Peer Effects and Corporate Corruption *

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Abstract

We find evidence that financial misbehavior occurs in regionally concentrated waves: a firm's tendency to engage in misconduct increases with the misconduct rates of neighboring firms. This effect appears to be the result of peer effects, rather than exogenous shocks like regional variation in enforcement. Further, local waves of *financial* misconduct are correlated with *non-financial* misconduct, such as political fraud. Both firm and city performance suffer in the wake of local corruption waves.

Keywords: corporate corruption, financial misconduct, peer effects, political fraud

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1 Introduction

Traditional models of crime begin with Becker (1968), which frames the choice to engage in misbehavior like any other economic decision involving cost and benefit tradeoffs. Though somewhat successful when taken to the data, perhaps the theory's largest embarrassment is its failure to account for the enormous variation in crime rates observed across both time and space. Indeed, as Glaeser, Sacerdote, and Scheinkman (1996) argue, regional variation in demographics, enforcement, or other observables are simply not large enough to explain why, for example, two seemingly identical neighborhoods in the same city have such drastically different crime rates.[1] The answer they propose is simple: social interactions induce positive covariance in the tendency to break rules.

In this paper we examine whether likewise, peer effects influence the propensity of managers of large, public firms to engage in financial misconduct. Examples might include misrepresenting a firm's earnings, failing to disclose relevant news, trading on proprietary information, and spreading false rumors to depress the stock price of a potential takeover target.

We have three goals. First, we quantify the variation in rates of financial misconduct across cities, and over time within cities. Second, we aim to identify whether these static and dynamic regional patterns appear due to interactions between neighboring managers, or whether common local shocks, such as changing enforcement or trends in executive compensation, provide a better explanation for the patterns we observe. Last, we are interested in whether the effects of financial misconduct extend to other outcome variables such as bankruptcies and delistings, potentially providing an explanation for the spatial clustering we observe in these measures of firm performance.

We begin by measuring persistent differences in rates of financial misconduct across the twenty largest U.S. cities. Although the explanatory power from these regressions is low, city fixed effects are highly significant, indicating long-lived differences in financial misconduct

[1] For a theoretical justification of this idea, see Sah (1991).

across different locations. In a typical year, the SEC and/or DOJ prosecutes about 1.5% of firms for financial misconduct. But, for the quartile of cities least penalized (including midwestern cities Minneapolis, Cleveland, and Indianapolis) this rate is about 50% lower, whereas in the quartile of most penalized cities (e.g., Miami, Houston, and St. Louis), the rate is about 50% higher.

A similar picture emerges when we run logistic regressions predicting firm-level prosecutions of financial misconduct, as a function of the fraud rates of its local neighbors.[2] Whether measured year-by-year or averaged over time, the average fraud rate of a firm's neighbors has a positive, significant association with its own propensity for committing financial fraud. Roughly speaking, a move from the 25^{th} to the 75^{th} percentile of surrounding firm's fraud rates (from 0% to 1.72%) increases the likelihood of committing financial fraud from 1.46% to 1.70%, or an increase of about 17%.

That regional factors are significant determinants of corporate fraud is consistent with a number of explanations. One possibility is that the tolerance for corruption differs geographically due to *exogenous* factors such as demographics, wealth, religion, or culture.[3] Second, cross-city differences in financial misconduct could arise from *contextual* factors, such as differences in enforcement and/or resources dedicated to fighting corporate corruption. Finally, the tendency to commit financial fraud can spread within a region due to *endogenous* interactions between local managers.[4] In contrast to the numerous positive spillovers identified in the urban agglomeration literature, white collar crime being "contagious" would represent a distinctly negative externality.

In order to better distinguish between these alternatives, we conduct a number of addi-

[2]The data we use identities the years in which firms is suspected to have engaged in fraud, as indicated by investigations from the SEC and/or DOJ. Similar to virtually all papers in this literature, we do not directly observe fraud events, but rather the joint event of fraud and subsequent prosecutions.

[3]A good example of such long-lived cultural influences is Fisman and Miguel's (2007) study of parking ticket violations in New York City for U.N. diplomats, an interesting laboratory because diplomats are (over the sample period) immune from any prosecution. Even for diplomats residing in the United States for many years, standard country-level corruption measures remained strong predictors of violations, suggesting that norms affiliated with country of origin, and not just current environment, matters for predicting corruption.

[4]Manski (1993) is credited with developing this taxonomy when describing the "reflection problem" in studies of correlated behavior between groups.

tional tests. The first asks a cross-sectional question: Are cities more prone to corporate corruption also corrupt in other dimensions? An affirmative answer here would help rule out contextual and/or common treatment effects, provided that the rules and enforcement between arenas were not governed by the same factors. Glaeser and Saks' (2006) measure of political corruption is convenient in this regard because, as they argue, federal prosecutions of local government officials are initiated at the *national* rather than *local* level.[5] Thus, any correlation between city-level political and corporate corruption would appear to stem from either exogenous attributes or peer effects, even if local enforcement of financial misconduct is important (deHaan, Kedia, Koh, and Rajgopal (2012)).

Figure 1 compares the time-series average of each city's corporate (x-axis) and political corruption rate (y-axis). The simple correlation is 0.31, and in logistic regressions predicting prosecutions of corporate fraud, the time-series average of each city's rate of political fraud is highly significant. Moreover, the relation indicated in Figure 1 holds *in the time series within each city.* That is, for a given city, locally headquartered firms are more likely to commit financial fraud when public officials are also misbehaving at higher than average (for that city) rates. By holding constant the average fraud rates within each city through fixed effects, these findings cast doubt on the ability of slow-moving exogenous factors like religion or culture to explain our results.[6] Peer effects, on the other hand, are consistent with both the time-series and cross-sectional patterns.

A second set of analysis rules out both exogenous and contextual effects by construction, and thus represents perhaps the strongest evidence for endogenous peer effects. We begin by identifying metropolitan areas containing a single, dominant industry, such as Houston (energy) or San Francisco (software). Then we use the fraud rates of firms *within* these dominant industries, but *outside* the local area as an instrument for the fraud rates of the dominant-industry firms within the city of interest. For example, when predicting the

[5]For example, if Houston's mayor is convicted of accepting a bribe, the impetus for this investigation originates from Washington D.C., rather than Houston.

[6]See Kanatas, Grullon, and Weston (2013) and Dyreng, Mayew, and Williams (2013) for evidence that financial fraud is lower in areas characterized by low rates of church attendance.

likelihood that Houston-based energy firm Apache commits fraud, we use as an instrument the fraud rate of (a portfolio of) non-Houston oil and gas firms, like Chesapeake, which is located in Oklahoma City. This methodology generates variation in fraud rates of a city's dominant-industry firms that, by construction, cannot stem from local factors (here, anything specific to Houston).

The next step is to relate these instrumented fraud rates to those of local firms outside the dominant sector. For example, in San Francisco, the question is whether locally headquartered clothing retailer Gap or food producer Del Monte are more likely to engage in fraud when the performance of the software sector (measured outside San Francisco) is poor. Our analysis, which controls also for firm and industry performance, indicates a strong affirmative answer. Such a test remedies virtually any generic local contextual effects, and provide fairly direct evidence that peer effects catalyze the local propagation of financial misconduct.

The final part of the paper explores how local waves of corporate misbehavior may extend to other outcomes, both at the firm and city level. First, we explore whether being surrounded by high local fraud rates influences firms' likelihood of declaring bankruptcy or delisting.[7] Our joint hypothesis is that: 1) general trustworthiness or "social capital" (Guiso, Sapienza, and Zingales (2004)) is especially important for firms in financial distress, where agency and information problems are likely of first order concern, and 2) the firm's stakeholders understand that social capital has a local component. If so, then being surrounded by high rates of fraud may hasten a struggling firm's demise, *even having not committed fraud itself.*

To test this idea, we augment the failure model developed by Campbell, Hilscher, and Szilagyi (2008) to account for the financial misconduct of a firm's local peers, finding a strong

[7]Poor accounting quality has been linked to poor analyst forecast accuracy (Lang and Lundhold (1996), Byard and Shaw (2003)), higher cost of external finance (Francis et al. (2005)), and low investment prospects (Biddle and Hilary (2006)). See also Bushman and Smith (2001), Healy and Palepu (2001), Lambert, Leuz, and Verrecchia (2007), and Biddle and Hilary (2009). Our paper differs by examining the fraud rates of a firm's *peers*, rather than its own fraudulent activity.

relation to the probability of bankruptcy. Moreover, the effect is concentrated among the most vulnerable firms: among those with above median leverage, doubling the rate of local fraud increases by about 4% the likelihood that the firm declares bankruptcy or is delisted, an increase of about 70% relative to the baseline average.

Following up on the relation between fraud and the overall business climate, we also examine the relation between an area's fraud rate and employment in that region. We find a strong relation, even in the presence of city and year fixed effects. The sensitivity of future (up to three years) unemployment to area fraud rates is in the range of .2 to .3, and in most specifications is statistically significant at the 1% level. To put this in perspective, a one standard deviation increase in area fraud rates (about 1.4 %) decreases employment growth about 40 basis points, suggesting that local waves of corruption can have lasting effects on the local economy.

Our final tests explore the extent to which markets accurately understand these implications of local fraud waves for nearby, and thus potentially involved, companies. For the 426 unique corporate fraud events in our data set, we explore whether the announcement of a legal investigation has an immediate impact on the stock prices of nearby firms. The median, industry-adjusted CAR in such cases is about -20 basis points, which is statistically significant at the 1% level. Furthermore, when we limit the sample to firms that are subsequently (though not immediately) targeted for investigations themselves, the magnitudes increase three to fourfold. Together, this evidence suggests that investors anticipate the diffusion of corruption, and almost immediately incorporate this information into prices.

The results in this paper contribute directly to studies investigating the causes and consequences of financial misconduct. A number of factors have been identified as being relevant, including firm performance (Harris and Bromiley (2007)), manager or director career concerns (Fich and Shivdasani (2007)), compensation arrangements (Erickson and Maydew (2006)), institutional monitoring, and the strength of enforcement (Kedia and Rajgopal (2011)). Our study identifies peer effects as also contributing to corporate misbehavior, and uniquely relative to those listed above, represents an externality between neighboring firms. As noted in the crime economics literature, corporate behavior demonstrates geographic-specific path dependence: keeping misbehavior in check is therefore easiest "early" in the cycle.

The remainder of our paper is organized as follows. Section 2 describes the data we use for our analysis, including a description of how corporate misbehavior is identified and recorded. Then, we characterize the extent to which corporate misbehavior varies across regions and within regions over time in section 3. In section 4, we provide evidence that the spatial clustering we observe is most likely due to peer effects between local firms, rather than common environmental shocks. We explore some implications for corporate fraud in section 5, such as a heightened probability of bankruptcy, and consequently, higher unemployment in areas stricken by fraud waves. Section 6 concludes.

2 Data

2.1 Financial misconduct

The primary source for our fraud data is the one collected and described by Karpoff, Koester, Lee, and Martin (2012), hereafter KKLM, which details their hand-collection of over 10,000 events related to cases of corporate fraud and/or financial misconduct. Here, we provide a brief summary of the types of fraudulent events including in their dataset, and refer the reader interested in further detail (e.g., regarding the data collection method itself, comparison with other measures of fraud) to their paper.

KKLM aggregate information from four databases: 1) Government Accountability Office (GAO), 2) Audit Analytics (AA), 3) Securities Class Action Clearinghouse (SCAC), and 4) Securities and Exchange Commission's Accounting and Auditing Enforcement Releases (AAERs). The first two sources contain (mostly) information on financial statement "re-statement" announcements, and therefore are good sources for detecting a firm's attempt to manipulate earnings.[8] The third, the SCAC, maintains a registry of Federal class action securities litigation; accordingly, compared with the first two sources, this database reflects a wider variety of corporate misbehavior including accounting fraud, fraudulent transfers in mergers and acquisition, misrepresentation, and insider trading. The last source, the AAER, contains releases announcing enforcement or action "expected to be of interest to accounts." There is substantial overlap between the all four primary sources, both in terms of events covered and timing (see KKLM, section 2.3).

A significant advantage of the KKLM data is that it distinguishes between dates when a firm is suspect of committing fraud (the "violation period") and the dates these actions became public (the "revelation" period). Most of our analysis will focus on the violation period, where we look for correlations in the tendency to conduct fraud within a given geographic area. However, some of our tests exploit the revelation dates as well, allowing us, for example, to detect stock price reactions of nearby firms to announcements of fraud investigations.

Table 1 contains summary statistics related to our fraud measures. In Panel A we present variables defined at the firm-year level, while Panels B and C show those defined at the area-year and industry-year level, respectively. At the firm level, most of our analysis will consider $Fraud_{j,t}^{i,a}$, a dummy variable denoting financial misconduct by firm j, operating in industry i, in area a, during year t. Across all years and firms, the average value of $Fraud$ is 0.0146, indicating that at any point in time, 1-2% of firms are engaging in financial misconduct. Because in most cases, such behavior lasts several years, we define a second variable, $FraudInitiations$, which takes a value of one during the first year of a financial misconduct event, and zero otherwise. As seen, the average value of $FraudInitiations$ is much lower at 0.0034: only about 1:300 firms initiates financial misconduct in a typical year.

At the city (Panel B) and industry (Panel C) levels, fraud is defined using rates instead

[8]However, as KKLM describe in detail, up roughly 80-90% of restatements are, in fact, unintentional errors, and thus, do not correspond to attempted financial fraud. Their dataset distinguishes between intentional and unintentional errors by linking misstatements to subsequent SEC action.

of dummy variables, e.g., the average fraud rate for Seattle in the year 2001 is simply the sum of $Fraud$ of firms headquartered in Seattle in year 2001, divided by the number of firms headquartered in the Seattle economic area that year. The same applies to industry-level average. As expected, the means for city- and industry-level fraud rates are similar to the average at the firm level ($Fraud$), but there is substantial variation across both industries and cities, as well as over time. We return to these cross-industry and cross-city patterns in the next section.

2.2 Firm location

Our dataset includes firms headquartered near any of the twenty large metropolitan areas in the United States. The specific variable we use is ADDZIP listed in COMPUSTAT, corresponding to the current zip code each firm's headquarters or home office. Although this convention means that our dataset excludes firms once headquartered in one of our twenty areas but no longer are, the fact that firms move so infrequently means that very few observations are lost.

The geographic unit we use is an "Economic Area," as defined by the U.S. Bureau of Labor Statistics. EAs are larger than metropolitan statistical areas (MSAs), and are designed to capture regions within which workers commute. Examples of economic areas are Dallas-Arlington-Fort Worth, Washington D.C.-Columbia-Baltimore, and San Francisco-Oakland-San Jose. We use the term "area" and "city" interchangeably throughout the paper.

2.3 Other variables

Some of our later analysis will examine bankruptcy rates as a function of a firm's own financial misconduct, as well as that of its local neighbors. Accordingly, Table 1 also shows the average rate at which firms declare bankruptcy and/or are delisted from public exchanges. The average is 6%, which as we will also discuss later, varies substantially over time and across cities and industries.

Our tests also employ a number of standard control variables, all of which are obtained from standard sources. Stock returns are from CRSP and firm fundamentals from COMPUSTAT. Most of our fraud regressions include lagged stock returns, size (total assets), leverage (total liabilities over total assets), market-to-book ratio, and cash flow (EBITDA to assets). The summary statistics are shown in Table 1. In addition, when we predict firm failure in Section 5.1, we will augment the bankruptcy model developed by Campbell, Hilsher, and Szilagyi (2008), and so Table 1 also reports our sample average for the variables used in their logit model.

3 Financial misconduct and a firm's local environment

In this section, we establish the basic empirical foundation on which the rest of the paper builds, quantifying the extent to which financial misconduct tends to be regionally clustered. We begin in subsection 3.1 with some non-parametric analysis, using city effects to quantify persistent, cross-city variation in the tendency of firms to commit financial fraud. Subsection 3.2 then quantifies these cross-city findings more formally using logistic regressions, where we can control for various firm, industry, and market determinants of corporate fraud. Both sets of analysis indicate that a firm's local environment – even firms in very different sectors – is strongly related to the likelihood the firm commits fraud.

3.1 Variation in fraud rates across cities

To gauge the importance of a firm's headquarter location in the context of corporate fraud, we first compare the ability of year, industry, and area fixed effects to explain the total variation we observed in financial misconduct. Observations are at the firm-year level, with our dependent variable, $Fraud_{j,t}^{i,a}$,taking a value of one if firm i in industry j and area a is, in year t, initiates fraud or other financial misconduct. For now, the first years of each fraud event (initiations) are commingled with subsequent years (continuations), but in subsequent analysis, we often consider these separately.

We are interested in the change in explanatory power as we progressively add and subtract various vectors of fixed effects, shown in different columns in Table 3. The first column includes only year effects, and thus accounts for time-series effects that may influence the aggregate rate of prosecutions of financial misconduct. Examples of such factors might include changes in enforcement, macro effects, or changes in the sample composition over time, say, toward industries more apt to engage in fraud. Regardless of the specific reason, year fixed effects are highly significant, with a F-statistic equal to 16.78, far exceeding the 1% threshold (1.61 for 39 degrees of freedom). Note, however, that the R^2 is small, with year effects explaining less than 0.5% of the total variation in firm-level financial misconduct.

The second column replaces year fixed effects with industry fixed effects. Here too, the R^2 is quite low, but the significance of the industry fixed effects is strong, far exceeding the 1% threshold, indicative of persistent cross-industry differences in financial misconduct. The industry with the highest average fraud rate over our sample is software, with approximately 1.9% of firm-years being associated with a fraud event. At the other end of the spectrum, the health care and energy sectors are least likely to commit financial fraud, with rates less than half the software industry (0.87% and 0.83% respectively).

The third column focuses on area fixed effects, and thus, captures differences in the average rates of financial misconduct across our twenty different economic areas. Some sense for these patterns can already be gained by examining Table 2, which shows the average rates of financial misconduct by economic area. Midwestern cities Indianapolis, Cleveland, and Minneapolis have the lowest rates of financial misconduct in our sample, with an average annual fraud rate of 0.6% less than half the overall average of 1.3%. At the other extreme, Texas is home to two of the three highest offenders in Dallas and Houston, exceeded only by Miami, the only city with an average annual fraud rate exceeding 2%. Column 3 of Table 3 formalizes such differences in a unified framework, and, as indicated by the F-statistic of 5.33 (1% threshold of 1.91), suggests that persistent differences in financial fraud among cities is highly significant.

Columns four through six combine various combinations of year, industry, and city fixed effects. In most cases, the R^2 are approximately additive, indicating that variation across cities, industries, and over time is largely independent. In the final column, all three families of fixed effects are significant with area effects, as before, easily exceeding the 1% threshold for statistical significance.

3.2 Logistic analysis

Although the fixed effects analysis in Table 3 indicates long-lived differences in the fraud propensities of firms located across different geographic regions, one objection may be a lack of firm, industry, or market-level controls. For example, firms headquartered in some location may be concentrated in a particular sector, or may differ in capital structure, performance, size, or other factors potentially related to fraud incentives. To address this concern, we estimate logistic models of firm-level fraud events:

$$Pr(Fraud_{j,t}^{i,a}) = \frac{1}{1 + e^{-(\delta + \beta_1 Fraud_{p,t}^{-i,a} + \beta_2 Fraud_{p,t}^{i,a} + \beta_3 Fraud_{p,t}^{i,-a} + \beta_4 Controls_{j,t-1}^{i})}}. \tag{1}$$

Here, $Pr(Fraud_{j,t}^{i,a})$, is the probability of firm j being investigated for financial misconduct in year t, and as before (and throughout the paper), subscript i refers to Fama and French-12 industry classification, and a to economic area. The main coefficient of interest is β_1, measuring whether, at a given point in time (t), firm j is more likely to commit fraud when the average of a portfolio (p) consisting of local (a) firms *outside* its industry ($-i$) indicates a highly corrupt environment. We distinguish this (the fraud rates of a firm's local, different-industry peers, $Fraud_{p,t}^{-i,a}$) from that of its local, same-industry peers ($Fraud_{p,t}^{i,a}$), captured by β_2. Together, these coefficients capture the extent to which a firm's (potentially time-varying) local environment influence the likelihood it engages in financial misconduct.

As mentioned above, the main benefit of estimating Equation (1) is the ability to control for various firm, industry, and market factors potentially correlated with a firm's location. Accordingly, we include as a control variable the yearly average of fraud rates for firms in the same industry (i), but located outside any of our 20 cities. Yearly fluctuations in the $Fraud_{p,t}^{i,-a}$ capture industry dynamics, implying that any local effects (β_1 and β_2) are identified net of these. Additional $Controls$ include the average fraud rates of firms in the overall market, as well as various firm-level characteristics: one-year lagged stock returns, total assets, market-to-book ratio, and profitability ($\frac{EBITDA}{Assets}$).

To give a specific illustration of our methodology, and provide some intuition for what each coefficient measures, suppose that we are trying to predict the likelihood that San Francisco Bay Area technology firm Google commits fraud in a given year (say 2005). In this case, we would control for the fraud rates in the technology sector, measured outside the Bay Area in 2005, for instance Seattle-based Microsoft or IBM (headquartered in New York), captured by β_3. We also control for the overall rate of corporate fraud, including the thousands of firms outside any of the twenty largest cities (Austin's Whole Foods, Arkansas's WalMart, Memphis's Federal Express, and so on). After controlling for these, as well as Google's fundamentals like recent stock returns and size, we would be interested in whether local firms – both in and outside the technology sector – predict Google's fraudulent activity. Local SF firms outside the technology industry might include clothing retailer Gap, food producer Del Monte, or pharmaceutical-biotechnology firm Genentech (β_1). Yahoo! would be an example of a firm sharing both Google's industry and location (β_2).

Consider the results presented in Panel A of Table 4. In the first column, our estimate of β_1 is 8.12, with a t-statistic of 4.79. This indicates that an increase of 1% in the contemporaneous fraud rates of a firm's local, non-industry peers increases the odds ratio of it committing fraud by about $1 - e^{.0812} \approx 8.5\%$. Against a baseline average fraud rate of 1.46%, this would imply a fraud rate of about 1.59%, with an equal sized reduction (to about 1.31%) for a one percent decrease in surrounding firms' average fraud rates. The interquartile range of $Fraud_{p,t}^{-i,a}$ is 0% to 1.72%, translating to a shift of about 17% in the baseline average.

Also, though not our main focus, note that most of the control coefficients are intuitive. Larger firms are more likely to be prosecuted for fraud (the payoff is likely larger from investigating), as are growth firms (who likely have more discretion in determining earnings). Stock returns are high prior to fraud investigations, which is consistent with fraudulent accounting being, at least temporarily, effective in fooling the market.

In the second column, we estimate firm-level fraud sensitivities to industry fraud rates. With an estimated coefficient of about 13 ($t = 4.29$), the industry effect is larger, though not dramatically, than the area effect. The third column considers firms in the same industry *and* area. Here, the coefficient is significant, but the magnitude is small. Column 4 includes all three fraud portfolios in the same specification, with all three maintaining statistical significance at the 1% level. Taking this column as the most representative of the underlying behavior, the two local portfolios contain about as much information as does the single, non-local industry portfolio. Moreover, most (about 80%) of the significance of the local portfolios comes from firms outside the firm's dominant sector.

The final column shows the results when the fraud portfolios are converted to discrete variables, like the firm-level fraud indicator. In each case, "High Fraud" takes a value of one if the average fraud rate for the respective portfolio exceeds 1.2% (the sample median across all three), and zero otherwise. As seen, the coefficients are relatively similar, though the estimated magnitudes of the two area portfolios is slightly larger than the industry portfolio. The effect of the local, non-industry portfolio indicates that for values about 1.2%, fraud rates are elevated by about 46%, or about 67 basis points against a benchmark average fraud rate of 1.46%.

Moving to the bottom panel (B), we conduct similar analysis, but instead, consider only the first year of each corporate fraud event, denoted *FraudInitiations*. To appreciate how this variable is constructed, if a firm is *ex post* prosecuted for financial misconduct involving the years 1997, 1998, and 1999, *FraudInitiations* takes a value of one only in 1997, and zero otherwise. We apply this convention to both the dependent and explanatory variables, allowing us to specifically focus on the initial decision to engage in corporate fraud.

Compared to Panel A, there are two main differences. First, the effect of a firm's local, non-industry neighbors on fraud initiations is larger. Although the average rate of fraud initiations is (of course) lower than when both initiations and continuations are jointly considered, the estimates in Panel B indicate that a 1% increase in fraud initiations by a firm's local neighbors increases by about 26%, over twice the magnitude observed in Panel A. Second, neither the local nor non-local industry portfolio seems to matter much, although the pure industry portfolio is marginally significant. Although one might suspect low power for these portfolios given that fraud initiations are relatively rare, this concern should also apply to the local, non-industry portfolio, which indicates very strong effects.

4 Why does location matter for corporate corruption?

Together, the results in Tables 3 and 4 indicate the presence of local fraud waves that induce spatial correlation in the tendency for firms to engage in financial misconduct. In this section, we seek to better understand the reason underlying such patterns.

In his description of what he calls the "reflection problem," Manski (1993) identifies three possible reasons. First, firms and/or people living in certain areas can differ in *exogenous* ways that influence their tolerance of corruption, such as demographics, religion, or wealth. Given that the results above should be interpreted primarily as cross-sectional patterns,[9] it remains possible that we are simply picking up static, or at least very long-lived, attributes of people living in different cities. A second possibility stems from differences in *context*, whereby a common "treatment" generates correlated behavior. In our case, enforcement – to the extent that it has a local component – could be viewed as a contextual effect, as regions dedicating more resources to fight corruption might be expected to have less of it. Finally, managers may influence one another through endogenous, peer-to-peer effects. Here,

[9]Note that the analysis in Table 4 did control for time effects, but we did not control for time effects *within* cities, which is what is needed to address exogenous locational effects.

it is the behavior of one's peers, per se, that allows the tendency for financial misconduct to spread within a region.

In this section, we present two sets of analysis that hopefully make some headway on this distinction. Our first analysis, presented in subsection 4.1, tests for a correlation, both across cities and within cities over time, between corporate and political corruption. Because we find a strong effect in the time series, this pattern is difficult to square with quasi-fixed exogenous city variables like wealth or demographics. Further, because politicians and corporate managers operate in nearly independent arenas, with different rules, and (usually) different bodies of enforcement, it would seem difficult for local differences in enforcement to explain these correlations.

However, because there are – at least theoretically – time varying local shocks capable of simultaneously influencing the fraud incentives of managers and politicians, we develop further analysis intended to be immune to any generic local contextual effect. In subsection 4.2, we design tests that subject some (but not all) firms in a region to a quasi-exogenous, *non-local* shock that increases their propensity to commit fraud. Then, we look for spillovers to other firms headquartered nearby, i.e., those not originally subject to the shock. The results of this analysis provides fairly direct support for a peer-effects interpretation, whereby the tendency to engage in fraud diffuses throughout a region through social interactions.

4.1 The correlation between corporate and political corruption

In this section, we test whether cities ranking high in corruption corruption also rank high in political corruption, and more specifically, whether these ebb and flow together within a given region over time. If they do, we take this as very strong evidence of exogenous effects, since they cannot explain the time series correlation. Moreover, such analysis represents fairly strong evidence against contextual effects, such as the rotation of SEC officers between local offices. While this may influence firms' incentives to engage in fraud, the SEC has nothing to do with prosecutions of political fraud (as this is processed by the DOJ). As such, local changes in SEC enforcement are not a plausible explanation for time-series local correlation in political and corporate fraud. On the other hand, this test is not completely immune to any generic local shock, as is the analysis that follows in subsection 4.2, thus the qualifier "fairly strong."

Returning to Table 2, note the strong, positive relation between corporate and political corruption ($\rho = 0.31$); also observed in Figure 1. Shown for each city is the time-series average rate of federal convictions of public officials for crimes such as electoral fraud, conflicts of interest, campaign violations, and obstruction of justice.[10] Glaeser and Saks (2006) were the first to link this measure of corruption to economic variables, finding not only that wealthier and more educated states are less corrupt, but also that increases in corruption foreshadow slower growth.

To more formally characterize the relation between political and corporate corruption, we estimate the following logistic regression:

$$Pr(Fraud^{i,a}_{j,t}) = \frac{1}{1 + e^{-(\delta + \alpha_1 PolCor^a + \alpha_2 Controls^i_{j,t-1})}}. \tag{2}$$

Here, $Pr(Fraud^{i,a}_{j,t})$, is the probability of firm i being investigated for financial misconduct in year t, and as before (and throughout the paper), subscript j refers to Fama and French-12 industry classification, and a to economic area. The coefficient of interest, α_1, measures the extent to which the propensity for financial misconduct is related to Glaeser and Saks' (2006) area-level measure of political corruption, $PolCor$. Firm-level $Control$ variables include one-year lagged stock returns, total assets, market-to-book ratio, and profitability ($\frac{EBITDA}{Assets}$).

The results of this estimation are presented in Panel A of Table 5. In the first column, we relate the probability of corporate fraud to the time series average value of political corruption for each area, denoted $\overline{PolCor}^a$. The coefficient is 0.0395 ($t = 2.55$), translating

[10]Data are reported by the Justice Department's Report to Congress on the "Activities and Operations of the Public Integrity Section." DOJ's website (`http://www.justice.gov/criminal/pin/`) also describe which years for which the data are available. Figure 1 omits Orlando, as it does not have a DOJ district headquarter office, unlike the other nineteen cities in our sample. The Middle District of Florida serves Orlando, but is headquartered in Tampa.

to an increase in the odds ratio of $e^{0.035} \approx 3.6\%$, confirming the graphical evidence in Figure 1 that cities ranking high in political corruption also rank high in corporate corruption.

The next columns present the results when the political corruption variables non parametrically. $High\overline{PolCor}^a$ is an indicator for the quintile of most politically corrupt cities, and $Low\overline{PolCor}^a$ an indicator for the quintile of least corrupt cities.[11] The coefficient on $High\overline{PolCor}^a$ is 0.119 ($t = 1.77$), indicating that relative to the middle three quintiles, the odds ratio for firms headquartered in the most politically corrupt cities is elevated by $1 - e^{0.119} \approx 12.6\%$. By contrast, the magnitude is over twice as large (in absolute value) and of opposite sign for the least corrupt cities ($t = 2.83$). Taking the difference between these coefficients, the difference in the odds ratio is $1 - e^{0.38} \approx 46.2\%$, when evaluated at the mean values for all other covariates in Equation (2). This translates to a percentage change in $Pr(Fraud_{j,t}^{i;a})$ of about sixty basis points, 41% of the overall average corporate fraud rate of 1.46%.[12]. Note that this difference is virtually identical to that implied by Figure 1 (raw fraud rates), suggesting that the persistent variation between the least and most politically corrupt cities is mostly orthogonal to firm, industry, and market controls.

4.2 Ruling out contextual effects with instrumental variables

In this section, we present analysis that cannot be explain by area shocks, and thus, represents fairly direct evidence that peer effects, at least in part, play a role in the local diffusion of corporate corruption. In order to rule out exogenous shocks, the tests in this section exploit the fact that some of our cities – four to be exact – have over 30% of their total market capitalization concentrated in a single industry: Houston (energy), Detroit (durables), San Francisco (software), and Atlanta (non-durables). Throughout the remaining analysis, we

[11]Cities in the $High\overline{PolCor}^a$ quintile are Washington, D.C., Chicago, Miami, and Cleveland, while those in the least corrupt group include San Francisco, Seattle, Indianapolis, and Minneapolis.

[12]The average value for $Fraud_{j,t}^{i;a}$ is 1.46%, implying a log odds ratio of $log(\frac{0.0146}{1-0.0146}) = -4.21$. Being headquartered in the decile of least politically corrupt cities reduces the log odds ratio by 0.12, implying a mean value of $Fraud_{j,t}^{i;a}$ is 1.30%, as $log(\frac{0.0130}{1-0.0130}) = -4.33$. The same calculation implies a log odds of -3.94 for the most corrupt decile, translating to a mean value of 1.90% for $Fraud_{j,t}^{i;a}$.

refer to these as the "dominant" industries for each respective city.

What makes these dominant industry-city pairs so useful is that we can use variation in *non local* factors to impose a shock on some – and crucially, only some – firms in an area to engage in financial fraud. The source of this variation is the annual average fraud rates of firms in each city's dominant industry (e.g., energy in the case of Houston), but measured outside the local area. Keeping with the Houston example, we instrument for Houston-based Apache's tendency to commit fraud using the fraud rates of Oklahoma City's Chesapeake, or California-based ARCO. The fact that we use no Houston-specific information to proxy for the fraud rates of (in this example) firms in Houston's energy sector means that time-varying, local contextual effects cannot explain any spillovers to other local firms outside these dominant sectors.

In Table 6, we formalize this test in an instrumental variables regression. We estimate a variant of Equation (2), but with three main changes. First, because we are instrumenting for local fraud rates, we cannot use maximum likelihood (previous results were estimated with logistic regressions). Accordingly, the dependent variable is a discrete indicator for whether firm j is implicated for fraud in year t. The second change is that we estimate an IV regression, where the endogenous covariate, $Fraud_{p,t}^{Dom,a}$, is the average fraud rate of firms in the city's dominant industry (e.g., Houston energy firms). The third and final change is that we estimate the second stage only for firms in one of the four cities mentioned above, but outside the dominant sector (e.g., non-energy firms in Houston).

In the first four columns of Table 6, we present the results of our analysis separately for each of our four cities. Atlanta is first, and shows no evidence of spillovers from the non-durables sector to locally headquartered firms in different sectors. Stronger evidence is seen in the next three columns, which show the results for Detroit, Houston, and San Francisco respectively. In the cases of Detroit and Houston, there is a statistically significant relationship between the instrumented fraud propensities of the relevant dominant industry, and the actual fraud propensities of firms outside thee dominant sectors. San Francisco has nearly the same point estimate (0.63) as Detroit, but a p-value of only 0.25.

The next three columns show the results of aggregate specifications that combines all four cities into a single specification. Column five includes only firm fixed effects, but to facilitate comparisons with the four city-level regressions, does not include time dummies.[13] The estimated coefficient of 0.52 ($t = 2.31$) indicates that a 1% change in predicted fraud for firms in a city's dominant sector translates to a .52% change in the likelihood of fraud for a given firm outside the sector. In the last two columns, we present the results when year (column 6) and industry*year (column 7) fixed effects are added to the regression, either of which strengthens the result somewhat.

5 Implications of local fraud waves

We conclude our analysis by considering the extent to which local fraud waves extends to other firm outcomes. In subsection 5.1 we present evidence that firms surrounded by waves of corporate fraud are – even if they do not commit fraud themselves – more likely to suffer poor future performance, even to the point of declaring bankruptcy. Subsection 5.2 extends this reasoning to the city level, where we find that fraud rates in an area are a strong determinant of its employment rate in future years. We conclude with an examination of stock prices in subsection 5.3. Here, our focus is not so much on the announcement returns of violators themselves (i.e., firms targeted by the SEC or DOJ), but on those of neighboring firms. Given that fraud appears to have consequences beyond the immediate violators, the question is whether the stock market understands these externalities, and incorporates it into securities prices.

[13]When the regression is run one city at a time, year dummies are perfectly collinear with instrumented fraud propensities of the dominant industries, resulting in the dummy trap.

5.1 Failure

An extensive literature in accounting probes the link between accounting quality and measures of future firm performance.[14] Numerous studies have shown that poor quality and/or opaque accounting practices impedes outsiders' abilities to evaluate the firm (e.g., institutions, analysts), increases its cost of raising external finance, and ultimately impairs investment efficiency.

In this short section, we relate financial misconduct, the most egregious and intentional instances of poor accounting, to the likelihood that a firm declares bankruptcy. To do so, we build on the logistic regression developed by Campbell et al. (2008), which identifies a number of firm characteristics associated with bankruptcy. Our interest is whether, in addition to these factors, the fraud rates of companies surrounding a firm can provide additional information about its chance of surviving.

There are at least two reasons to suspect that it might. First, as we have already seen, being surrounded by financial misconduct makes a firm more likely to engage in misconduct itself. Thus, the fraudulent activity of a firm's neighbors provides information about the *threat* of future misconduct, even for firms not having yet engaged in any wrongdoing. Such a threat may damage its relationships with financiers, customers, or other stakeholders, and thus, put additional downward pressure for struggling firms. A second possibility is that local fraud waves induce a response on the *supply side*, whereby local capital withdraws temporarily when corruption in a region increases. In this instance, even firms "resistant" to local peer effects may nonetheless suffer consequences, especially when local capital is required.

Table 7 presents the results. In the first column, we replicate the analysis by Campbell et al. (2008). The identified predictors have similar effects in our sample, with size, net income, stock returns, cash holdings, market-to-book ratio, and stock price all being

[14] For an excellent review of this literature, including measures of accounting quality, see Dechow et al. (2010).

negatively associated with bankruptcy, and return volatility and leverage having a positive effect. In the second column, we include these legacy variables, but add $Fraud_{p,t}^{-i,a}$ the financial misconduct rate of firm j's local neighbors, but outside its FF-12 industry. The estimated coefficient indicates that a one percentage point increase in city-level fraud rates increases the probability of default or performance-based delisting by about 5.5% (t=5.71). This odds ratio settles to 2.99 (t=2.85) in column 3, when we include the fraud rates outside a firm's industry and area, accounting for the aggregate fraud rate in the U.S. economy.[15]

The final three columns explore whether the effects of an area's fraudulent activity are strongest for firms already closest to financial distress. As discussed above, because struggling firms may already face trouble raising external capital, any factor that makes lenders wary (here, the threat of a fraud wave) is likely to have a disproportionate impact on these companies. If so, then the impact of fraud rates on bankruptcy and delisting should be magnified for firms already teetering on financial distress.

The data support this conjecture. In column 4, we run the same regression as in column 3, but only for firms below the median debt-to-asset ratio for the sample (about 0.4). Column 5 shows the results for firms with debt ratios above the median. The first thing to note is that with the exception of cash holdings and volatility, the marginal effects for the remaining control variables are very similar across the two subsamples.

What differs is the importance of local fraud rates on the probability of bankruptcy and/or delisting. For firms with moderate leverage ratios, we find no evidence that these matter at all. In fact, the point estimate on $Fraud_{p,t}^{-i,a}$ is negative, but with a minuscule t-statistic. On the other hand, firms with leverage ratios above 0.4 are very sensitive to local rates of financial misconduct: a one percentage point increase in local fraud rates increases the probability of being delisting for performance reasons or declaring bankruptcy by almost 5%, with a t-statistic of 3.50. The final column formalizes this difference in a

[15]Year dummies cannot be estimated consistently with in non-linear discrete choice models, due to the "incidental parameters" problem originally formalized by Kalbfleisch and Sprott (1970). Note also that Campbell et al. (2008) does not use year fixed effects; see their discussion on pages 2916-2917.

unified regression. Here, the coefficient of interest is the interaction between $Fraud_{p,t}^{-i,a}$ and leverage (TLMTA), which is significant at better than the 1% level.

5.2 Local unemployment

If waves of local fraud foreshadow firm failure as indicated in Table 7, one further implication should be decreased employment in a region impacted by a recent wave of financial misconduct. To test for this, we switch to observations at the city-year level, reducing our sample to 20*39 = 780 observations. In this analysis, the dependent variable is employment growth, measured using U.S. Census data for each city in our sample. The key covariate of interest is $Fraud_{t-1}^{a}$, the average one-year lagged fraud rates in each city, aggregated across all industries.[16] We report the parameter estimates in Table 8.

In Panel A, the estimates indicate that the average fraud rate of each city has a strong, negative relation to employment growth in the following year. Whether we include no (column 1), area (column 2), year (column 3), or city and year fixed effects (column 4), the sensitivity between fraud rates and future employment growth remains statistically significant, with fairly stable magnitudes in the range of -0.1 to -0.2. To put this in perspective, a one standard deviation of city-level fraud rates, about 1.3 percent, is associated with a decrease in future unemployment growth of more than 0.18 percent. Against a baseline average of 1.65 percent for employment growth, this represents a change on the order of 11%.

Given that both rates exhibit considerable persistence at the city-level, one potential concern is that employment growth leads, rather than lags, city-level fraud rates. Accordingly, in column 5, we include the one-year lag of each city's employment growth as a predictor. Perhaps unsurprisingly, last year's employment growth rate has a strong impact on this rate in the current year. However, note that the impact of city-level fraud rates is minimally impacted, with a magnitude of -0.11, and a robust t statistic of three.

[16]Technically, this differs from the covariate estimated in Table 7, $Fraud_{p,t}^{-i,a}$, which is defined relative to each firm i. Because the observation here is at the area-year level, the relevant fraud variable does not change within cities. With an average of over 200 firms in each city, this difference is negligible.

Further evidence that fraud rates lead employment growth can be gleaned from Panel B, which analyzes the predictive ability of further lags of city-level fraud rates. Generally, allowing the impacts of local fraud waves to manifest over several years gives even stronger results. Statistical significance is generally (again, with the exception of the third column) very strong, with sensitivities perhaps 50% higher compared to the first four columns. Allowing the last three years' fraud rate to predict employment growth, the typical area can expect a reduction in employment growth of about -0.2 to -0.3%. Taking the estimates in the final column (which include lagged employment growth as a regressor), a one standard deviation in three-year fraud rates is associated with a 0.24 percentage change in future unemployment growth, or about 8.82% of its sample standard deviation.

5.3 Stock prices

We conclude by exploring stock return patterns around the *announcement* of legal investigations into financial misconduct. In these tests, we are not so much interested in the stock price reactions of the firms actually being targeted, but instead, on whether or not the market anticipates the negative externalities a firm engaging in financial misconduct imposes on surrounding firms, such as elevated probabilities of fraud (Table 4) and bankruptcy (Table 7), and incorporates these into prices.

Table 9 shows the results. Panel A summarizes announcement returns for firms targeted by the SEC and/or DOJ for financial misconduct. Confirming prior research including Karpoff, Koester, Lee, and Martin (2013), we find that the initiation of fraud investigations are associated with large, negative, and highly significant stock returns. The median return is -11.62%, with a mean of -18.10% (t=-18.10).

Of greater interest to us, however, is the extent to which these announcements impact neighboring firms who, at least immediately, are not themselves targeted for fraud investigation. Panel B presents the results of this analysis. Although the point estimate is negative, the magnitude is very small (-4 basis points), as is the statistical significance (t=-1.15).

Thus, at least for the typical firm, news that a neighboring firm is being investigated for fraud has a minimal impact on its stock price.

A much different picture emerges, however, if we focus on the stock price reactions of neighboring firms that *are* subsequently investigated for fraud themselves. This is, of course, a much smaller set: for every firm implicated for financial misconduct, an additional two neighboring firms will be targeted for SEC/DOJ action over the following year. Panels C1 and C2 show the results. In panel C1, we simply adjust announcement returns by the market, indicating an abnormal return of negative 89 basis points (t=-2.55). In panel C2, we subtract the returns of all *non-locally headquartered* firms investigated for fraud over the following year, finding a similar magnitude (point estimate of -0.84, t=-2.32). This second normalization eliminates any non-local "bellweather" effects for the firm originally targeted for fraud, as the following example hopefully clarifies.

Suppose that in 1995, Seattle-based Boeing is investigated for financial misconduct, and that Starbucks (also based in Seattle) is subsequently investigated in 1996. The results in Panel A suggest that on average, Boeing's stock price will drop -18% upon being targeted in 1995, but that the typical Seattle firm (say, Nordstrom) is not impacted. Panel C1 indicates that Starbucks, which is later investigated for fraud in 1996, reacts to Boeing's fraud investigation in 1995, on the order of negative one percent relative to the overall market. The remaining concern, however, is that companies with linkages to Boeing – having the same auditor for example – may suffer an immediate price decline, but not due to local factors. By subtracting off the 1995 stock returns of *non-Seattle* firms later investigated for fraud in 1996, this alternative is eliminated.

6 Conclusion

Taken together, the results in this paper suggest that local factors are important determinants of financial misconduct. Some of these factors appear to be persistent given that we find substantial differences in average fraud rates between cities over a 40-year time period. Candidate explanations in such cases are inherently slowly trending, including differences in religiosity (which vary widely across states, see Galluo (2012)) and/or measures of ethical norms. One potential source of such disparities may be country of origin, particularly that cross-nation disparities in tolerance of corruption is so large (Transparency International (2013)), and because these differences appear to persist over many generations.

On the other hand, higher frequency trends in financial misconduct within cities – i.e., year to year – is not as well explained by such quasi-static factors. In such cases, peer effects seem particularly plausible, whereby one firm's willingness to engage in fraud influences the tendencies of others to follow suit. Perhaps the most direct evidence for peer effects uses non-local industry shocks (such as changes in oil prices) to "shock" some of an area's firms' incentives to engage in fraud (such as energy firms in Houston), and then look for spillovers to local firms outside this sector. This methodology removes the impact of local, area shocks, leaving only endogenous interactions between neighboring firms as the only feasible mechanism.

What our tests cannot definitely establish is the specific motivation underlying such firm-to-firm peer influences. For example, perhaps *information* related to financial misconduct spreads throughout a region – think about a particular accounting technique being learned from one's neighbors. Or, perhaps managers might have the desire to preserve *relative consumption* and/or status; provided that fraudulent accounting influences stock prices, earnings, or other determinants of managerial compensation, perhaps a "keeping up with the Joneses" type motivation could generate local waves of misbehavior. Lastly, the tendency to engage in misconduct may be partly determined by *dynamic social norms*, and an associated stigmatization from being caught. If the social stigma is lessened when many firms are engaging in misconduct, positive feedback may occur.

Clean distinctions between these three micro-variants of peer effects are generally not possible, and in fact, all likely play some role in the diffusion of corporate corruption within a region. However, we note two observations to conclude. First, the cross-sectional and time-series correlation between corporate and political corruption (Table 5, Figure 1) is hard to square with a purely information story. Presumably, the techniques associated with "cooking the books" in a financial conduct are fundamentally different than those utilized to manipulate an election, take a bribe, or interfere with a federal investigation.

Second, as for relative consumption/status motives, note the inconsistency with our previous findings regarding the performance of a city's dominant sector. Recall that, and using Houston as an example, fraud rates of energy firms is highest when the energy sector is performing poorly. Accordingly, if relative status is the motivation for neighboring firms' behavior, this would predict *lower*, not higher, rates of misconduct in the area. In both cases, time-varying ethical standards that span both the political and corporate sector seem, at least to us, more plausible.

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Figure 1: Political and Corporate Frauds

This figure reports the scatterplot of financial misconduct rate and political corruption measure. The numbers used to generate this scatterplot are reported in Table 2. The straight line depicts the best-fit line.

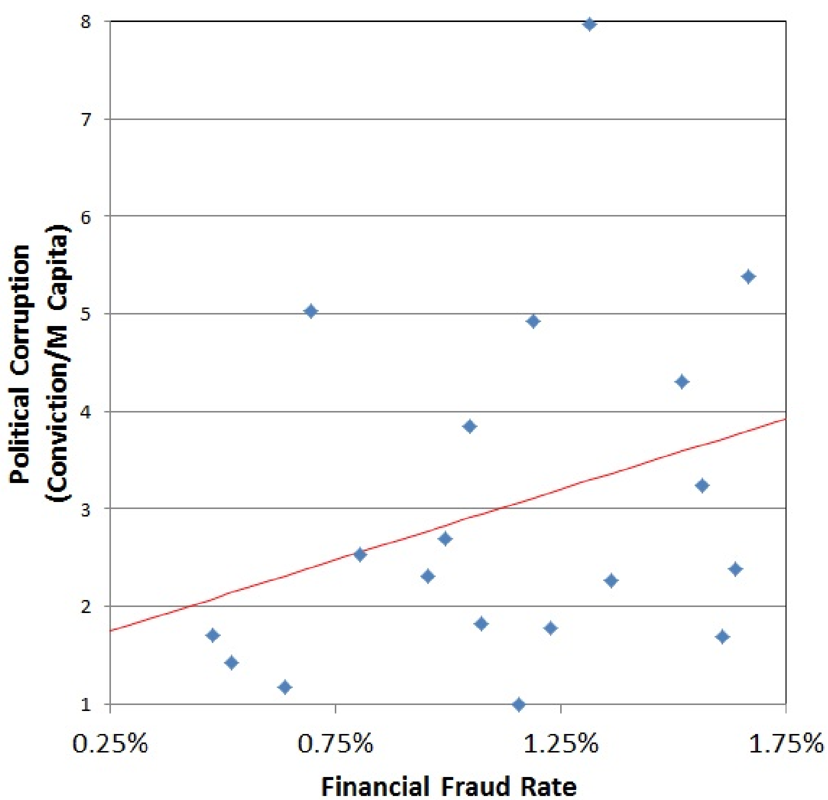

Table 1: Summary Statistics

This table contains summary statistics related to our fraud measures. Panel A presents variables defined at the firm-year level, while Panels B and C show those defined at the city-year and industry-year level, respectively. At the firm level, $Fraud^{i,a}_{j,t}$, is a dummy variable denoting financial misconduct by firm j, operating in industry i, in area a, during year t. $FraudInitiations$, is a dummy variable which takes a value of one during the first year of a financial misconduct event, and zero otherwise. $Bankruptcy$, is a dummy variable which takes a value of one if the firm experience default or delisting due to performance issues during the year, and zero otherwise. At the city (Panel B) and industry (Panel C) levels, fraud, initial fraud, and bankruptcy are defined using rates instead of dummy variables, e.g., the average fraud rate for area a in year t is simply the sum of $Fraud$ in area a during year t, divided by the number of firms headquartered in area a that year. The same applies to industry-level averages. In Panel B, $PoliticalFraud^a_t$ is the count of prosecutions of elected and appointed public officials at all levels of government and/or of election crimes (per million of population) in area a during year t. We report time-series averages of cross-sectional summary statistics.

Panel A. By Firm-Year					
Variable	**Mean**	**Std. Dev.**	**25^{th} Pctl.**	**Median**	**75^{th} Pctl.**
$FraudInitiations^{i,a}_{j,t}$; Indicator Variable	0.0034				
$Fraud^{i,a}_{j,t}$; Indicator Variable	0.0146				
$Bankruptcy^{i,a}_{j,t}$; Indicator Variable	0.0627				
Stock Characteristics					
Lagged Stock Return	0.0783	0.5071	-0.2643	0.0271	0.3393
Lagged Asset (Logged)	4.8992	2.0220	3.3376	4.7739	6.3669
Lagged Leverage	0.3109	0.2879	0.0000	0.2747	0.5443
Lagged Q	1.4026	1.1042	0.6821	0.9980	1.6976
Lagged Cash Flow / Asset	0.0492	0.1421	0.0113	0.0763	0.1344
Campbell et al. (2008) Variables					
Size (RSIZE)	-10.2374	2.1776	-11.7839	-10.3477	-8.8086
Net Income (NIMTA)	-0.0223	0.2590	-0.0211	0.0232	0.0473
Leverage (TLMTA)	0.4288	0.2668	0.1967	0.4054	0.6401
Excess Return (EXRET)	0.0080	0.3896	-0.2850	-0.0397	0.2657
Cash Holdings (CASHMTA)	0.1038	0.1555	0.0191	0.0526	0.1259
Price (PRC)	1.9351	1.0653	1.4171	2.4744	2.7081
Volatility (SIGMA)	0.0392	0.0262	0.0220	0.0329	0.0489
Market-to-book (MB)	2.1937	2.4754	0.8060	1.4672	2.6232
Compensation Variables					
Missing Compensation; Indicator Variable	0.8415				
Fraction of Non-Cash Compensation | Missing=0	0.5301	0.2918	0.3186	0.5844	0.7753
Panel B. By City-Year					
$FraudInitiations^a_t$	0.0030	0.0066	0.0000	0.0000	0.0040
$Fraud^a_t$	0.0112	0.0140	0.0000	0.0073	0.0172
$Bankruptcy^a_t$	0.0513	0.0392	0.0174	0.0499	0.0782
$PoliticalFraud^a_t$	3.0317	2.8930	1.0012	2.1602	4.3345
Panel C. By Industry-Year					
$FraudInitiations^i_t$	0.0032	0.0058	0.0000	0.0000	0.0049
$Fraud^i_t$	0.0120	0.0133	0.0000	0.0101	0.0170
$Bankruptcy^i_t$	0.0508	0.0386	0.0152	0.0484	0.0758

Table 2: Summary Statistics, by City

This table contains summary statistics of our fraud measures for each city in our sample. $Fraud_t^a$, is the average fraud rate for area a in year t, i.e., the sum of $Fraud_{j,t}^{i,a}$ in area a during year t, divided by the number of firms headquartered in area a that year. $Bankruptcy_t^a$ is the failure rate of firms in area a in year t. $PoliticalFraud_t^a$ is the count of prosecutions of elected and appointed public officials at all levels of government and/or of election crimes (per million of population) in area a during year t. We report time-series summary statistics. Economic areas are sorted in ascending order by the mean fraud rate.

		$Fraud_t^a$					
Economic Area	Number of Firms	Mean	Std. Dev.	25th Pctl.	75th Pctl.	$Bankruptcy_t^a$	$Political\ Fraud_t^a$
Indianapolis	28.03	0.48%	1.34%	0.00%	0.00%	4.95%	1.70
Seattle	47.90	0.52%	1.12%	0.00%	0.00%	5.04%	1.42
Minneapolis	123.05	0.64%	0.82%	0.00%	0.98%	4.48%	1.18
Cleveland	76.65	0.69%	1.06%	0.00%	1.19%	3.84%	5.03
Atlanta	98.08	0.80%	0.83%	0.00%	1.11%	4.89%	2.53
Boston	219.20	0.96%	1.11%	0.00%	1.59%	4.71%	2.31
Orlando	27.78	0.98%	2.12%	0.00%	0.00%	5.80%	
Phoenix	46.25	0.99%	1.18%	0.00%	1.84%	5.62%	2.70
Philadelphia	138.63	1.05%	0.96%	0.23%	1.61%	4.49%	3.86
Detroit	68.90	1.07%	1.61%	0.00%	1.67%	4.29%	1.83
San Francisco Bay	234.55	1.16%	1.25%	0.00%	1.46%	5.27%	1.00
Chicago	180.10	1.19%	1.06%	0.00%	1.97%	4.90%	4.92
Denver	96.40	1.23%	1.40%	0.00%	2.23%	7.65%	1.78
Washington, DC	133.18	1.31%	1.22%	0.00%	1.91%	4.93%	7.97
Los Angeles	270.88	1.36%	0.73%	0.85%	1.91%	5.83%	2.27
New York	599.13	1.52%	0.99%	0.75%	1.94%	5.79%	4.30
Houston	136.83	1.56%	2.05%	0.00%	1.75%	4.82%	3.24
Dallas	154.73	1.61%	1.87%	0.00%	2.15%	5.37%	1.69
St. Louis	45.45	1.64%	1.95%	0.00%	3.06%	3.91%	2.39
Miami	105.45	1.66%	1.44%	0.00%	2.69%	6.12%	5.39

Table 3: City Effects in Financial Misconduct

This table reports the statistics of regressions predicting fraud that include various fixed effects. The dependent variable is $Fraud^{i,a}_{j,t}$. We report the fit statistics and statistical tests of the significance of each fixed effect.

	(1)	(2)	(3)	(4)	(5)	(6)
	Year FE			Year FE	Year FE	Year FE
		Ind. FE			+ Ind. FE	+ Ind. FE
			Area FE	+ Area FE		+ Area FE
Observations	113,245	113,245	113,245	113,245	113,245	113,245
Adjusted R^2	0.0054	0.0014	0.0007	0.0062	0.0065	0.0075
R^2	0.0057	0.0015	0.0009	0.0067	0.0069	0.0081
Statistical tests:						
Year FE						
F-stat	**16.776**					
Critical value for p<0.01	1.603					
Critical value for p<0.001	1.851					
Ind. FE					**vs (1)**	
F-stat		**15.468**			**12.357**	
Critical value for p<0.01		2.249			2.249	
Critical value for p<0.001		2.845			2.845	
Area FE				**vs (1)**		**vs (5)**
F-stat			**5.333**	**5.757**		**7.111**
Critical value for p<0.01			1.907	1.907		1.907
Critical value for p<0.001			2.309	2.309		2.309

Table 4: Logistic Regressions of Financial Misconduct

This table contains parameter estimates from panel logit regression predicting our fraud measure. The dependent variable in all regressions in Panel A is $Fraud^{i,a}_{j,t}$, is a dummy variable denoting financial misconduct by firm j, operating in industry i, in area a, during year t. The main dependent variables of interest are $Fraud^{-i,a}_{p,t}$, $Fraud^{i,-a}_{p,t}$, $Fraud^{-i,-a}_{p,t}$, and $Fraud^{i,a}_{p,-j,t}$. They are the fraud rates of firms located in the same area but operating in a different industry, operating in the same industry but located in a different area, operating in a different industry and located in a different area, and other firms operating in the same industry and located in the same area, respectively. In the last column, the rates are replaced with high fraud rate indicator variables, which take the value of 1 if the respective fraud rate is higher than 1.2%. In Panel B, the fraud indicators and rates are replaced with fraud initiation indicators and rates, respectively. The t-stats reported in parentheses are adjusted for clustering at the industry-year level. The significance levels are abbreviated with asterisks: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Fraud					
	(1)	(2)	(3)	(4)	(5)
Peer Fraud Variables:	Raw Fraud Rate				High Fraud Indicator
Dependent Variable:	Fraud	Fraud	Fraud	Fraud	Fraud
$Fraud^{-i,a}_{p,t}$	8.1118***			7.9641***	0.3805***
	(4.79)			(4.69)	(4.46)
$Fraud^{i,-a}_{p,t}$		13.1937***		12.6083***	0.3361***
		(4.29)		(4.18)	(4.92)
$Fraud^{i,a}_{p,-j,t}$			2.1368***	1.7517***	0.4005***
			(3.84)	(3.02)	(4.51)
Lagged stock return	0.0686***	0.0653***	0.0669***	0.0651***	0.0656***
	(3.97)	(3.90)	(3.80)	(3.95)	(4.00)
Lagged asset	0.2588***	0.2608***	0.2600***	0.2607***	0.2749***
	(12.68)	(12.82)	(12.74)	(12.83)	(13.92)
Lagged leverage	-0.1417	-0.1458	-0.1454	-0.1515	-0.0922
	(-1.32)	(-1.45)	(-1.38)	(-1.50)	(-0.90)
Lagged Q	0.2239***	0.2059***	0.2194***	0.2086***	0.2038***
	(7.40)	(8.06)	(7.52)	(8.15)	(7.67)
Lagged cash flow	0.2257	0.2119	0.2185	0.2077	0.1030
	(0.83)	(0.84)	(0.82)	(0.82)	(0.40)
Constant	-6.6508***	-6.6568***	-6.6439***	-6.6717***	-6.8049***
	(-44.37)	(-47.34)	(-45.22)	(-47.34)	(-44.81)
Market Fraud Rates	Yes	Yes	Yes	Yes	Yes
Observations	90,208	90,208	90,208	90,208	90,208
Pseudo R^2	0.0551	0.0566	0.0549	0.0580	0.0572

Table 4: Local Waves of Financial Misconduct (Continued)

Panel B: Fraud Initiations					
	(1)	(2)	(3)	(4)	(5)
Peer Fraud Variables:	Raw FraudInit Rate				High FraudInit Indicator
Dependent Variable:	Fraud Init	Fraud Init	Fraud Init	Fraud Init	Fraud Init
$FraudInit_{p,t}^{-i,a}$	23.5383***			22.6751***	0.6401***
	(3.75)			(3.66)	(3.66)
$FraudInit_{p,t}^{i,-a}$		17.8104		15.6833	0.5115***
		(1.42)		(1.23)	(3.98)
$FraudInit_{p,-j,t}^{i,a}$			3.6153*	3.2525	0.1887
			(1.82)	(1.45)	(0.98)
Lagged stock return	0.0786***	0.0752***	0.0762***	0.0770***	0.0898***
	(3.42)	(3.00)	(3.12)	(3.25)	(4.88)
Lagged asset	0.1843***	0.1850***	0.1851***	0.1839***	0.2067***
	(4.79)	(4.90)	(4.85)	(4.87)	(5.46)
Lagged leverage	-0.5358**	-0.5263**	-0.5282**	-0.5385**	-0.4380*
	(-2.29)	(-2.26)	(-2.26)	(-2.30)	(-1.85)
Lagged Q	0.1975***	0.1919***	0.1964***	0.1921***	0.2040***
	(3.79)	(3.74)	(3.77)	(3.74)	(3.90)
Lagged cash flow	0.1640	0.1482	0.1469	0.1477	0.0655
	(0.36)	(0.34)	(0.33)	(0.33)	(0.15)
Constant	-7.4918***	-7.4817***	-7.4766***	-7.4927***	-7.7538***
	(-26.09)	(-26.73)	(-26.16)	(-26.83)	(-23.47)
Market Fraud Rates	Yes	Yes	Yes	Yes	Yes
Observations	90,208	90,208	90,208	90,208	90,208
Pseudo R^2	0.0392	0.0379	0.0381	0.0407	0.0360

Table 5: The Relation Between Political and Corporate Corruption

This table contains estimates from regressing corporate fraud on political fraud. Panel A contains parameter estimates from panel logit regression predicting firm-level fraud measure. The dependent variable in all regressions is $Fraud^{i,a}_{j,t}$, is a dummy variable denoting financial misconduct by firm j, operating in industry i, in area a, during year t. The main dependent variables of interest are derived from $PolCor^a_t$, which is the count of prosecutions of elected and appointed public officials at all levels of government and/or of election crimes (per million of population) in area a during year t. In Model (1) we employ $\overline{PolCor}^a$, the time-series mean of $PolCor^a_t$. In Models (2)-(4), we use $High\ \overline{PolCor}^a$ and $Low\ \overline{PolCor}^a$, indicator variables for cities in the top and bottom quintiles of $\overline{PolCor}^a$. The t-stats reported in parentheses are adjusted for clustering at the industry-year level. Panel B contains parameter estimates from panel regression predicting city-level fraud rate. The dependent variable in all regressions is $Fraud^a_t$, the city-level average of $Fraud^{i,a}_{j,t}$ for all firms operating in any industry in area a during year t. The main dependent variables of interest are $PolCor^a_t$, which is the count of prosecutions of elected and appointed public officials at all levels of government and/or of election crimes (per million of population) in area a during year t, and its lagged value, $PolCor^a_{t-1}$. The t-stats reported in parentheses are adjusted for clustering at the year level. The significance levels are abbreviated with asterisks: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A: Firm-Level Analysis

	(1)	(2)	(3)	(4)
Dependent Variable:	Fraud	Fraud	Fraud	Fraud
$\overline{PolCor}^a$	0.0395**			
	(2.55)			
$High\ \overline{PolCor}^a$		0.1768***		0.1119*
		(2.98)		(1.77)
$Low\ \overline{PolCor}^a$			-0.3201***	-0.2670***
			(-3.61)	(-2.83)
Lagged stock return	0.0853***	0.0860***	0.0843***	0.0851***
	(4.97)	(5.02)	(4.80)	(4.84)
Lagged asset	0.2984***	0.2980***	0.3014***	0.3003***
	(14.25)	(14.27)	(14.40)	(14.33)
Lagged leverage	-0.1135	-0.1148	-0.1248	-0.1258
	(-0.97)	(-0.98)	(-1.07)	(-1.07)
Lagged Q	0.2658***	0.2645***	0.2758***	0.2758***
	(8.70)	(8.63)	(9.13)	(9.12)
Lagged cash flow	-0.2192	-0.2174	-0.2522	-0.2422
	(-0.81)	(-0.80)	(-0.93)	(-0.89)
Constant	-6.3337***	-6.2707***	-6.1841***	-6.2314***
	(-45.41)	(-46.75)	(-45.10)	(-45.15)
Observations	90,274	90,274	90,274	90,274
Pseudo R^2	0.0371	0.0374	0.0378	0.0381

Table 5: The Relation Between Political and Corporate Corruption (Continued)

Panel B: City-Level Analysis						
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	$Fraud_t^a$	$Fraud_t^a$	$Fraud_t^a$	$Fraud_t^a$	$Fraud_t^a$	$Fraud_t^a$
$PolCor_t^a$	0.0547***	0.0425*			0.0151	0.0160
	(2.80)	(1.74)			(0.67)	(0.68)
$PolCor_{t-1}^a$			0.0713***	0.0674**	0.0615**	0.0605**
			(3.45)	(2.55)	(2.39)	(2.22)
Constant	1.1120***	1.1503***	1.0667***	1.0787***	1.0492***	1.0493***
	(7.28)	(7.36)	(7.11)	(7.16)	(6.90)	(6.84)
Year Fixed Effects		Yes		Yes		Yes
Observations	613	613	613	613	613	613
R^2	0.013	0.090	0.022	0.097	0.023	0.098

Table 6: Instrumenting for Local Corruption

This table contains parameter estimates from linear probability model regressions predicting our fraud measure. The dependent variable in all regressions is $Fraud^{i,a}_{j,t}$, a dummy variable denoting financial misconduct by firm j, operating in industry i, in area a, during year t. The main dependent variable of interest is $Fraud^{Dom,a}_{p,t}$, which is the fraud propensities of firms in the dominant industry in area a, instrumented using $Fraud^{Dom,-a}_{p,t}$, the dominant industry's fraud rate calculated using only firms headquartered outside the relevant area ($-a$). Models (3) and (4) employ the lagged value of the instrument variable rather than the contemporaneous value. The t-stats reported in parentheses are adjusted for clustering at the industry-year level. The significance levels are abbreviated with asterisks: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
2SLS Stage:	1st stage	2nd stage	1st stage	2nd stage
Dependent Variable:	$Fraud^{Dom,a}_{p,t}$	$Failure^{i,a}_{j,t}$	$Fraud^{Dom,a}_{p,t}$	$Failure^{i,a}_{j,t}$
Instrumented $Fraud^{Dom,a}_{p,t}$		1.94***		0.53**
		(3.43)		(1.98)
$Fraud^{Dom,-a}_{p,t}$	0.46***			
	(7.78)			
$Fraud^{Dom,-a}_{p,t-1}$			0.53***	
			(8.87)	
Lag 1 $\text{return}^{i,a}_{p,-j,t}$	-0.00	0.01	-0.00	0.00
	(-0.14)	(0.72)	(-0.52)	(0.19)
Lag 1 $\text{return}_{j,t}$	0.00	0.00*	0.00	0.00*
	(1.64)	(1.80)	(1.45)	(1.66)
Lagged asset	0.00***	0.01**	0.00***	0.00
	(10.34)	(2.22)	(10.28)	(0.91)
Lagged leverage	-0.00	-0.01**	0.00	-0.01*
	(-0.08)	(-2.12)	(0.16)	(-1.66)
Lagged Q	0.00**	0.00**	0.00*	0.00
	(2.24)	(2.13)	(1.78)	(1.42)
Lagged cash flow	-0.02***	-0.01	-0.02***	-0.01
	(-6.26)	(-1.24)	(-5.76)	(-0.89)
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	9,775	9,775	9,722	9,722
R^2	0.111	0.322	0.125	0.397

Table 7: Probability of Failure of Local Fraud Rates

This table contains parameter estimates from logit regressions predicting firm failure following Campbell, Hilscher, and Szilagyi (2008). The dependent variable in all regressions is $Failure_{j,t}^{i,a}$, a dummy variable denoting failure of firm j during year t; failure is defined as either default or delisting due to performance reasons. In addition to the independent variables in Campbell, Hilscher, and Szilagyi (2008), we include the following variables: $Fraud_{p,t}^{-i,a}$, the fraud rate of area firms in different industries, and $Fraud_{p,t}^{-i,-a}$, the fraud rate of non-area firms in different industries (i.e., the market fraud rate excluding firms in the same area and/or industry). In Models (4) and (5), we split our sample into below- and above-median leverage. In model (6), we add the interaction between $Fraud_{p,t}^{-i,a}$ and leverage (TLMTA). The t-stats reported in parentheses are adjusted for clustering at the industry-year level. The significance levels are abbreviated with asterisks: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Full	Full	Full	Below-Median Leverage	Above-Median Leverage	Full
Dependent Variable:			$Failure_{j,t}^{i,a}$			
$Fraud_{p,t}^{-i,a}$		5.5002***	2.9946***	-0.1441	4.8543***	-3.1025
		(5.71)	(2.85)	(-0.09)	(3.50)	(-1.53)
$Fraud_{p,t}^{-i,a}$ * TLMTA						11.8939***
						(3.61)
Variables from Campbell et al. (2008)						
Size (RSIZE)	-0.1625***	-0.1549***	-0.1515***	-0.1378***	-0.1565***	-0.1506***
	(-15.38)	(-14.69)	(-14.44)	(-8.52)	(-11.27)	(-14.36)
Net Income (NIMTA)	-0.8726***	-0.8592***	-0.8259***	-0.7509***	-0.9018***	-0.8276***
	(-12.17)	(-12.05)	(-11.71)	(-8.65)	(-8.52)	(-11.71)
Leverage (TLMTA)	0.5798***	0.5712***	0.6067***	0.8253***	0.9604***	0.4114***
	(10.96)	(10.82)	(11.48)	(4.42)	(8.42)	(5.50)
Excess Return (EXRET)	-0.1709***	-0.1759***	-0.1654***	-0.1924***	-0.1472**	-0.1668***
	(-4.27)	(-4.40)	(-4.14)	(-3.56)	(-2.49)	(-4.17)
Cash Holdings (CASHMTA)	-0.4613***	-0.4695***	-0.4976***	-0.3609***	-0.6554***	-0.4925***
	(-4.92)	(-5.01)	(-5.29)	(-3.20)	(-3.79)	(-5.23)
Price (PRC)	-0.1379***	-0.1507***	-0.1645***	-0.1327***	-0.1769***	-0.1650***
	(-8.00)	(-8.66)	(-9.35)	(-4.82)	(-7.69)	(-9.39)
Volatility (SIGMA)	5.2738***	4.7568***	4.1899***	2.7808***	5.5022***	4.1822***
	(9.51)	(8.74)	(7.89)	(4.40)	(6.60)	(7.84)
Market-to-book (MB)	0.0006**	0.0006**	0.0006**	0.0005**	0.0020***	0.0006**
	(2.32)	(2.28)	(2.29)	(2.11)	(2.62)	(2.30)
Constant	-4.8111***	-4.7611***	-4.7808***	-4.5787***	-5.1666***	-4.6716***
	(-36.49)	(-36.13)	(-36.19)	(-22.28)	(-27.15)	(-34.51)
Market Fraud Rates			Yes	Yes	Yes	Yes
Observations	104,722	103,871	103,871	51,759	52,112	103,871
Pseudo R^2	0.0795	0.0794	0.0804	0.0469	0.105	0.0807

Table 8: Fraud Waves and Local Employment

This table contains parameter estimates from pooled regressions predicting area-level employment growth rates. The dependent variable in all regressions is $EmploymentGrowth_t^a$, which is the employment growth of area a in year t. The independent variable of interest in Panel A is $Fraud_{t-1}^a$, the fraud rate of local firms in year $t-1$. The independent variable of interest in Panel B is $Fraud_{t-3,t-1}^a$, the three-year moving average of fraud rate of local firms, calculated in years $t-3$ to $t-1$. Both panels include various variations of fixed effects as well as the lagged dependent variable measured at the same horizon as the lagged fraud rates: $EmploymentGrowth_{t-1}^a$ in Panel A and $EmploymentGrowth_{t-3,t-1}^a$ in Panel B. The t-stats reported in parentheses are adjusted for clustering at the area level. The significance levels are abbreviated with asterisks: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

Panel A. 1-Year Lag

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	$EmploymentGrowth_t^a$				
$Fraud_{t-1}^a$	-0.2168***	-0.2455***	-0.1470**	-0.1841***	-0.1095***
	(-3.23)	(-3.11)	(-2.25)	(-3.17)	(-2.99)
$EmploymentGrowth_{t-1}^a$					0.6244***
					(24.03)
Constant	0.0193***	0.0196***	0.0185***	0.0057***	0.0043***
	(7.36)	(21.72)	(7.29)	(8.96)	(6.37)
Area FE		Yes		Yes	Yes
Year FE			Yes	Yes	Yes
Observations	780	780	780	780	780
R^2	0.013	0.151	0.590	0.728	0.835

Panel B. 3-Year Average

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	$EmploymentGrowth_t^a$				
$Fraud_{t-3,t-1}^a$	-0.3328***	-0.3757***	-0.1940*	-0.2468***	-0.1654**
	(-4.38)	(-5.39)	(-1.80)	(-3.48)	(-2.28)
$EmploymentGrowth_{t-3,t-1}^a$					0.4172***
					(8.07)
Constant	0.0206***	0.0211***	0.0190***	0.0087***	0.0157***
	(7.39)	(26.64)	(6.63)	(7.92)	(10.11)
Area FE		Yes		Yes	Yes
Year FE			Yes	Yes	Yes
Observations	780	780	780	780	740
R^2	0.024	0.164	0.591	0.729	0.768

Table 9: Stock Returns around Fraud Revelation

This table contains the stock returns around revelations of financial misconduct. We examine the abnormal stock returns of the firm investigated by the SEC and/or DOJ for financial misconduct (Panel A) and other firms in the same area but operating in a different industry (Panels B and C). Panel B examines the market-adjusted stock returns of all surrounding firms, i.e., those located proximate to a firm targeted for SEC/DOJ action, but not targeted themselves. In Panel C, we characterize the market-adjusted return patterns of a much smaller set of local firms: those that are subsequently targeted for financial misconduct themselves. Panel C1 adjust the returns by market returns, while Panel C2 adjusts the returns by a control group of non-area firms that are subsequently targeted for financial misconduct. For the last three panels, we first aggregate within each event, and then report the summary statistics of the event mean across events. The t-statistics are reported in parentheses.

	Mean	
Panel A: Event firms (N=426 events)		
CAR(0:1) of revelation	-18.10%	(-18.10)
Panel B: Non-event firms in the same area but different industry		
Number of firms / event	259	
Fraction of CAR(0:1)>0 / event	46.51%	
Mean CAR(0:1) / event	-0.04%	(-1.15)
Panel C1: Non-event firms in the same area but different industry; caught in the next year (N=270 events)		
Number of firms / event	2.06	
Fraction of CAR(0:1)>0 / event	41.37%	
Mean CAR(0:1) / event	-0.89%	(-2.55)
Panel C2: Adjusting for control group of non-area firms caught in the next year		
Fraction of CAR(0:1)>0 / event	44.65%	
Mean CAR(0:1) / event	-0.84%	(-2.32)