the death rate declines by 0.64

with a connected executive's departure. We also list the change in log employment, which is 0.17 in each case, suggesting that firms' growth trajectories are uncorrelated with changes in *PoliticallyConnected*.[11]

The relatively rapid and material impact of political connections of top executives on worker fatalities is in line with the literature on the effects of CEOs on firm-level outcomes. Bertrand and Schoar (2003) 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. Indeed, Morten Bennedsen, Pérez-González, Nielsen and Wolfenzon (2007) 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. The context we consider here offers a plausible channel for rapid change - given the rampant corruption in safety oversight, a political connection could result in a discrete shift in a firm's ability to skirt regulation.

We now examine the relationship between *PoliticallyConnected* and worker fatalities in a regression framework. Our baseline specification is as follows:

$$\begin{aligned} Deaths_{cy} = \; & \beta_0 + \beta_1 \times PoliticallyConnected_{cy} \\ & + \beta_2 \times StateOwnership_{cy} \\ & + \beta_3 \times log(Employment_{cy}) + \beta_4 \times ROA_{cy-1} \\ & + \beta_5 \times log(Assets)_{cy} + \beta_6 \times Investment_{cy-1} + \varepsilon_{cy} \end{aligned} \quad (1)$$

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, and will use a monotonic transformation of *Deaths* as the dependent variable.

Before turning to standard OLS specifications, we first show our main results in Table 4 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 company, except as noted below when we exploit province-level variation in studying the effects of safety regulations. We begin with a specification with no covariates. The coefficient on *PoliticallyConnected* is very large in magnitude - its coefficient of 1.66 implies a death incidence rate for connected companies that is more than 500 percent ($e^{1.66}$) of the baseline, significant at the 1 percent level. Adding basic controls for size (both assets and employment) and investment increases the coefficient on *PoliticallyConnected*, which is again significant at the 1 percent level. When we include fixed effects for year, province, and industry, the coefficient on *PoliticallyConnected* increases to 2.06, implying a

[11] The full sample mean of employment growth is 0.08, suggesting that *changes* in political connections are associated with higher growth companies.

death incidence rate of about 800 percent ($e^{2.06}$) of the baseline. Finally, it is noteworthy that the coefficient on state ownership is positive (though generally not statistically significant) - the opposite of what one might anticipate if state ownership produced greater emphasis on safety and other objectives beyond profit maximization.

We next present a set of OLS specifications in Table 5, first using $log(1 + Deaths)$ as the dependent variable. In column (1) we present results using the full set of controls in Equation (1), as well as industry, year, and province fixed effects. The coefficient of 0.40 is signficant at the 1 percent level. As expected, firm size as proxied by number of employees is also predictive of worker fatalities. When we include firm fixed effects in column (2), the coefficient on *PoliticallyConnected* is identified from within-firm variation in political connections; that is, the 49 firms (187 firm-year observations) where a political connection is gained or lost during 2008-2011. *PoliticallyConnected's* coefficient increases to 0.70, significant at the 1 percent level; firm size is no longer a significant predictor of worker deaths.

In columns (3) and (4), 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 *PoliticallyConnected* is 0.18 in (3), and 0.27 in (4), and it indicates the increased probability of at least one worker death at firms with a connected executive, relative to unconnected firms. For unconnected firms, the fraction of firm-year observations with $I(Deaths_{cy} > 0)$ is 0.13, implying that political ties are associated with a more than doubling of the probability of a worker death.

While we have controlled for firm size in all specifications, in columns (5) and (6) we scale worker fatalities by the number of employees at a firm, using *DeathRate* as the dependent variable. The coefficient on *DeathRate* in (5) is 0.47, significant at the 1 percent level, implying one additional workplace fatality in a given year for each 2,000 employees at politically connected firms. The coefficient increases to 0.64 in column (6), which includes firm fixed effects.

As we noted in the prior section, $PoliticallyConnected = 1$ firms are substantially larger than their unconnected counterparts, raising concerns that the latter group may not serve as appropriate controls. While the firm fixed-effects analyses deal with this concern to some degree, we also employ propensity score matching to examine whether our results are robust to this alternative approach of matching on observables. We report these results in the Appendix, using a range of matching methods. In all cases, the point estimates are significant at the 1 percent level and are generally larger in magnitude than the OLS results.

In our analysis, we have implicitly assumed honest corporate disclosures of worker deaths. However, as indicated by some high-profile examples of attempted cover-ups, companies may try to avoid disclosing accidents that could invite greater scrutiny and safety controls. 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.) We note that concerns of underreporting may bias our results downward since, as the *Times* article suggests, it is easier for well-connected companies to avoid disclosing work deaths. Alternatively, our results could be biased upward if well-connected companies face greater media scrutiny.

We examine the issue of under-reporting in a couple of ways. First, media reports have suggested that worker deaths may be more easily hidden in mining industries, where production takes places out of view and often in more isolated locales.[12] A reporting bias would then be reflected in the measured effect of connections on worker deaths in mining versus non-mining industries. In the first two columns of Table 6, we include an interaction term $PoliticallyConnected \times Mining$ that allows the effect of $PoliticallyConnected$ to differ for mining industries (E01 and E05). The point estimates are negative though insignificant, which suggests that if anything the relationship between political connections and worker deaths is stronger in non-mining industries, perhaps indicating that our main results are biased downward.

A separate approach to assessing potential 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. In columns (3) and (4) of Table 6, we present results using an indicator for years when at least one severe accident occurs, $I\left(Deaths \geq 3\right)$, as the dependent variable. Compared to the results for any accident reported in Table 5, columns (3) and (4), the coefficient on $PoliticallyConnected$ is slightly lower for severe accidents, though the standard deviation of $I\left(Deaths \geq 3\right)$ is also far lower than that of $I\left(Deaths > 0\right)$.

While not conclusive, our findings are hard to reconcile with underreporting as the primary explanation for the difference in death rates at connected versus unconnected firms.

2.1 The effect of safety quotas

We now examine whether the relationship between political connections and worker deaths is affected by the existence of "no safety, no promotion" reforms that limited the promotion opportunities of regulators who failed to meet safety quotas. Plausibly, this made it costlier for regulators to turn a blind eye to unsafe working conditions at a firm, regardless of its political connections.

Our analysis is limited by the fact that very few reforms took place during 2008-2011, making it impossible to identify the effect of reform from within-province variation alone. For ex-

[12]For example, one recent article described cases of miners hiding the bodies of other workers in underground shafts to elude detection. See "Coal mine deaths fall but still remain high," *China Daily*, February 26, 2011

ample, in provinces that implemented reforms during 2009 or 2010, i.e., those that would allow for a clear pre-post analysis, there are only four companies with $PoliticallyConnected = 1$. Thus, in the results that follow, the effects of reform are essentially identified from cross-sectional differences at the province level in the presence of safety quotas.

As a preliminary overview of the data, in Table 7 we show worker deaths per firm for company-year observations in provinces that had passed "no safety, no promotion" reforms versus those that had not. For the purposes of these descriptive statistics, we set $SafetyQuota_{cy} = 1$ for firm-year observations where the province where the company is located had implemented reforms in year $y - 1$ or earlier. For the year of reform, we define $SafetyQuota_{cy} = 1$ if the reforms went into effect in the first half of the year, and $SafetyQuota_{cy} = 0$ otherwise. (Note that none of the results below are at all sensitive to our treatment of reform years, in large part because relatively few provinces undertook reforms during our sample period.)

In the first row of Table 7, we compare $log(1 + Deaths)$ for firms in provinces with safety quotas to those without, as a function of political connectedness. For firms with $PoliticallyConnected = 0$, we observe no difference across the two groups of provinces. By contrast, for politically connected firms, $log(1+Deaths)$ is less than half as large for companies located in provinces with safety quota reforms. In the second and third rows, we observe qualitatively similar patterns for both $I\,(Deaths > 0)$ and also $DeathRate$.[13]

In Table 8, we present regression analyses on the effects of safety quotas by including the interaction term $PoliticallyConnected \times SafetyQuota$ as a covariate in equation (1).[14] Since the relevant variation in this analysis is at the province-level, we cluster standard errors by province; in unreported results, we cluster by firm, which produces smaller standard errors. Column (1) includes basic firm-level controls, as well as province, industry, and year fixed effects. The coefficient on $PoliticallyConnected \times SafetyQuota$ is -0.50, significant at the 10 percent level. It is almost equal in magnitude to the direct effect of $PoliticallyConnected$, implying virtually no difference between politically connected and unconnected firms in $SafetyQuota = 1$ provinces. We observe a similar pattern in column (2), which includes firm fixed effects.

However, when we include $PoliticallyConnected$ by $Province$ fixed effects in column (3), the coefficient on $PoliticallyConnected \times SafetyQuota$ falls by more than a half, with a large standard error. This underscores the fact that we cannot identify the effect of safety quotas from within-province variation in regulation, since there are insufficient data on firms in provinces that implemented safety reforms during 2008-11. This raises concerns of unobserved

[13] In unreported results, we also find a similar pattern if we look at residuals from a regression of $log(1 + Deaths)$ on covariates other than $PoliticallyConnected$, as well as year and industry fixed effects.

[14] We observe similar patterns if we use other measures of worker fatalities - such as $DeathRate$ - as the outcome variable.

heterogeneity across provinces, where adopters of safety quotas may differ systematically in their oversight of well-connected companies. As noted in the Data section above, there are no obvious province-level attributes that predict worker deaths that are also correlated with "no safety, no promotion" policies. Nonetheless, considerable caution is warranted in drawing any conclusions on the efficacy of quotas in reducing the role of influence-seeking in dangerous industries given the source of identification.

A separate concern is that the reduction in fatalities may reflect an increase in underreporting after safety quotas are put in place. This issue certainly applies to the overall difference in worker death rates, which are about half as large for firms in provinces with safety quotas than those without (0.17 versus 0.08). However, we note that it is not clear why this would disproportionately affect politically connected firms. As a separate check on whether underreporting might account for our findings on safety quotas, we repeat the preceding analysis using an indicator variable for the occurrence of a severe ($Deaths \geq 3$) accident as the dependent variable, which is less prone to misreporting. We obtain very similar results in terms of implied magnitudes and statistical significance.

2.2 Impact of worker death on expected profits

Up to this point, we have focused on the determinants of worker fatalities. We now turn to examine the effects that fatalities have on expected future profits, as captured by stock market reaction to worker deaths, in order to adjudicate among potential mechanisms through which political ties affect worker safety. *Ex ante*, the effect of political connections is ambiguous. Most obviously, to the extent that connections insulate a firm from regulatory backlash following worker deaths, one would expect the share price of a well-connected firm 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. However, as we have noted above, prior work has found a positive correlation between political connections and profitability (Fan et.al. (2008) and Xu and Zhou (2011)). If a fatal accident forces the departure of well-connected executives, the market may respond more negatively to news of safety lapses at connected firms. The overall effect is thus an empirical question.

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 134 of the 239 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 ollowing the first - and repeat the process. There are 39 instances of accidents clustered within 30 trading days, 31 in construction, and leads to the deletion of 59 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, $MonthlyDeaths_{ca}$.

In Figure 2, we present cumulative abnormal returns (CARs), estimated using a standard Fama-French three-factor model, over event windows of up to 30 trading days following each accident date. We show CARs for the full sample and also for the sample divided based on $PoliticallyConnected$. For the full sample, there is no effect on the stock price following an accident. The same is true for the subsample of $PoliticallyConnected = 0$ firms, where the average CARS from [1,1] to [1,30] sit virtually on top of the full sample returns, simply because these companies represent 86 percent of the sample. For the subset of 23 observations with $PoliticallyConnected = 1$, we observe a steady decline in firm value from the date of an accident, which levels off after about 20 trading days. There is good reason to expect this delay in investor response: in a majority of cases (110 of 174), 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 have been slow to reach the market.

The regression results in Table 9 echo the patterns observed in Figure 2 - politically connected firms suffer relatively large declines in market value following worker deaths, with the decline increasing over the first 20 trading days following each accident. Over a three day window following an accident, connected firms decline by about 2.4 percent more than unconnected firms do. This difference widens to over 9 percent after 20 days. Our estimates of the effect of $PoliticallyConnected$ are imprecise over all event windows, with p-values ranging from 0.15 to 0.06, so these results are necessarily interpreted with caution. We note additionally that total deaths in the 30 trading days after the accident is also negatively correlated with returns; however, the coefficient on $log(MonthlyDeaths_{ca})$ is not significant at conventional levels over most event windows.

To examine whether the negative effect of worker deaths on connected firms' returns is validated by subsequent profits, we look in Table 10 at ROA as a function of lagged worker deaths and lagged connections:

$$\begin{aligned}
ROA_{cy+1} = & \beta_0 + \beta_1 \times log(1 + Deaths_{cy}) + \beta_2 \times PoliticallyConnected_{cy} \\
& + \beta_3 \times log(1 + Deaths_{cy}) \times PoliticallyConnected_{cy} \\
& + \beta_4 \times StateOwnership_{cy} + \beta_5 \times log(Employment_{cy}) \\
& + \beta_6 \times ROA_{cy} + \beta_7 \times log(Assets_{cy}) + \beta_8 \times Investment_{cy} + \varepsilon_{cy}
\end{aligned} \tag{2}$$

In column (1) we present the results without the interaction term. Confirming the findings of Fan et.al. (2008) and Xu and Zhou (2011), we find that well-connected firms are relatively profitable: the coefficient on *PoliticallyConnected* is 0.018, significant at the 5 percent level. Notably, lagged worker deaths are uncorrelated with profits for the full sample. In column (2) we add the interaction term; its coefficient β_3 is negative, significant at the 5 percent level, and implies that the profit premium from (lagged) connections largely disappears in the year following worker deaths.[15]

The most straightfoward interpretation for the observed negative returns at connected companies following fatal accidents is a greater chance that a well-connected executive - and the profits his connections generate - will depart. 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), potentially resulting in the departure of connected executives implicated in an accident. We thus examine whether a company's political connections are affected by worker fatalities. We use the following specification, which captures the relationship between fatalities in year y-1 and political connections at the beginning of year y:

$$\begin{aligned}
PoliticallyConnected_{cy} = & \beta_0 + \beta_1 \times log(1 + Deaths_{cy-1}) \\
& + \beta_2 \times PoliticallyConnected_{cy-1} \\
& + \beta_3 \times log(Employment_{cy-1}) + \beta_4 \times ROA_{cy-1} \\
& + \beta_5 \times log(Assets)_{cy-1} + \beta_6 \times Investment_{cy-1} + \varepsilon_{cy}
\end{aligned} \tag{3}$$

We present the results in Table 11. In our baseline specification, including year, industry, and province fixed effects, the coefficient on *log(*1 + *Deaths*) is -0.10, significant at the 1 percent level. To appreciate its magnitude, we note that for the set of firm-year observations

[15] Bond (2002) shows that for a simple AR(1) model with a small number of time periods, within group estimation of "Large N, Small T" panel data typically leads to a downward bias, and he suggests the differenced and system Generalized Method of Moments (GMM) approach. Given that we only have 4 years' data (which is larger than the required mininum of 3), Bond's analysis suggests that our within-group estimation may provide a lower bound estimate. When we include lagged ROA in our regression and follow Roodman's (2008) procedure to perform the differenced and system GMM estimations, we get qualitatively similar results, though these point estimates are larger than our within-group estimation, as suggested by Bond (2002).

with non-zero deaths, the median is 3; the results indicate that a firm with this median number of annual deaths is 13.9 percentage points (-0.10*$log(1+3)$) less likely to employ a politically connected executive by the beginning of the following year relative to a firm with no deaths, significant at the 1 percent level. In column (2) we include firm fixed effects, which yields similar results.

The specification in Equation (3) effectively treats the loss and gain of connections symmetrically. Yet the executive turnover hypothesis relates primarily to the loss of connections - it is unclear why worker fatalities would make it less likely for a former government official to join a company the following year (indeed, the opposite is more plausibly the case, if adding political connections is a means of dealing with a potential regulatory response). In columns (3) and (4), we therefore include the interaction term $log(1 + Deaths_{cy-1}) \times PoliticallyConnected_{cy-1}$, which allows for a differential effect of worker fatalities on gains versus losses of political connections. In column (3), we find that the direct effect of $log(1 + Deaths_{cy-1})$, which reflects the impact of lagged deaths for companies without political ties at the beginning of year $y - 1$, is still negative, though only a little over half the magnitude of the comparable coefficient in column (1). The coefficient on the interaction term is negative and significant at the 1 percent level. Its magnitude, -0.254, is more than four times that of the direct effect, implying a sharp asymmetry between the impact of worker mortalities on the gain versus loss of connections. The firm fixed effects specification in column (4) shows a less stark difference between gains and losses in connections - the implied effect of worker fatalities on future political connections is still more than twice as large in losses relative to gains, but the difference is not significant (p-value for the coefficient on the interaction term is 0.22).[16]

2.3 Identification concerns

There are two primary challenges to identification. First, prior political experience may be correlated with unobserved differences in managerial attributes. Second, 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.

First, connected versus unconnected executives may differ 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

[16] Unless a government official is actually arrested, it is possible to obtain further employment from a different (private) firm where they may continue to exploit their social and political resources. Unfortunately, it is difficult to track individuals following their departure from publicly traded companies.

expect their firms to be poorly run more broadly and hence less profitable, whereas we observe higher profits at connected company in the data. Rather, we are concerned spercifically with managers' ability to execute worker safety measures, and incentives affecting profit-safety tradeoffs.

To explore the possible effect of the executives' competence in executing worker safety, we include 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 anexecutive's familiarity with the industry's safety practices. These additional controls have little effect on our findings.

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 lead to connected managers causing higher 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. In addition to hinging on the assumption that connected executives are more likely to seek politicaloffice in the future, this model also predicts that connected managers would run their companies in order to promote growth. We find no evidence of this in the data: neither employment nor sales is correlated with connections, after controlling for industry and year effects.

A second class of identification concerns relate 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. Contrary to this hypothesis, we find that companies with connected executives underperform following fatal accidents. Further, if connected managers were hired to deal with problems following worker deaths, we would not expect firms to lose their connections following worker fatalities, as we find in Section 2.2.

A final selection concern derives from the possibility that firms differ in their safety compliance costs, and it is precisely those with high expected safety expenses that hire former politicians to avoid such costs. This would bias our results upward only if companies with high safety costs are also those where weak compliance would have a pronounced effect on worker safety. There are two primary reasons we believe that this is unlikely to be a first-order consideration. First, given the positive relationship between connections and worker deaths survives our fixed-effects specifications, any correlation between anticipated safety costs and selection of connected executives cannot be the result of time-invariant worker risk. We cannot, on this basis, rule out the possibility that a change in compliance costs in turn induce firms to hire well-connected executives, but it emphasizes the strong assumptions required for a selection-based bias to exist. Second, if safety expenditures were a first-order consideration in the labor market for managers, we would expect a lower prevalence of connected executives at companies located in "No Safety, No Promotion" provinces, where there is less scope to exploit connections to circumvent safety rules. However, we observe no such difference in the prevalence of political connections: the average of *PoliticallyConnected* is 0.103 in province-year observations with $SafetyQuota = 0$ versus 0.104 for province-year observations where $SafetyQuota = 1$.

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.

3 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. We argue that our results are best explained by well-connected companies using their political relationships to circumvent safety oversight and regulation, consistent with anecdotal reports in the Chinese media. Further, we find that the relationship between connections and worker deaths is attenuated for companies located in provinces where officials' promotion is contingent on meeting safety quotas, suggesting that stronger safety incentives for regulators could significantly reduce the potential for politically favored firms to shirk on safety. 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 corruption 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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Table 1: Passage of province-level "no safety, no promotion" policies

Province	Effective date	Passage date
Guangdong	1-Feb-2005	1-Feb-2005
Guizhou	1-Jun-2006	1-Jun-2006
Ningxia	1-Nov-2006	27-Sep-2006
Heilongjiang	1-Jan-2007	28-Nov-2006
Shanxi	29-Jan-2008	29-Jan-2008
Yunnan	18-Sep-2008	18-Sep-2008
Guangxi	1-Jan-2009	27-Dec-2008
Jilin	20-Jul-2009	8-Jan-2009
Hainan	2-Jun-2010	2-Jun-2010
Jiangsu	23-Nov-2010	23-Nov-2010
Hebei	1-Jan-2011	8-Nov-2010
Henan	3-Jan-2011	3-Jan-2011
Inner Mongolia	4-Mar-2011	4-Mar-2011
Hunan	22-Mar-2011	22-Mar-2011
Sichuan	11-Apr-2011	11-Apr-2011
Chongqing	4-Jul-2011	4-Jul-2011
Shaanxi	4-Jan-2012	4-Jan-2012
Liaoning	1-Feb-2012	30-Nov-2011
Shandong	6-Apr-2012	6-Apr-2012
Jiangxi	3-May-2012	3-May-2012
Hubei	No safety quota	No safety quota
Qinghai	No safety quota	No safety quota
Shanghai	No safety quota	No safety quota
Tianjin	No safety quota	No safety quota
Tibet	No safety quota	No safety quota
Xinjiang	No safety quota	No safety quota
Zhejiang	No safety quota	No safety quota
Anhui	No safety quota	No safety quota
Beijing	No safety quota	No safety quota
Fujian	No safety quota	No safety quota
Gansu	No safety quota	No safety quota

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. Guodong was the first province to adopt this policy in 2005, while others followed suit only in recent years.

Table 2A: Summary statistics for firm-year panel data

Variable	Mean	SD	Median	Min	Max	Obs
log(1+Deaths)	0.259	0.658	0.000	0.000	5.094	933
I(Deaths>0)	0.169	0.375	0.000	0.000	1.000	933
DeathRate	0.143	0.807	0.000	0.000	11.433	933
I(Deaths>=3)	0.039	0.194	0.000	0.000	1.000	933
PoliticallyConnected	0.103	0.304	0.000	0.000	1.000	933
SafetyQuota	0.280	0.449	0.000	0.000	1.000	933
StateOwnership	0.248	0.261	0.178	0.000	0.900	933
log(Assets)	22.169	1.542	22.047	18.329	28.136	933
ROA	0.042	0.077	0.035	-0.310	0.250	933
log(Employment)	8.144	1.490	8.014	3.135	13.223	933
Investment	0.077	0.065	0.064	-0.179	0.455	933

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*); *I(Deaths>=3)* is an indicator variable denoting at least three fatalities from one accident in a firm-year observation; *PoliticallyConnected* 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; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares; *SafetyQuota* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year, while for the passage year, it is defined as the fraction of days the law was in effect.

Table 2B: Summary statistics for firm-year panel data: politically connected vs. politically unconnected

	PoliticallyConnected=0	PoliticallyConnected=1
log(1+Deaths)	0.184	0.915
I(Deaths>0)	0.133	0.490
DeathRate	0.099	0.520
I(Deaths>=3)	0.024	0.177
SafetyQuota	0.280	0.281
StateOwnership	0.234	0.376
log(Assets)	22.023	23.443
ROA	0.039	0.070
log(Employment)	8.015	9.264
Investment	0.075	0.095

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*); *I(Deaths>=3)* is an indicator variable denoting at least three fatalities from one accident in a firm-year observation; *PoliticallyConnected* is a firm-year indicator variable, to be equal to one if a senior executive at a 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; *Investment* is the ratio of capital expenditure to total assets; *StateOwnership* is the ratio of state shares to total outstanding shares; *SafetyQuota* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year, while for the passage year, it is defined as the fraction of days the law was in effect.

Table 3: Relationship between departure or arrival of politically connected executives and worker mortality

	Departure of a politically connected executive	Arrival of a politically connected executive
ΔDeaths	-5.040	5.222
ΔI(Deaths>0)	-0.320	0.417
ΔDeathRate	-0.641	0.987
Δlog(Employment)	0.174	0.165
Observations	25	36

Note: This table shows the change in various measures of worker mortality in the year a firm experiences a change in political connections. Δ denotes a first-difference between year of arrival or departure and the preceding year. *Deaths* is total worker fatalities; *I(Deaths>0)* is an indicator variable denoting at least one fatality in a firm-year observation; *DeathRate* is equal to 1000*(*Deaths/Employment*).

Table 4: The impact of political connections on workplace deaths: negative binomial regression

	(1)	(2)	(3)	(4)
VARIABLES		Dependent variable: Deaths		
PoliticallyConnected	1.663***	1.788***	2.062***	2.133***
	(0.394)	(0.427)	(0.420)	(0.384)
StateOwnership		0.795	0.374	1.009*
		(0.499)	(0.537)	(0.563)
log(Assets)		0.421**	0.520***	0.744***
		(0.176)	(0.175)	(0.211)
ROA		4.748*	1.366	-0.766
		(2.434)	(1.882)	(1.996)
log(Employment)		0.433***	0.284*	0.130
		(0.157)	(0.160)	(0.166)
Investment		-0.976	-0.771	-1.424
		(2.528)	(2.673)	(2.321)
Constant	-0.295	-14.411***	-13.910***	-17.975***
	(0.317)	(3.020)	(2.808)	(3.616)
Fixed Effects	No	No	Industry & Year	Industry ,Year & Province
Observations	933	933	933	933
Log pseudolikelihood	-825.315	-755.473	-716.220	-673.792

Notes: A count model is used to estimate the impact of political connections on workplace mortality. The dependent variable, *Deaths,* is firm-level worker fatalities; *PoliticallyConnected* is a firm-year indicator variable, to be equal to one if a senior executive at a firm previously served as mayor or vice-mayor ("Tingju Ji" in Chinese) in the city 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; *Investment* is the ratio of capital expenditure to total assets. Both *Investment* and *ROA* are lagged one year. Robust standard errors, clustered at the firm-level, are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: The impact of political connections on workplace deaths: OLS regression

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	log(1+Deaths)		I(Deaths>0)		DeathRate	
PoliticallyConnected	0.402***	0.696***	0.177***	0.273***	0.468**	0.642*
	(0.139)	(0.191)	(0.060)	(0.072)	(0.220)	(0.371)
StateOwnership	0.129	0.097	0.081	0.130	0.064	0.288
	(0.091)	(0.173)	(0.055)	(0.095)	(0.084)	(0.199)
log(Assets)	0.051	-0.075	0.030	-0.049	0.145	0.109
	(0.036)	(0.068)	(0.020)	(0.044)	(0.090)	(0.211)
ROA	-0.009	0.077	-0.117	-0.007	-0.017	0.381
	(0.214)	(0.265)	(0.121)	(0.159)	(0.263)	(0.547)
log(Employment)	0.111***	0.046	0.054**	0.021	-0.171*	-0.406
	(0.041)	(0.070)	(0.022)	(0.038)	(0.091)	(0.360)
Investment	-0.432	-0.016	-0.251	0.002	-0.038	0.696
	(0.334)	(0.467)	(0.196)	(0.257)	(0.523)	(0.621)
Constant	-1.454**	1.411	-0.679**	0.994	-1.599	0.781
	(0.570)	(1.393)	(0.306)	(0.900)	(1.203)	(2.607)
Fixed Effects	Province, Industry & Year	Firm & Year	Province, Industry & Year	Firm & Year	Province, Industry & Year	Firm & Year
Observations	933	933	933	933	933	933
Adjusted R-squared	0.368	0.651	0.371	0.638	0.102	0.377

Notes: A standard OLS model is used to estimate the impact of political connections on workplace mortality. In columns (1) and (2), the dependent variable is log(*1+Deaths*), where *Deaths* is firm-level worker fatalities. In columns (3) and (4), the dependent variable is *I(Deaths>0)*, an indicator variable denoting at least one fatality in a firm-year observation, and in columns (5) and (6), the dependent variable is *DeathRate*, equal to 1000*(*Deaths/Employment*); *PoliticallyConnected* is a firm-year indicator variable, equal to one if a senior executive at a firm previously served as mayor or vice-mayor ("Tingju Ji" in Chinese) in the city where the firm is located or as a provincial or central government official of the same or higher rank; *State Ownership* is the ratio of state shares to total outstanding shares; *Investment* is the ratio of capital expenditure to total assets. Both *Investment* and *ROA* are lagged one year. Robust standard errors, clustered at the firm-level, are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: The impact of political connections on workplace death: possible under-reporting

	(1)	(2)	(3)	(4)
VARIABLES	Log(1+Deaths)		I(Deaths>=3)	
PoliticallyConnected	0.493***	0.789***	0.141***	0.255***
	(0.146)	(0.193)	(0.0489)	(0.0792)
PoliticallyConnected	-0.249	-0.493		
*Mining	(0.301)	(0.575)		
StateOwnership	0.096	0.089	0.014	0.004
	(0.100)	(0.175)	(0.03)	(0.06)
log(Assets)	0.048	-0.066	0.004	-0.038
	(0.032)	(0.071)	(0.009)	(0.035)
ROA	0.075	0.092	0.048	-0.007
	(0.209)	(0.269)	(0.052)	(0.080)
log(Employment)	0.120***	0.044	-0.064	0.026
	(0.035)	(0.071)	(0.010)	(0.205)
Investment	-0.480	-0.069	0.019**	0.023
	(0.315)	(0.485)	(0.009)	(0.040)
Constant	-1.393**	1.239	-0.235*	0.675
	(0.542)	(1.440)	(0.138)	(0.657)
Fixed Effects	Province, Industry & Year	Firm & Year	Province, Industry & Year	Firm & Year
Observations	933	933	933	933
Adjusted R-squared	0.334	0.655	0.118	0.325

Notes: Each column shows the results of a standard OLS model. In columns (1) and (2), the dependent variable is log(1+*Deaths*) where *Deaths* is firm-level worker fatalities. In columns (3) and (4), the dependent variable is *I(Deaths>=3)*, an indicator variable denoting at least three fatalitiesfrom one accident in a firm-year observation; *PoliticallyConnected* is a firm-year indicator variable, to be equal to one if a senior executive at a firm previously served as mayor or vice-mayor ("Tingju Ji" in Chinese) in the city where the firm is located or as a provincial or central government official of the same or higher rank; *Mining* is an indicator variable equal to 1 if the firm is in a mining industry (codes B01 and B05); *StateOwnership* is the ratio of state shares to total outstanding shares; *Investment* is the ratio of capital expenditure to total assets. Both *Investment* and *ROA* are lagged one year. Robust standard errors, clustered at the firm-level, are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: The effect of safety quotas: summary statistics

	SafetyQuota=0 & PoliticallyConnected=0	SafetyQuota=1 & PoliticallyConnected=0	SafetyQuota=0 & PoliticallyConnected=1	SafetyQuota=1 & PoliticallyConnected=1
Log(1+Deaths)	0.178	0.199	1.062	0.540
I(Deaths>0)	0.131	0.137	0.551	0.333
DeathRate	0.109	0.075	0.665	0.150

Notes: *Deaths* is firm-level worker fatalities; *I(Deaths>0)* is an indicator variable denoting at least one fatality in a firm-year observation; *DeathRate* is equal to 1000*(*Deaths*/*Employment*). *PoliticallyConnected* is a firm-year indicator variable, equal to one if a senior executive at a firm previously served as mayor or vice-mayor ("Tingju Ji" in Chinese) in the city where the firm is located or as a provincial or central government official of the same or higher rank; *SafetyQuota* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year, while for the passage year, it is defined as the fraction of days the law was in effect.

Table 8: The effect of safety quotas: OLS regression

	(1)	(2)	(3)
VARIABLES		Log(1+Deaths)	
PoliticallyConnected	0.560***	0.882***	
	(0.174)	(0.211)	
PoliticallyConnected *SafetyQuota	-0.497**	-0.616**	-0.057
	(0.222)	(0.303)	(0.423)
SafetyQuota	0.003	0.019	0.006
	(0.068)	(0.087)	(0.064)
StateOwnership	0.132	0.086	0.125
	(0.095)	(0.173)	(0.095)
log(Assets)	0.045	-0.072	0.037
	(0.036)	(0.073)	(0.037)
ROA	0.014	0.091	0.363
	(0.216)	(0.265)	(0.260)
log(Employment)	0.122*	0.0540	0.129**
	(0.0619)	(0.0678)	(0.0594)
Investment	0.115***	0.044	0.122***
	(0.040)	(0.071)	(0.037)
Constant	-1.377**	1.348	-1.584***
	(0.570)	(1.496)	(0.575)
Fixed Effects	Province, Industry & Year	Firm & Year	Year & PoliticallyConnected X Province
Observations	933	933	933
Adjusted R-squared	0.377	0.661	0.409

Notes: Each column shows the results of a standard OLS model. The dependent variable is log(1+*Deaths*) where *Deaths* is firm-level worker fatalities; *PoliticallyConnected* is a firm-year indicator variable, equal to one if a senior executive at a firm previously served as mayor or vice-mayor ("Tingju Ji" in Chinese) in the city where the firm is located or as a provincial or central government official of the same or higher rank; *SafetyQuota* is a province-year variable denoting whether a province had passed a "no safety, no promotion" law in a previous year, while for the passage year, it is defined as the fraction of days the law was in effect; *StateOwnership* is the ratio of state shares to total outstanding shares; *Investment* is the ratio of capital expenditure to total assets. Both *Investment* and *ROA* are lagged one year. Robust standard errors, clustered at the firm-level, are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: The impact of political connections on cumulative abnormal event returns (CARs): OLS regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	CARs[1,3]	CARs[1,5]	CARs[1,10]	CARs[1,15]	CARs[1,20]	CARs[1,25]	CARs[1,30]
PoliticallyConnected	-0.024	-0.046	-0.054	-0.061	-0.093**	-0.097*	-0.104**
	(0.015)	(0.029)	(0.036)	(0.037)	(0.046)	(0.050)	(0.050)
StateOwnership	-0.012	-0.004	0.015	0.019	0.037	0.047	0.074**
	(0.009)	(0.015)	(0.019)	(0.023)	(0.030)	(0.037)	(0.037)
log(Assets)	-0.006	-0.013	-0.008	0.008	-0.000	-0.009	-0.026*
	(0.007)	(0.009)	(0.010)	(0.010)	(0.011)	(0.017)	(0.014)
ROA	-0.118	0.138	0.081	-0.161	-0.041	-0.180	0.198
	(0.139)	(0.167)	(0.239)	(0.211)	(0.275)	(0.310)	(0.320)
log(MonthlyDeaths)	-0.006	-0.010**	-0.014**	-0.010	-0.010	-0.008	-0.011
	(0.004)	(0.005)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)
Constant	0.198	0.356*	0.216	-0.113	0.068	0.261	0.662**
	(0.159)	(0.204)	(0.230)	(0.223)	(0.252)	(0.390)	(0.310)
Fixed Effects	Province, Industry & Year						
Observations	174	174	174	174	174	174	174
Adjusted R-squared	0.280	0.359	0.379	0.376	0.390	0.351	0.397

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 three-factor model, over the window of [1,K] where the accident day is set as 1; *PoliticallyConnected* is a firm-year indicator variable, equal to one if a senior executive at the firm previously served as mayor or vice-mayor ("Tingju Ji" in Chinese) in the city 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 occur within a month of each initial accident; *StateOwnership* is the ratio of state shares to total outstanding shares. Robust standard errors, clustered at the firm-level, are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Political connections and firm performance

	(1)	(2)
VARIABLES	ROA_F1	
PoliticallyConnected	0.018**	0.030***
	(0.009)	(0.011)
PoliticallyConnected		-0.016**
*log(1+Deaths)		(0.007)
log(1+Deaths)	-0.001	0.004
	(0.004)	(0.004)
log(Assets)	0.008	0.008
	(0.007)	(0.007)
log(Employment)	-0.009	-0.009
	(0.006)	(0.006)
Investment	0.158***	0.156***
	(0.057)	(0.057)
StateOwnership	0.001	0.001
	(0.015)	(0.015)
Constant	0.004	-0.001
	(0.122)	(0.121)
Fixed Effects	Year & Industry & Province	
Observations	658	658
Adjusted R-squared	0.205	0.206

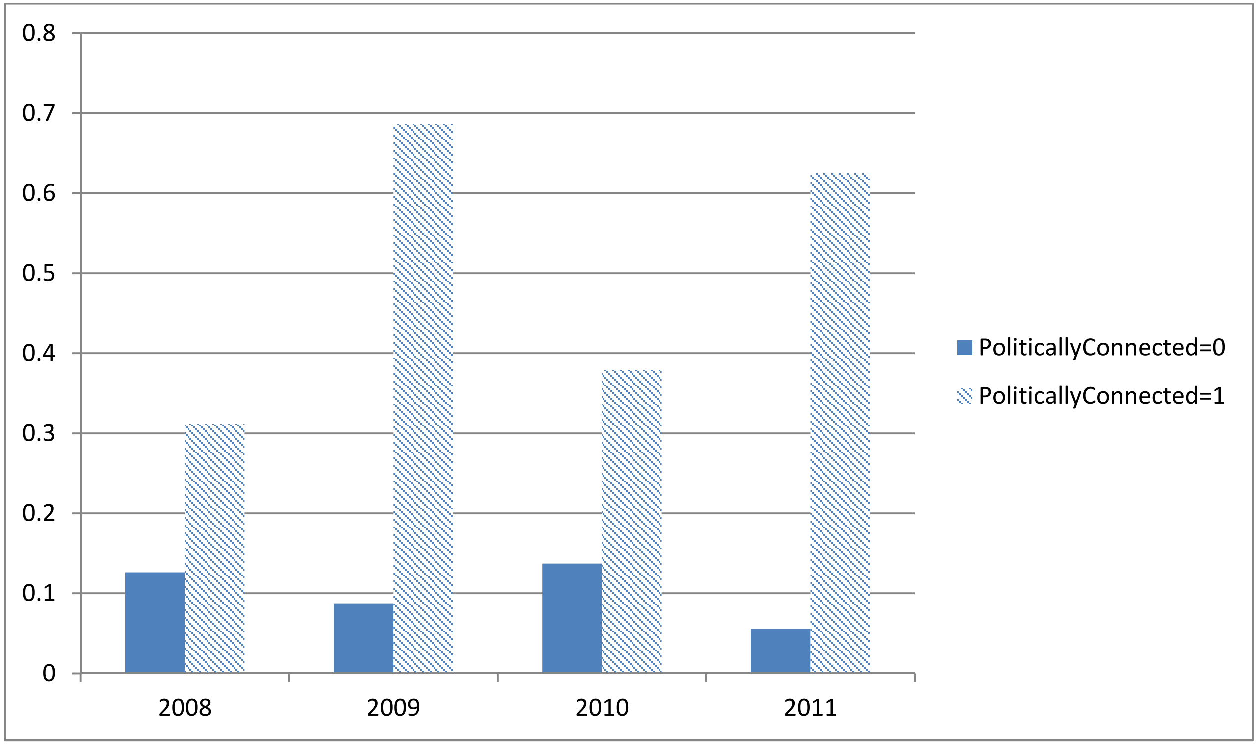
Notes: All columns show the results of a standard OLS regression. The depedent variable, *ROA_F1* is a firm's ROA in the next year. *PoliticallyConnected* is a firm-year indicator variable, equal to one if a senior executive at a firm previously served as mayor or vice-mayor ("Tingju Ji" in Chinese) in the city where the firm is located or as a provincial or central government official of the same or higher rank; *Deaths* is firm-level worker fatalities per year; *StateOwnership* is the ratio of state shares to total outstanding shares; *Investment* is the ratio of capital expenditure to total assets. All right hand side variables are measured in year *y-1* while the dependent variable is measured in year *y*. Robust standard errors, clustered at the firm-level, are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: The effect of workplace deaths on political connections

	(1)	(2)	(3)	(4)
VARIABLES	Dependent Variable: PoliticallyConnected in year y			
log(1+Deaths)	-0.100***	-0.0893***	-0.0599***	-0.0708**
	(0.0236)	(0.0333)	(0.0203)	(0.0359)
PoliticallyConnected in year y-1	0.465***	-0.0976	0.585***	-0.0362
	(0.0644)	(0.0761)	(0.0788)	(0.0771)
PoliticallyConnected in year y-1			-0.254**	-0.153
* log(1+Deaths)			(0.109)	(0.135)
StateOwnership	0.0244	0.0507	0.0198	0.0630
	(0.0526)	(0.102)	(0.0494)	(0.101)
log(Assets)	0.0177	0.0835	0.0175	0.0885
	(0.0163)	(0.0868)	(0.0149)	(0.0864)
log(Employment)	0.0128	0.0214	0.0231	0.0152
	(0.0201)	(0.0916)	(0.0184)	(0.0962)
ROA	0.355***	0.0770	0.405***	0.0591
	(0.119)	(0.197)	(0.125)	(0.194)
Investment	0.0393	-0.555	0.102	-0.537
	(0.199)	(0.380)	(0.187)	(0.367)
Constant	-0.331	-1.853	-0.512**	-1.920
	(0.252)	(1.879)	(0.223)	(1.859)
Fixed Effects	Province, Industry & Year	Firm & Year	Province, Industry & Year	Firm & Year
Observations	655	655	655	655
Adjusted R-squared	0.377	0.704	0.377	0.706

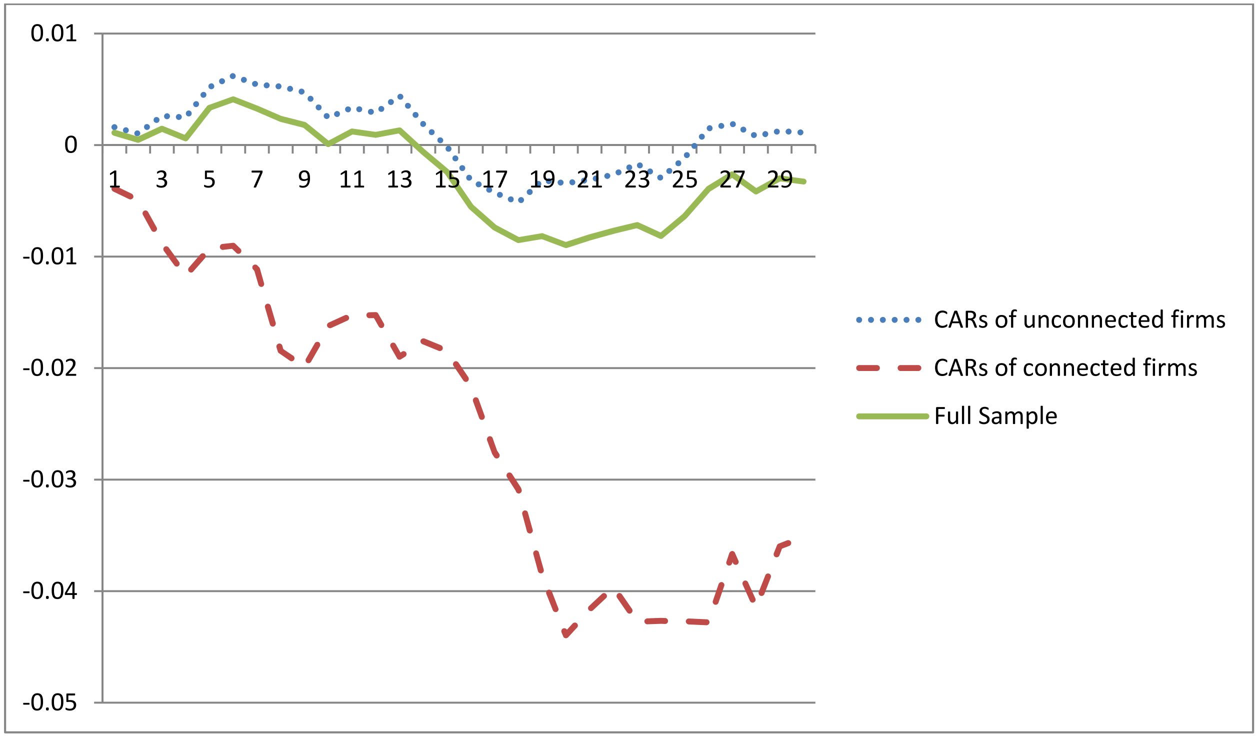
Notes: All columns show the results of a standard OLS regression. The dependent variable, *PoliticallyConnected*, is a firm-year indicator variable, equal to one if a senior executive at a firm previously served as mayor or vice-mayor ("Tingju Ji" in Chinese) in the city where the firm is located or as a provincial or central government official of the same or higher rank; *Deaths* is firm-level worker fatalities per year; *StateOwnership* is the ratio of state shares to total outstanding shares; *Investment* is the ratio of capital expenditure to total assets. All right hand side variables are measured in year *y-1* while the dependent variable is measured in year *y*. Robust standard errors, clustered at the firm-level, are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1: Average annual worker deaths per 1000 employees in safety-regulated industries, 2008-11, as a function of political connections



Notes: *PoliticallyConnected*, is a firm-year indicator variable, equal to one if a senior executive at a firm previously served as mayor or vice-mayor (“Tingju Ji” in Chinese) in the city where the firm is located as a provincial or central government official of the same or higher rank.

Figure 2: Market reaction to fatal accidents, by political connectedness



Notes: Each data point represents the post-accident cumulative abnormal returns CARs[1,K], estimated using a standard Fama-French three-factor model, over the window of [1,K] where the accident date is set as 1.

Appendix: Propensity Score Tests

Dependent Variable	Matching Method	Number of Treatment Firms	Number of Control Firms	ATT	Std. Err.
log(1+Deaths)				0.663	0.1
I(Deaths>0)	Radius Matching	85	785	0.312	0.056
DeathRate				0.481	0.18
log(1+Deaths)				0.485	0.105
I(Deaths>0)	Kernel Matching	96	785	0.209	0.053
DeathRate				0.43	0.177
log(1+Deaths)				0.426	0.204
I(Deaths>0)	Nearest-Neighbor Matching	88	67	0.216	0.096
DeathRate				0.456	0.218

Notes: Stata codes come from Becker Sascha 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(Assets)*, *ROA*, log(*Employment*), *Investment* and SIC-1 digit industry. The Epanechnikov kernel is used in the kernel matching where the bandwidth is set at 0.04, smaller than the default value of 0.06.