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THE GEOGRAPHY OF FINANCIAL MISCONDUCT

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ABSTRACT

We find that a firm's tendency to engage in financial misconduct increases with the misconduct rates of neighboring firms. This appears to be caused by peer effects, rather than exogenous shocks like regional variation in enforcement. Effects are stronger among firms of comparable size, and among CEOs of similar age. Moreover, local waves of financial misconduct correspond with local waves of non-financial corruption, such as 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, and 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 correlations in the tendency to break rules.

This paper extends the literature by examining geographical patterns in *white collar crime*. We begin by documenting a strong geographic effect: the average rates of financial misconduct varies substantially across U.S. cities, and over time within these cities. Then, in the second part of the paper, we seek to better understand the mechanism. In particular, we try to distinguish between managers being subject to common local influences like enforcement, and managers influencing each others' behavior through peer effects. Our final analysis studies how stock prices react when financial misconduct of neighboring firms is exposed. As we will see, when a fraud event becomes public, stock prices in the area tend to dip, most strongly for firms that will later be targeted themselves.

The source of our data is the hand-collected sample of over 1,000 events of financial misconduct compiled by Karpoff, Koester, Lee, and Martin (2013). Examples of corporate misbehavior 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. See section 2 for more detail.

Our benchmark analysis shows that among the largest twenty cities in the U.S., financial misconduct is exposed at dramatically different rates. For example, averaged over 1970-2010, about 1 in 190 firms headquartered in Indianapolis, Seattle, and Minneapolis are prosecuted for misconduct in a typical year, whereas firms based in Dallas (1:62), St. Louis (1:61), and

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

Miami (1:60) are investigated nearly three times as often. Virtually none of these differences is due to industry clustering within cities, as shown in the bottom panel (B) of Figure 1.

With these basic patterns established, the next (and largest) part of the paper attempts to shed light on the mechanism, drawing heavily on Manski (1993). The first possibility is that, for lack of a better term, cities differ in terms of the "types" of their inhabitants, owing to long-standing factors like cultural origin (e.g., Minnesota being home to many Scandinavian descendants), wealth, or religion.[2] In the second alternative, what differs across cities is not so much the people, as much as their local environment, such as economic conditions or enforcement. Finally, the propensity for financial misconduct may spread within a region via peer effects, or more specifically, through interpersonal interactions.

Our attempt to distinguish between these mechanisms starts by arguing that exogenous differences among city cultures is, at best, an incomplete explanation. Specifically, in section 3.2 we extend our analysis to include time-series variation within cities and show that time series movements in the tendency to be prosecuted for financial fraud have strong regional patterns. That is, after accounting for (say) the fact that firms headquartered in Seattle have lower than average fraud rates over our sample, we find that Seattle's food and beverage providers (Starbucks), online retailers (Amazon), senior living providers (Emeritus), and software firms (F5 Networks) tend to commit financial misconduct *during the same times*, despite operating in very different lines of business. Static or slow-moving regional factors provide a poor account of such dynamics.

We next examine the potential impact of environmental factors in section 4. One possibility is that a city's prospects – think Detroit versus San Francisco – may influence a manager's incentives to invest in reputation or social capital. Yet, we find little relation between financial misconduct and measures of city health like population or income growth, and more importantly, their inclusion does not attenuate the effect of a firm's local peers (section 4.1). A second possibility is regional fluctuation in enforcement efforts. Although this provides

[2]A good example of such long-lived cultural influences can be gleaned from Fisman and Miguel's (2007) study of parking ticket violations in New York City for U.N. diplomats, an interesting laboratory because diplomats were, 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 (and remediation of any violations that do occur).

a good explanation for why financial misconduct may be exposed simultaneously within a region (which we also observe), it does not account for coordinated initiations of financial misconduct. Indeed, in section 4.2, we find that the first years of financial misconduct occurrences are highly clustered within regions, even for those that are later detected at different times.

Our final approach (section 4.3) involves tests that rule out *any* generic regional factor by construction. We begin by identifying metropolitan areas containing a single, dominant industry, such as Houston (energy) or San Francisco (software). Then, we use industry-level variation as an instrument for the fraud rates of firms in these dominant industries, e.g., using the fraud rates of Oklahoma's Chesapeake Energy to instrument for Houston's Apache. The final step is to relate fraud rates of firms outside the dominant sector (e.g., a pharmaceutical firm in Houston) to the instrumented fraud rates of the city's dominant industry players. That we find a strong relation here is difficult to reconcile with local environmental effects of any form.

In section 5, we conduct further analysis that lends more direct support to the idea that peer effects between managers is, at least in part, responsible for the observed regional correlations in financial misconduct. First, we divide a firm's local neighbors into groups, based on the: 1) similarity of market capitalization, and 2) similarity of the CEO's age. The idea of each is to identify proxies for the strength of local interaction. Matching on size seems intuitive (even across industries), given that the largest firms in an area – think Google, Wells Fargo, and Genentech in the Bay Area – are likely to share linkages on corporate boards, civic organizations, and so forth. The intuition behind age matching is similar, i.e., a CEO in his 40s is more likely to socially interact with CEOs in the same age range, versus those thirty years his senior.

Confirming this intuition, we find striking results. Large firms are sensitive *only* to the financial misconduct of other large local firms; likewise, small firms are sensitive only to the behavior of other small local firms. The results are somewhat weaker for the age-matching results, though young CEOs are roughly twice as sensitive to the behavior of other young (local) CEOs, versus their more mature counterparts.

Our second test considers the possibility that CEOs – particularly those of large public firms – likely interact regularly with elected officials. If so, and if ethical norms are transmitted through social interactions, then we might expect misbehavior in the corporate arena to correlate with misbehavior of public servants. Not only is this true on average across cities (see Figure 2), but also *over time within each city*. This relation is particularly strong for large firms, whose executives are most likely to interact with elected officials. As before, such regional ebbs and flows are difficult to explain with static regional factors, moreover, because there is little (though not zero) overlap between the relevant enforcement bodies, we interpret this as additional evidence against regionally correlated enforcement driving the results.

The paper concludes in section 6 with an analysis of stock price reactions to local waves of financial misconduct. That is, when a firm is targeted by the SEC, do stock prices of its neighbors drop, either because the market expects them to also be targeted, or because of negative externalities? The answer is a qualified yes. When examining all of an investigated firm's local neighbors, we observe a small negative announcement return, but this is not statistically significant. However, for the smaller set of neighboring firms that are subsequently investigated for fraud, the negative reaction is quite significant. Thus, the market appears capable not only of recognizing the existence of fraud waves, but also of identifying the specific local firms most susceptible.

Our results are directly related 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 contributes by identifying the behavior of a firm's local peers as a first-order determinant of corporate misbehavior, over both long and short horizons.

Our results also contribute to the literature on urban agglomeration. Beginning with Marshall (1880), economists have sought to understand the reasons behind spatial clustering of firms of individuals, most recently de-emphasizing geographical features (e.g., river access), and shifting focus to "people-based" externalities like knowledge spillovers, or pooling of labor markets that improve firm-worker matches.[3] While on net, the existence of cities suggests that the benefits of agglomeration tend to outweigh the costs, our results suggest that not all externalities are positive. For just as proximity facilitates the spread of disease, the spillover of ideas and social norms can permit the diffusion of both prosocial and antisocial behavior.

2 Data

2.1 Financial misconduct

The primary source for our fraud data is Karpoff, Koester, Lee, and Martin (2013), 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 included in their dataset, and refer the reader interested in further detail (e.g., regarding the data collection method itself and 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 "restatement" announcements, and therefore are good sources for detecting a firm's attempt to manipulate earnings.[4] The third, the SCAC, maintains a registry of Federal class action securities litigation; accordingly, compared with the data from 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 among 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

[3]See Duranton and Pagu (2004) for an excellent review of this literature.

[4]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.

a firm commits fraud (the "violation period") and the dates these actions became public (the "revelation period"). Most of our analysis focuses on the violation period and examines 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 considers $Fraud^{i,a}_{j,t}$, a dummy variable denoting financial misconduct by firm j, operating in industry i, in area a, during year t. The average value of $Fraud$ is 0.0146 across all years and firms, 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, 0.0034, indicating that 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 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 Seattle that year. The same applies to the 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 largest metropolitan areas in the United States. The specific variable we use is ADDZIP listed in COMPUSTAT, which is the current zip code of 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 that now reside elsewhere, firms move infrequently so 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

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.

3 Geography and financial misconduct

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 a simple non-parametric analysis showing that city fixed effects load significantly in linear probability models predicting firm level financial misconduct. Subsection 3.2 extends the analysis to a logistic framework, a more appropriate model given the discrete nature of the dependent variable. Here, we also control for various firm, industry, and market determinants of corporate fraud, focusing on the fraud-related activities of a firm's local peers as the covariates of interest.

3.1 Variation in financial misconduct *across* cities

As a first step, we quantify the ability of year, industry, and area fixed effects to explain the total variation 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 commits fraud or other financial misconduct in year t. For now, we include each year the firm commits fraud. We will later separately consider the first year that fraud is committed.

We are interested in the change in explanatory power as we progressively add and subtract various vectors of fixed effects in OLS regressions of firm-level fraud events. The results are shown in Table 3. The first column includes only year effects, and thus accounts for time-series effects that may influence the aggregate rate of the 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 an F-statistic equal to 16.78, far exceeding the 1% threshold. 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. These patterns can be understood 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%, which is 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 these differences in a unified framework, and, as indicated by the F-statistic of 5.33 (versus a 1% threshold of 1.91), suggests that there exist persistent differences in financial misconduct among cities.

Columns four through six report regressions that include 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 Variation in financial misconduct *within* cities

Although the fixed effects regressions in Table 3 indicate 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 regions 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:

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

Here, $Pr(Fraud^{i,a}_{j,t})$, 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 the 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 local firms *outside* its industry ($-i$) commit more fraud. Similarly, β_2 measures the influence of the fraud rate of the firm's same-industry, local peers ($Fraud^{i,a}_{p,t}$). 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. While we cannot use fixed effects in logit regressions, we include as a control variable the yearly average of fraud rates for firms in the same industry (i), but located outside the firm's city (a). Yearly fluctuations in $Fraud^{i,-a}_{p,t}$ 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, leverage, and cash flows.

To give a specific illustration of our methodology, and provide some intuition about 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 operating outside of the firm's industry ($-i$) and outside of the firm's metropolitan area ($-a$), e.g., Austin's Whole Foods, Arkansas's Wal-Mart, Memphis's Federal Express, and so on. After controlling for these, as well as Google's fundamentals like recent stock returns and size, we are 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! is 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 8.11, with a t-statistic of 4.79, which 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 $e^{.0811} - 1 \approx 8.45\%$. Against a baseline average fraud rate of 1.46%, this implies 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 variable 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 incentive to manipulate earnings because they tend to raise more capital). 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. Using the estimates in this column, the two local portfolios seem to 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 fifth column adds to the model $\overline{Fraud_p}^{-i,a}$, the average rate of financial misconduct in each city. This control accounts for the cross-city differences identified in Tables 2 and 3, and leaves our primary variable of interest, $Fraud_{p,t}^{-i,a}$ to capture variation within cities. This only strengthens the coefficient on the dynamic peer variable (t=4.76), confirming the statistical significance of the peaks and troughs *within* each contour of Figure 1 (or more appropriately, within each individual city).

To highlight the economic magnitude of these correlations, the final column (6) 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 across the three area/industry portfolios. The coefficient on the local, non-industry portfolio indicates that for local fraud rates above 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 a similar analysis, but instead, consider only the first year of each corporate fraud event, denoted as *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 displays a very strong effect.

To summarize our results thus far: financial misconduct occurs in local waves, rising and falling within cities (Table 4 and Figure 1), and the average rate of financial misconduct differs from city to city (Tables 2 and 3). Further, the dynamic nature of local fraud waves is not consistent with slow trending city-level attributes such as differences in wealth, culture, religion, or ethnic background (e.g., the high percentage of Scandinavian descendants occupying Minnesota) since these do not fluctuate appreciably year-to-year.[5] We will re-emphasize this point occasionally in further tests, but proceed under the notion that insofar as identifying the underlying mechanism, the more important distinction is between local peer effects and time-varying environmental factors like changes in enforcement. The next section deals specifically with this issue.

4 Common environmental influences

As mentioned above, the tendency for locally headquartered firms to engage in financial misconduct at roughly the same time poses a challenge to explanations based on static, *exogenous* factors. On the other hand, it is more difficult to distinguish between local managers influencing each others' behavior per se (*endogenous* effects in Manski's taxonomy), versus simply responding to common environmental (*contextual*) shocks. The goal of this section is to make headway on this distinction.

First, in subsection 4.1, we consider whether correlations in financial misconduct are

[5] In unreported robustness, we have experimented with various measures of religious participation as controls in Table 4; none approaches statistical significance, while the estimates for the peer variables remain virtually unchanged.

related to measures of city health. Using the model presented in Table 4 as the starting point, we include numerous measures of local economic conditions as explanatory variables, measured at both leads and lags. As we discuss below, fluctuations in the local economy may change the incentives to engage in financial misconduct, either directly or indirectly. Yet, these variables have very little impact on our results.

A second type of contextual effect could stem from regional fluctuations in enforcement, which is particularly relevant given that we do not observe misbehavior directly. To assess the relative importance of regional fluctuations in enforcement versus the underlying behavior, we exploit the differential timing between when financial misconduct begins, and when it is detected. Our conclusion from this analysis is that correlated enforcement does not entirely account for our main findings.

While these tests address specific types of contextual effects, they are not exhaustive. Accordingly, in our final tests, we introduce an instrument that rules out the effect of *any* local environmental influence by construction. This sets the stage for our final tests (Section 5) that provide direct support for peer effects as determinants of corporate misbehavior.

4.1 Local demographic and economic trends

This section analyzes the impact of local economic variables on the propensity for managers to engage in financial misconduct. There are a number of reasons to expect a relation. Perhaps the most direct is a type of reputation/horizon effect, whereby managers of growing cities take a longer term perspective compared to managers of struggling cities. Intuitively, investments in local reputation may have less time to pay off in declining cities, compared to cities with brighter futures.[6]

In addition, local policy variables such tax rates or subsidies to firms headquartered nearby are undoubtedly affected by local economic health. Whether firms are responding similarly to common economic shocks or to policy variables downstream of these shocks, accounting for trends in population, wages, and employment gives a sense for the importance of these

[6] Note that this may also be related to peer effects, whereby social penalties vary with the intensity of others' misbehavior; here, we consider only the role played by exogenous changes in managers' effective horizons that are a function of city health.

local factors.

To explore these issues, we augment Equation (1) with controls for population growth, employment growth, and per capita wage growth, all measured annually within each economic area. We report this analysis in Table 5. As in the prior analysis, we consider separately all (Panel A) and only first (Panel B) years involving financial misconduct separately. To ease comparison, the first column reproduces the penultimate column of Table 4. Then, in the second column, we add five controls for population growth, starting from two years prior to the event date ($t-2$) and continuing through two years afterward ($t+2$). Lagged values are intended to control for the impact of historical changes in population, whereas future values are a (noisy) proxy for expectations of future population growth. None of the population variables predict financial misconduct, and moreover, have virtually no effect on the other variables.

A similar picture emerges in the second and third columns, which incorporate, respectively, locally measured employment growth (also with two year leads and lags) and per-capita wage growth. When all fifteen covariates are included simultaneously, only contemporaneous employment growth approaches statistical significance (t=−1.95), entering with a negative sign (as expected).[7] More importantly, though modestly reduced in magnitude (9.33 versus 7.89), the coefficient on $Fraud_{p,t}^{-i,a}$ remains highly statistically significant (t=3.98).

As a further test, we split the sample by the median market capitalization, with estimates for firms above the median (*Large*) and those below (*Small*) shown in columns 6 and 7, respectively. Intuitively, the idea is that all else equal, larger firms should be less sensitive to the fluctuations in local economic factors: whereas it seems far-fetched that population growth in Atlanta could play a meaningful role in any of Coca-Cola's major business decisions, this may be reasonable for firms with more localized businesses (e.g., a small Atlanta-based consulting firm). Accordingly, the importance of local economic variables might be expected to differ between firms of different sizes.

Although the last column provides some, albeit very weak, evidence that small firms are more sensitive to local economic factors, the more important comparison between columns

[7] The fifteen covariates are jointly significant.

6 and 7 suggests that if anything, large firms are *more* sensitive to the behavior of nearby peers. Given that large firms appear completely insensitive to local economic forces (none of the fifteen covariates is significant), this result is difficult to reconcile with regional economic shocks driving the local correlations in the financial misbehavior that we observe.

4.2 Correlated Enforcement

In addition to local economic and demographic trends, a second type of local contextual effect might be enforcement efforts correlated at the regional level. Recall that we do not observe actual occurrences of financial misconduct, but rather instances when formal enforcement action is brought against a firm. Without further analysis, it is ambiguous whether a city's executives are actually committing financial misconduct during the same times, or whether they are simply caught simultaneously. The goal of this subsection is to help separate these two effects.

To better appreciate why geographically clustered "whistle blowing" may be a plausible explanation for our results, it is useful to revisit which parties typically expose financial misconduct. Dyck, Morse, and Zingales (2010) tracks 216 cases of corporate fraud in detail, paying specific attention to how these events became public. Their headline finding is that no one entity plays a dominant role: industry regulators, law firms, equity holders, the national media, and industry competitors all bring financial misconduct to light, with none being responsible for more than 20% of detections. Perhaps the most surprising result is the relatively minor role played by the SEC, which blew the whistle on merely ten cases, or 7% of the total.

For us, the most important question is whether whistle blowers concentrate their forensic efforts on specific cities, and during certain times. In a few cases, one can construct plausible stories. For example, if equity holders are biased toward hold stocks of local companies, this may increase the scrutiny on local management. (However, home bias would seem more relevant for the cross-sectional evidence, and less so for regional ebbs and flows of financial misconduct.) A similar argument might be made for auditors, if their clients tend to cluster in certain regions. Finally, although the SEC appears relatively unimportant in fraud detection, a "tough" officer rotating to a local office may heighten the chance that fraud is detected and/or prosecuted (Kedia and Rajgopal (2011)).

On the other hand, it is harder to imagine a purely geographic motive for most of the remaining players. The financial media, for example, exposed almost one-sixth of fraud cases, but this exclusively occurred at the national level (e.g., *Wall Street Journal*) rather than local level (e.g., *Houston Chronicle*). Likewise, whistle blowers in a firm's supply chain –clients, competitors, and even its own workforce– would appear motivated to expose fraud in a particular company, not in a geographic area. Industry regulators may have an incentive to concentrate in industry clusters (e.g., energy firms in Houston), but recalling that we are interested in local correlation in financial misconduct *across* industries, it is less obvious how fluctuations in industry enforcement provide a satisfactory account of our main results.

In any regard, the analysis here attempts to distinguish between local shocks to enforcement and/or whistle blowing, and local correlations in the underlying (mis)behavior by executives. Our main question: for a set of misconduct events *exposed* in the same year, are the *start* dates for these events abnormally clustered by region? For example, take the set of misconduct events exposed in the year 2006. In this particular "vintage" of fraud exposures, some will be relatively young (i.e., having started in 2005), others slightly older (starting in 2004), and others even more longstanding (2003 and before). By asking whether exposure vintages in different regions are disproportionately populated by older (or younger) events, this analysis tests for local correlations in fraud initiations, holding constant the timing of enforcement.

For this test to have any power, there must be sufficient variation in initiation dates within a given year of exposure. Fortunately, there is. Although only a few cases (about 5%) are detected in the same year as they begin, there is considerable mass in events of one (25%) and two (27%) years' duration. Duration for the remaining events range from three to eighteen years, with no single duration accounting for more than 15% of our observations (and most less than 1%). Accordingly, we focus our analysis on frauds that begin one or two years prior to detection, capturing a little more than half of our total sample.

In the first row of Table 6, we start with a simple peer effects regression of financial misconduct *exposures* similar to Equation (1), irrespective of when each event is determined to have begun. This analysis does not, of course, distinguish between correlated behavior and correlated detection, but serves as a benchmark for the following tests that do. The dependent variable is $FraudExposed_{j,t}^{i,a}$, a dummy variable which takes a value of one if a firm j's financial misconduct is exposed in year t. As before a refers to area and i to industry. Also as before, the covariate of interest ($FraudExposed_{p,t}^{-i,a}$) captures the corresponding rate for the portfolio (p) of the firm's local neighbors (a) that operate outside its primary industry designation ($-i$). The positive, statistically significant estimate ($t = 2.61$) indicates a local synchronicity for the revelation of financial misconduct. Yet, as discussed above, this is consistent with either correlated detection efforts or correlated misbehavior.

Rows two and three allow us to refine the interpretation. In the second row, the dependent variable is $FraudExposed_{j,t}^{i,a}|FraudInit_{j,t-1}^{i,a}$, an indicator variable that takes a value of one if, and only if, the financial misconduct that firm j initiated in the previous year $(t-1)$ becomes exposed in year t. The dependent variable takes a value of zero otherwise, *including time t exposures of financial misconduct determined to have begun in prior years, e.g., $t-2$, $t-3$, etc.*. The dependent variable in row three, $FraudExposed_{j,t}^{i,a}|FraudInit_{j,t-2}^{i,a}$, is defined identically, except that financial misconduct events beginning two years prior to exposure $(t-2)$ are substituted for those starting one year prior $(t-1)$.

To capture local correlation in starting dates for financial misconduct in a given exposure vintage, we construct two mutually exclusive local covariates: $FraudExposed_{p,t}^{-i,a}|FraudInit_{p,t-1}^{-i,a}$ and $FraudExposed_{p,t}^{-i,a}|FraudInit_{p,t-2}^{-i,a}$. We include both covariates in rows two and three. The identifying assumption is that if correlated whistle blowing is responsible for the common revelation of financial misconduct (as shown in column 1), then there should be little difference between the coefficients. In other words, during an enforcement "sweep" of say, Atlanta-based firms in 1998, there is no reason why frauds starting in 1997 are more likely to be exposed than those beginning in 1996, unless the actual incidence of (here) one-year frauds are more prevalent. On the other hand, correlation in the underlying behavior would predict strong significance of the "diagonals" in rows two and three, with exposure of younger (older) frauds being related to the exposure rates of younger (older) frauds of neighboring firms.

We find consistent evidence in columns two and three. In the analysis of frauds beginning one year prior to the exposure date (column 2), only local frauds also beginning one year prior seem to matter. Although not significant at conventional levels ($t = 1.57$), the point estimate for $FraudExposed_{p,t}^{-i,a}|FraudInit_{p,t-1}^{-i,a}$ is six times the magnitude for $FraudExposed_{p,t}^{-i,a}|FraudInit_{p,t-2}^{-i,a}$, the latter exhibits virtually no relation to the dependent variable ($t = 0.18$).

The evidence is considerably stronger in the third column. Mirroring the results in the second column, when predicting frauds starting two years ago, $FraudExposed_{j,t}^{i,a}|FraudInit_{j,t-2}$, the effect of other local frauds of the same age, $FraudExposed_{p,t}^{-i,a}|FraudInit_{p,t-2}^{-i,a}$, is about twice as strong compared to those starting one year ago, $FraudExposed_{p,t}^{-i,a}|FraudInit_{p,t-1}^{-i,a}$. The difference in statistical significance is even larger ($t = 6.69$ versus $t = 1.53$). Also, the difference in these coefficients is significant at the 1% level.

Overall, the evidence in Table 6 suggests that while regionally correlated whistle-blowing may provide a partial explanation for our results, executives seem to be initiating financial misconduct at the same time. Importantly, this is not direct evidence of –although is consistent with– peer effects between local managers.

4.3 Addressing environmental effects using instrumental variables

In this section, we continue this line of reasoning, but design tests intended to remedy any generic local, environmental influence. We use our earlier finding that financial fraud is related to industry as well as location fixed effects to construct an instrument that captures the effect of local peers, but is unrelated to local environmental factors. The tests in this section exploit the fact that some of our cities represent industrial clusters, having a disproportionate number of firms in a single industry. Among the twenty cities we study, four have at least 30% of their market capitalizations (averaged over the total years in the sample) concentrated in a single Fama-French 12 industry: Houston (energy), Detroit (durables), San Francisco (software), and Atlanta (non-durables).

What makes these dominant industry-city pairs useful is that we can use variation in *non-local* factors to impose a shock on some local firms – and crucially, only some – to alter their probabilities of engaging in financial misconduct. 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 New York City's Hess, or California-based Occidental Petroleum. The fact that (in this example) we use no Houston-specific information to proxy for the fraud rates of firms in Houston's energy sector means that time-varying, local contextual effects cannot explain any spillovers to other local firms outside the dominant sector. This not only addresses contextual effects for which some information is observable, such as population growth, but also those for which we lack data (e.g., rotation of SEC officials between offices).

In Table 7, we formalize this test in an instrumental variables regression. We estimate a variant of Equation (2), but with two main changes. First, we estimate firm-level fraud with a linear probability model, as logit models are not amenable to IV. Second, the sample applies only to the four cities mentioned above, and for firms outside the dominant sector (e.g., non-energy firms in Houston). 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). We instrument for fluctuations in $Fraud_{p,t}^{Dom,a}$ using fluctuations in industry level fraud rates, measured exclusively outside the local area.

The first and third columns present the first stage IV results. Whether measured contemporaneously (column 1) or with a one-year lag (column 3), annual fluctuations in financial misconduct for each dominant industry-city pair (e.g., Houston-energy) is strongly related to year-to-year fluctuations at the industry level, when measured outside the city of interest (e.g., using energy firms outside Houston). This obviates weak-instrument concerns. Note also that because we are estimating this model with OLS, the incidental parameters is avoided, permitting both firm and year fixed effects to be included. (The former explains the reduction in statistical significance for many of the firm-level characteristics.)

Columns two and four, respectively, present the results of the second stage. The contemporaneous model (column 2) indicates a large sensitivity 1.94 (t=3.43), whereas the one-year lagged model (column 4), indicates one roughly on par with the industry effect (0.53, t=1.98). While the magnitudes here are not directly comparable to Table 4 – much smaller sample, logit versus OLS/IV, fixed effects versus no fixed effects – we note that the implied spillover rates in this four-city experiment are substantially larger than those in our benchmark regressions. One possible explanation for the larger magnitudes is that peer effects are not symmetric, and that the particularly visible/salient firms in an area, such as Google (SF Bay Area), General Motors (Detroit), or Coca-Cola (Atlanta), may have a disproportionate influence "setting an example" for neighboring firms, even those operating in different sectors.

Regardless, the more important goal of this exercise is to purge the influence of contextual effects. Because the estimates in Table 7 are based solely on the fraction of variation in $Fraud_{p,t}^{Dom,a}$ attributable to non-local variation at the industry level, local environmental influences cannot explain the results. Importantly, this test effectively rules out any generic contextual effect, be it related to the local economy, local enforcement (i.e., rotation of SEC officers), local media, and so on. In the section immediately following, we provide more specific evidence of the one remaining mechanism: peer effects involving local corporate managers.

5 Who defines a CEO's peers?

The analysis in the preceding section suggests that common environmental factors are unlikely to account for the regional ebbs and flows we observe in corporate misconduct. In this section, we provide more direct evidence that such regional patterns can be attributed to social interactions involving a city's top management.

In our first set of tests (subsection 5.1), we divide a firm's local peers into two groups based on similarity of: (i) firm size, or (ii) CEO age, with the idea that managers are more likely to interact with others in the same groups. As we will see, correlation in financial misconduct is much stronger within size groups: small firms predict the behavior of (only) other local small firms, and large firms predict the behavior of (only) other large firms. We also find a somewhat stronger within CEO-age groups. Both findings are consistent with endogenous social interactions, but harder to reconcile with alternative explanations.

The analysis in subsection 5.2 asks whether cities with the highest rates of financial misconduct appear more corrupt in other dimensions – specifically in the political arena. Using city-level convictions of publicly elected officials as a measure of political corruption (Glaeser and Saks (2006)), we find strong positive correlations between financial misconduct and political fraud, both across cities and within cities over time. As we will discuss below, these results effectively rule out area fixed attributes (e.g., culture), local shocks to enforcement, or other area-level contextual effects, and thus represent our strongest causal evidence that corruption-related norms are transmitted via local social interactions.

5.1 Refining peer groups: firm size and CEO age

If social interactions among local managers are the mechanism by which financial misconduct spreads within a city, then correlations should be stronger among parties likely to be in closer contact. In this section, we explore two proxies: firm size and CEO age. Our hypothesis is that managers that have similar ages and manage firms with similar sizes tend to interact more with each other. If so, provided that social interactions influence behaviors, we should observe stronger comovement in financial misconduct within each group.

There are a number of reasons why firm size and CEO age may be related to social interactions. Starting first with firm size, studies using the BoardEx database indicates that executives of large firms are much more likely to sit on boards of nearby companies and/or have leadership roles in local civic organizations (e.g., Engelberg, Gao, and Parsons (2013)).[8] Consequently, when an executive of a large firm joins (say) a local board, the social connections formed are disproportionately with other large-firm executives. Another possibility is that local peer groups may form along income cohorts. Because firm size is such a strong determinant of executive compensation, sorting on size is akin to a noisy sort on pay. If the wealthiest of a city's inhabitants concentrate in certain neighborhoods, restaurants, country clubs, etc., it is easy to see how firm size likely provides information about the social contact.

[8]BoardEx creates 'synthetic CVs' for thousands of firm executives and directors, allowing researchers infer common overlaps in schooling, past workplaces, or social organizations.

Our second proxy is the CEO's age, also seems intuitive given that social connections form during school (see, e.g., Cohen, Frazzini, and Malloy (2010)) as well as previous employment. We expect that the probability of two CEOs interacting socially is likely to be correlated with whether they are close in age.

Table 8 presents the empirical estimates when a firm's neighbors are segregated as described above. Panel A presents the size split, and Panel B the age split. Within every year, we rank firms from largest to smallest, taking those above the yearly median as $Large$, and those below the median as $Small$. Then, rather than running logit regressions of financial misconduct on a single portfolio of a firm's local, non-industry peers ($Fraud_{p,t}^{-i,a}$), we estimate the sensitivity to two, mutually exclusive covariates, $Fraud_{large,t}^{-i,a}$ and $Fraud_{small,t}^{-i,a}$.

Column 1 shows the estimates only for large firms, and column 2 only for small firms. In both cases, a clear pattern emerges: each group is sensitive to the behavior of its size-matched counterparts, and completely insensitive to those in the other group. The third column aggregates all firm-year observations together, and aggregates the diagonal elements of the prior column (Small-Small, and Large-Large) into a single $Match$ variable; all other observations are termed *Diff Size*. Confirming the patterns observed in columns 1 and 2, $Match$ coefficient is highly significant (t=7.07), whereas the portfolio involving firms of different sizes is not statistically different from zero (t=−1.40). The difference between the two estimates is highly statistically significant. Column 4 adds the overall time-series average fraud rate for each city (as we did in Table 4), with minimal change to the coefficients of interest.

Moving to Panel B, we conduct the same exercise, first for firms with young CEOs in column 1, where *Young* is defined as being 55-years old or younger at the observation year, and then for *Old* CEOs in column 2. Before describing the results, note the dramatic reduction (about 80%) in sample size relative to Panel A, due to the fact that we observe CEO ages only recently (post 1992) and only for firms in the EXECUCOMP database.

This caveat notwithstanding, the evidence is still broadly consistent with the size-matched results. Though neither portfolio is significant for older CEOs (with nearly identical point estimates), *Young* CEOs appear nearly twice as sensitive to the behavior of other young CEOs, though this difference is not statistically significant. When all observations are pooled in column 3, a similar pictures emerges: the point estimates are much larger (and significant) for the *Match* group, compared to the portfolio comprised of CEOs of different age.

5.2 Firm executives and local politicians as (corrupt) peers?

In this section, we test whether cities ranking high in corporate corruption also rank high in political corruption, and more importantly, whether these covary over time within a given region. This test has two motivations. First, it provides strong falsification against variation in regional enforcement (e.g., rotation of SEC officers) driving our main results, as local authorities play little to no role in the enforcement of political corruption.[9] Second, by broadening the set of peers to those in a completely different arena (politics), this test helps us better understand whether it is the transmission of information (i.e., "how to cheat?") or social norms (i.e., "how acceptable it is to cheat?") that generates the correlations in misbehavior we observe.

Regional data on political corruption are reported by the Justice Department's Report to Congress on the "Activities and Operations of the Public Integrity Section."[10] The types of activities prosecuted include electoral fraud, conflicts of interest, campaign violations, and obstruction of justice. 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.

An important advantage of this corruption measure is that, as discussed in Glaeser and Saks (2006), it is largely immune from the "usual problem that ... in corrupt places, the judicial system is itself corrupt and fewer people will be charged with corrupt practices." As they argue, "This problem is mitigated when focusing on Federal convictions, because the Federal judicial system is relatively isolated from local corruption and should treat people similarly across space (page 1054)." There is a further reason why, in our context, variation in local enforcement is unlikely to explain the results: the relevant enforcement bodies are different. The Department of Justice is solely responsible for federal prosecutions of local gov-

[9]See Glaeser and Saks (2006) for more discussion of this point, particularly pages 1054 and 1058.

[10]The Department of Justice's website (`http://www.justice.gov/criminal/pin/`) gives more detailed description of the data.

ernment officials, whereas in the majority of cases, the Securities and Exchange Commission investigates corporate misbehavior and securities fraud.

Returning once again to Table 2, inspection reveals a positive relation between corporate and political corruption ($\rho = 0.31$). To see this more clearly, Figure 2 plots time-series average rate of federal convictions of public officials for each city on the y-axis, and the time-series average of financial misconduct on the x-axis. To more formally characterize the relation between political and corporate corruption, we estimate the following logistic regression:

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

Here, $Pr(Fraud_{j,t}^{i,a})$, 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, leverage, and cash flows.

The results of this estimation are presented in Panel A of Table 9. 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 to an increase in the odds ratio of $e^{0.0395} - 1 \approx 4.03\%$, confirming the evidence illustrated in Figure 2 that cities ranking high in political corruption also rank high in corporate corruption.

To highlight the economic magnitude of the relation between political and corporate misconduct, the next four columns present the results when the political corruption variables enter 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 second column indicates a positive and significant effect for cities in the top quintile of political corruption, whereas the third suggests that cities ranking lowest in political corruption also have low rates of corporate corruption.

[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.

In column (4), the coefficient on $High\overline{PolCor}^a$ is 0.112 ($t = 1.77$), indicating that relative to the middle three quintiles, the odds ratio for firms headquartered in the most politically corrupt cities is elevated by $e^{0.112} - 1 \approx 11.8\%$. By contrast, the magnitude is over twice as large (in absolute value), but of the opposite sign for the least corrupt cities (t=−2.83). Taking the difference between these coefficients, the difference in the odds ratio is $e^{0.380} - 1 \approx 46.1\%$, when evaluated at the mean values for all other covariates in Equation (2). This translates to a percentage change in $Pr(Fraud^{i,a}_{j,t})$ 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 2 (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.

We conclude Panel A by allowing the relation between corporate and political fraud to differ among firms of different sizes. Our intuition is that if peer-to-peer interaction between politicians and corporate managers is the relevant mechanism driving the link between political and corporate fraud, the relation should strengthen with the potential for contact between these groups. Firm size strikes us as a reasonable proxy, given that large firms significantly impact local employment, have a disproportionate effect on local tax revenues, and, for these and other reasons, are almost certainly more likely to garner attention from local politicians. Provided that these considerations translate to more interactions, peer effects between companies and politicians should be stronger for larger companies.

Our analysis indicates that this is indeed the case. Within each year, we rank firms based on total assets, and place them into quintiles. $Large$ firms are those in the top 20%, $Small$ firms correspond to the bottom quintile, and $Medium$ firms to the middle three groups.[13] The fifth column shows the results when the $LargeFirm$ and $MediumFirm$ indicators are interacted with $High\overline{PolCor}^a$, with $SmallFirm$ being omitted to avoid the dummy trap. The

[12]The average value for $Fraud^{i,a}_{j,t}$ 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 0.11, implying a mean value of $Fraud^{i,a}_{j,t}$ is 1.31%, as $log(\frac{0.0131}{1-0.0131}) = -4.32$. The same calculation implies a log odds of -3.94 for the most corrupt decile, translating to a mean value of 1.90% for $Fraud^{i,a}_{j,t}$.

[13]The average $Large$ firm in our sample has US$ 3.3B in assets. The corresponding figures for $Medium$ and $Small$ firms are US$ 226M and US$ 12M, respectively.

coefficient on $High\overline{PolCor}^{a}{*}LargeFirm$ is positive and significant, indicating that compared to firms in the smallest quintile, fraud rates of the largest firms are more sensitive to prevailing political fraud rates. The point estimate on the interaction for firms in the middle group is also positive, though not statistically significant (t=1.61).

In Panel B of Table 9, we present the relation between political and corporate fraud from a slightly different perspective. Instead of using political fraud to predict firm-level occurrences of financial misconduct, we aggregate each city-year into a single observation, an exercise useful for two reasons. First, with a single observation for each city, any concerns about correlated residuals across firms are removed by construction. Second, the dependent variable can now be defined as a continuous rate of fraud defined at the city-year level, rather than as a discrete variable at the firm level. Consequently, we can estimate the model using OLS with city fixed effects (which the incidental parameters problem makes infeasible for logit models), providing a more robust account of cross-regional covariation in fraud rates.

Consistent with Panel A, Panel B indicates that cities ranking high in political fraud are associated with high rates of corporate fraud. In the first column, we show the contemporaneous relation, where a city's rate of corporate fraud in year t is regressed against the rate of convictions for political fraud that the same year. The point estimate indicates that in response to a standard deviation increase in the rate of political fraud (about three additional convictions per million inhabitants), corporate fraud rates increase by roughly sixteen basis points ($2.89 \times 0.055\% \approx .16\%$). Recalling that average rate of corporate fraud is a little over one percent, this represents a meaningful increase on a percentage basis.

Column two tests the same idea, but takes as the independent variable the rate of political corruption the previous year. Here, the goal is to explore whether the relation is truly contemporaneous, or whether a spike in political corruption foreshadows (or is foreshadowed by) a similar increase in financial misconduct by local executives. Indeed, the point estimate is about twenty percent larger in column 2, and when both contemporaneous and lagged covariates are included, only the lagged value retains its significance.

In the next three columns (4-6), we present the results of the same specifications shown in column 1-3, but add city fixed effects. This facilitates the interpretation of each coefficient as a (roughly) diff-in-diff, whereby fluctuations in local political corruption slightly lead similar increases and decreases in corporate misbehavior. The estimated magnitudes are nearly identical to the first three columns, and suggest that a standard deviation increase in a city's political corruption predicts an increase in corporate corruption in the neighborhood of twenty percent. Column seven adds lagged employment and population growth as controls, with almost no change in the results.

To summarize the results of this section: 1) there is a strong cross-sectional correlation, with cities ranking highest in political corruption also having higher than average rates of financial misconduct, 2) misconduct in the corporate and political spheres are strongly correlated over time within cities, 3) this time-series relation is concentrated among the largest companies. Whereas the first finding is consistent with all three elements of Manski's (1993) taxonomy – exogenous, contextual, and endogenous peer effects – the second result rules out relatively static exogenous attributes, and any time-varying contextual effect due to enforcement.

This leaves only non-enforcement environmental influences as alternatives to peer effects, but these are inconsistent with the third finding, as large firms should be the least sensitive to shifts in local demographics, wealth shocks, etc. On the other hand, it seems reasonable that CEOs of large companies would interact more frequently with local politicians, compared to CEOs of smaller firms. Accordingly, we argue that the analysis of corporate and political corruption provides strong evidence against other possibilities, and moreover, suggests that the transmission of ethical norms extends beyond the corporate sector.

6 Stock prices

We conclude with an examination of 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 violators themselves (i.e., firms targeted by the SEC), but instead, on whether or not there is a stock price reaction of its local neighbors. As fraud appears to have consequences beyond the immediate violators, the question is whether the stock market understands these effects, and incorporates them into securities prices. In particular, given that there appears to be a local component in the revelation of fraud, the market may assign a higher probability of fraud to firms located near a known violator.[14]

Table 10 presents the results. Panel A summarizes announcement returns for firms targeted by the SEC for financial misconduct. Confirming prior research including Karpoff, Koester, Lee, and Martin (2013), we find that the initial announcements 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 are not currently 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, when 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 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

[14]Here, we do not take a stand on a causal relation. That is, stock price reactions are consistent with both endogenous peer-to-peer effects, as well as contextual effects like enforcement driving local correlations in fraud revelation.

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.

7 Summary and Conclusion

The results in this paper can be summarized as follows:

1. Financial misconduct rates differ by up to a factor of three among large U.S. cities.

2. Cities experience "waves" of financial misconduct. Fluctuations in city misconduct rates are 50%-75% as important as industry fluctuations in explaining the time-series pattern of firm level financial misconduct.

3. Common enforcement and/or other environmental effects do not provide a complete explanation. Financial misconduct is initiated (not just exposed) simultaneously in an area. Moreover, a non-local instrument for misconduct in a city's core industry explains misconduct in other local industries.

4. At least part of these regional patterns is likely due to peer effects. Misbehavior of a firm's management is mainly influenced by local peers of similar size and/or age groups, and cities tend to have corresponding waves of political and corporate corruption.

The first pair of findings is novel, and regardless of their interpretation, should be of interest to researchers interested in the determinants of white collar crime. The specific interpretation – peer effects among local executives – as the most plausible mechanism is relevant for a number of additional reasons.

First, the offenders are, in a sense, unexpected. In contrast to the literature on urban crime, where the perpetrators are often youths or street criminals, the relevant parties here are highly educated, wealthy businesspeople – presumably an area's leaders rather than followers. Moreover, the costs of getting caught (e.g., career concerns) are clearly quite high for the executives, suggesting that cultural norms can have a powerful influence on behavior.

Second, just as street crime affects the vibrancy of an urban area – we certainly avoid shopping and socializing in areas that we believe are unsafe – the prevalence of white-collar crime can influence a city's business climate. Indeed, our preliminary evidence suggests that after controlling for the determinants of bankruptcy described in the existing literature, the incidence of financial misconduct in a city has a material effect on the probability of failure for its resident firms. We conjecture that struggling firms in cities with higher incidences of financial misconduct find it more difficult to raise capital, and may, as a result, fail to survive what could have been a minor liquidity problem if they were located in a city with more favorable social capital. This is an important issue that we will examine in future work.

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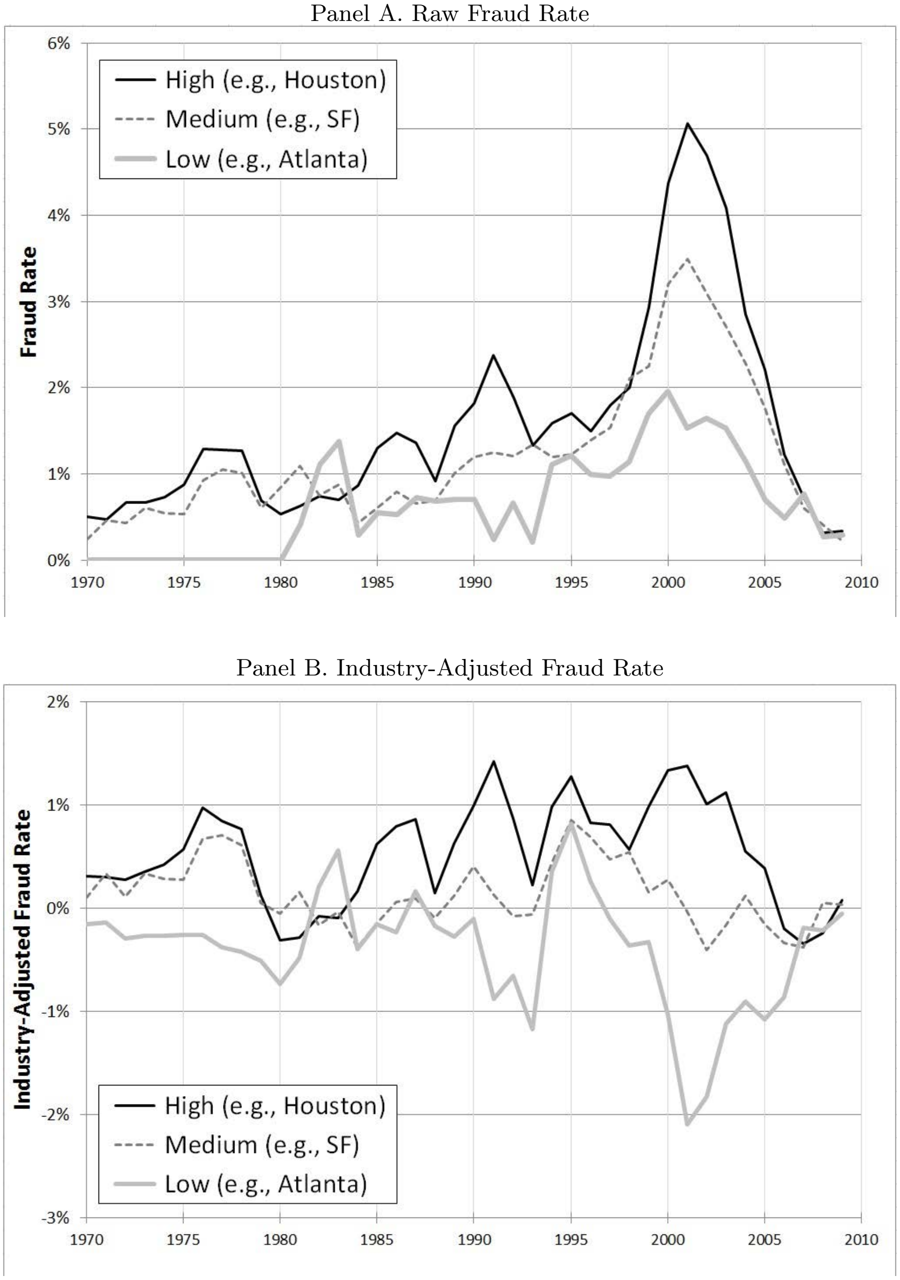
Figure 1: Time Series of Corporate Fraud Rate

This figure reports the time-series pattern of city-level corporate fraud rates for three different groups of cities sorted by their time-series average of fraud rate over the whole sample. The top and bottom quartiles are reported separately, and the middle two quartiles are combined. Panel A reports the raw fraud rates, while Panel B reports the industry-adjusted fraud rates, for which the average industry fraud rates (outside of our 20-city sample) are deducted from the raw fraud rates.

Panel A. Raw Fraud Rate



Panel B. Industry-Adjusted Fraud Rate

Figure 2: 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.

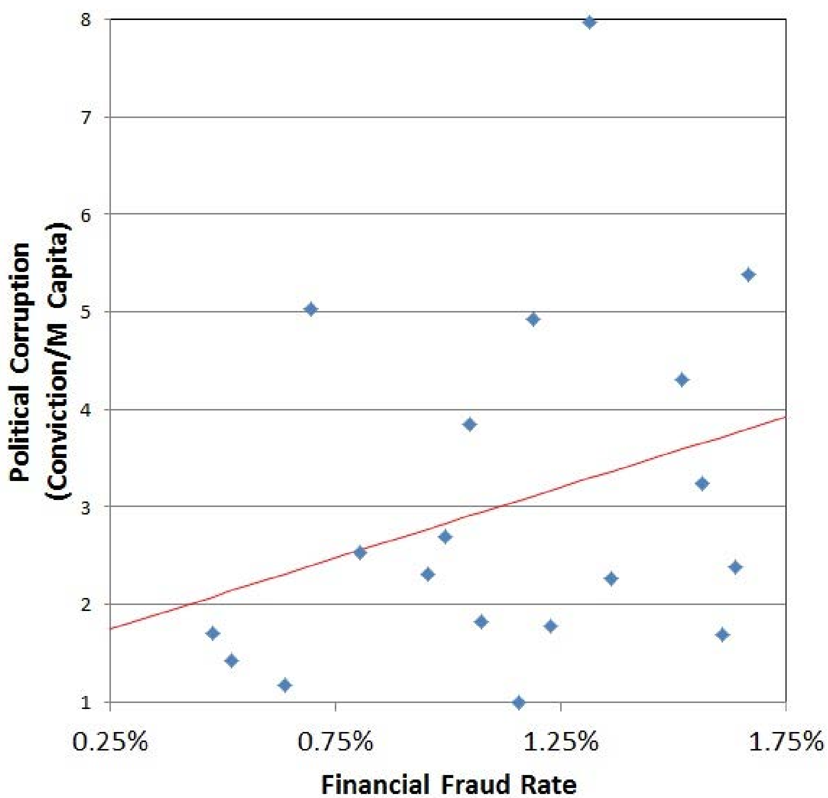


Table 1: Summary Statistics

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, $Fraud_{j,t}^{i,a}$, 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. At the city (Panel B) and industry (Panel C) levels, fraud and initial fraud 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, $PoliticalFraud_t^a$ 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 averages of cross-sectional summary statistics.

Variable	Mean	Std. Dev.	25^{th} Pctl.	Median	75^{th} Pctl.
Panel A: By Firm-Year					
$FraudInitiations_{j,t}^{i,a}$; Indicator Variable	0.0034				
$Fraud_{j,t}^{i,a}$; Indicator Variable	0.0146				
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
Panel B: By City-Year					
$FraudInitiations_t^a$	0.0030	0.0066	0.0000	0.0000	0.0040
$Fraud_t^a$	0.0112	0.0140	0.0000	0.0073	0.0172
$PoliticalFraud_t^a$	3.0317	2.8930	1.0012	2.1602	4.3345
Panel C: By Industry-Year					
$FraudInitiations_t^i$	0.0032	0.0058	0.0000	0.0000	0.0049
$Fraud_t^i$	0.0120	0.0133	0.0000	0.0101	0.0170

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. $PoliticalFraud_t^a$ 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.	*Political* $Fraud_t^a$
Indianapolis	28.03	0.48%	1.34%	0.00%	0.00%	1.70
Seattle	47.90	0.52%	1.12%	0.00%	0.00%	1.42
Minneapolis	123.05	0.64%	0.82%	0.00%	0.98%	1.18
Cleveland	76.65	0.69%	1.06%	0.00%	1.19%	5.03
Atlanta	98.08	0.80%	0.83%	0.00%	1.11%	2.53
Boston	219.20	0.96%	1.11%	0.00%	1.59%	2.31
Orlando	27.78	0.98%	2.12%	0.00%	0.00%	
Phoenix	46.25	0.99%	1.18%	0.00%	1.84%	2.70
Philadelphia	138.63	1.05%	0.96%	0.23%	1.61%	3.86
Detroit	68.90	1.07%	1.61%	0.00%	1.67%	1.83
San Francisco Bay	234.55	1.16%	1.25%	0.00%	1.46%	1.00
Chicago	180.10	1.19%	1.06%	0.00%	1.97%	4.92
Denver	96.40	1.23%	1.40%	0.00%	2.23%	1.78
Washington, DC	133.18	1.31%	1.22%	0.00%	1.91%	7.97
Los Angeles	270.88	1.36%	0.73%	0.85%	1.91%	2.27
New York	599.13	1.52%	0.99%	0.75%	1.94%	4.30
Houston	136.83	1.56%	2.05%	0.00%	1.75%	3.24
Dallas	154.73	1.61%	1.87%	0.00%	2.15%	1.69
St. Louis	45.45	1.64%	1.95%	0.00%	3.06%	2.39
Miami	105.45	1.66%	1.44%	0.00%	2.69%	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 $Fraud_{j,t}^{i,a}$, 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 $Fraud_{p,t}^{-i,a}$, $Fraud_{p,t}^{i,-a}$, and $Fraud_{p,-j,t}^{i,a}$. 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, and other firms operating in the same industry and located in the same area, respectively. The set of control variables also include the market fraud rate excluding firms in the same area and/or industry (in all models) and $\overline{Fraud}_{p}^{-i,a}$, the time-series average of $Fraud_{p,t}^{-i,a}$ (in the last two models). 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)	(6)
Peer Fraud Variables:	Raw Fraud Rate					High Fraud Indicator
Dependent Variable:	Fraud	Fraud	Fraud	Fraud	Fraud	Fraud
$Fraud_{p,t}^{-i,a}$	8.1118***			7.9641***	9.3299***	0.3708***
	(4.79)			(4.69)	(4.76)	(4.38)
$Fraud_{p,t}^{i,-a}$		13.1937***		12.6083***	12.1558***	0.3702***
		(4.29)		(4.18)	(4.06)	(5.39)
$Fraud_{p,-j,t}^{i,a}$			2.1368***	1.7517***	1.7069***	0.3856***
			(3.84)	(3.02)	(2.94)	(4.37)
$\overline{Fraud}_{p}^{-i,a}$					-11.2545	-14.2637**
					(-1.42)	(-2.17)
Lagged stock return	0.0686***	0.0653***	0.0669***	0.0651***	0.0655***	0.0655***
	(3.97)	(3.90)	(3.80)	(3.95)	(4.00)	(4.01)
Lagged asset	0.2588***	0.2608***	0.2600***	0.2607***	0.2609***	0.2748***
	(12.68)	(12.82)	(12.74)	(12.83)	(12.85)	(13.96)
Lagged leverage	-0.1417	-0.1458	-0.1454	-0.1515	-0.1470	-0.0875
	(-1.32)	(-1.45)	(-1.38)	(-1.50)	(-1.46)	(-0.85)
Lagged Q	0.2239***	0.2059***	0.2194***	0.2086***	0.2059***	0.1987***
	(7.40)	(8.06)	(7.52)	(8.15)	(8.14)	(7.63)
Lagged cash flow	0.2257	0.2119	0.2185	0.2077	0.2059	0.1054
	(0.83)	(0.84)	(0.82)	(0.82)	(0.81)	(0.42)
Constant	-6.6508***	-6.6568***	-6.6439***	-6.6717***	-6.5317***	-6.6284***
	(-44.37)	(-47.34)	(-45.22)	(-47.34)	(-38.17)	(-39.64)
Market Fraud Rates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	90,208	90,208	90,208	90,208	90,208	90,208
Pseudo R^2	0.0551	0.0566	0.0549	0.0580	0.0582	0.0574

Table 4: Logistic Regressions of Financial Misconduct (Continued)

	Panel B: Fraud Initiations					
	(1)	(2)	(3)	(4)	(5)	(6)
Peer Fraud Variables:	Raw FraudInit Rate					High FraudInit Indicator
Dependent Variable:	Fraud Init	Fraud Init	Fraud Init	Fraud Init	Fraud Init	Fraud Init
$FraudInit_{p,t}^{-i,a}$	23.5383***			22.6751***	23.5471***	0.5350***
	(3.75)			(3.66)	(3.74)	(4.14)
$FraudInit_{p,t}^{i,-a}$		17.8104		15.6833	15.3639	0.1772
		(1.42)		(1.23)	(1.19)	(0.92)
$FraudInit_{p,-j,t}^{i,a}$			3.6153*	3.2525	3.2030	0.1497
			(1.82)	(1.45)	(1.42)	(0.81)
$\overline{FraudInit}_{p}^{-i,a}$					-55.1867	-85.4018
					(-0.66)	(-0.98)
Lagged stock return	0.0786***	0.0752***	0.0762***	0.0770***	0.0769***	0.0894***
	(3.42)	(3.00)	(3.12)	(3.25)	(3.24)	(4.84)
Lagged asset	0.1843***	0.1850***	0.1851***	0.1839***	0.1845***	0.2074***
	(4.79)	(4.90)	(4.85)	(4.87)	(4.90)	(5.48)
Lagged leverage	-0.5358**	-0.5263**	-0.5282**	-0.5385**	-0.5368**	-0.4355*
	(-2.29)	(-2.26)	(-2.26)	(-2.30)	(-2.30)	(-1.84)
Lagged Q	0.1975***	0.1919***	0.1964***	0.1921***	0.1911***	0.2023***
	(3.79)	(3.74)	(3.77)	(3.74)	(3.71)	(3.86)
Lagged cash flow	0.1640	0.1482	0.1469	0.1477	0.1409	0.0562
	(0.36)	(0.34)	(0.33)	(0.33)	(0.32)	(0.13)
Constant	-7.4918***	-7.4817***	-7.4766***	-7.4927***	-7.3242***	-7.4958***
	(-26.09)	(-26.73)	(-26.16)	(-26.83)	(-17.77)	(-16.81)
Market Fraud Rates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	90,208	90,208	90,208	90,208	90,208	90,208
Pseudo R^2	0.0392	0.0379	0.0381	0.0407	0.0409	0.0362

Table 5: Controlling for Environmental Variables

This table contains parameter estimates from panel logit regression predicting our fraud measure. The dependent variable in all regressions in Panel A is $Fraud_{j,t}^{i,a}$, 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 $Fraud_{p,t}^{-i,a}$, $Fraud_{p,t}^{i,-a}$, and $Fraud_{p,-j,t}^{i,a}$. 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, and other firms operating in the same industry and located in the same area, respectively. The set of control variables also includes $\overline{Fraud}_{p}^{-i,a}$, the time-series average of $Fraud_{p,t}^{-i,a}$, and the market fraud rate excluding firms in the same area and/or industry. In columns (2)-(5), we add lagged, contemporaneous, and lead variables reflecting city-level economic conditions: population, employment, and wage growth rates. Columns (6) and (7) replicate column (5) for firms above and below the annual median asset size, respectively. 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.

Table 5: Controlling for Environmental Variables (Continued)

Panel A: Fraud							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Subsample:	All Firms	All Firms	All Firms	All Firms	All Firms	Large Firms	Small Firms
Dependent Variable:	Fraud	Fraud	Fraud	Fraud	Fraud	Fraud	Fraud
$Fraud_{p,t}^{-i,a}$	9.3299***	8.6371***	9.3692***	9.3829***	7.8859***	8.3418***	6.1108
	(4.76)	(4.51)	(4.82)	(4.85)	(3.98)	(3.49)	(1.50)
$Fraud_{p,t}^{i,-a}$	12.1558***	11.7638***	11.9379***	11.9205***	11.4825***	10.1285***	14.4278***
	(4.06)	(4.00)	(4.13)	(4.17)	(4.10)	(3.04)	(3.09)
$Fraud_{p,-j,t}^{i,a}$	1.7069***	1.6120***	1.7167***	1.7193***	1.5759***	2.5013***	-3.3145
	(2.94)	(2.79)	(3.01)	(2.97)	(2.68)	(4.34)	(-1.40)
$\overline{Fraud}_{p}^{-i,a}$	-11.2545	-12.5274	-11.7366	-11.4604	-12.0758	-10.8044	-14.2618
	(-1.42)	(-1.55)	(-1.49)	(-1.49)	(-1.54)	(-1.07)	(-0.99)
Lagged stock return	0.0655***	0.0617***	0.0650***	0.0660***	0.0666***	0.0069	0.0805***
	(4.00)	(3.62)	(4.02)	(4.18)	(4.33)	(0.15)	(4.63)
Lagged asset	0.2609***	0.2674***	0.2659***	0.2627***	0.2641***	0.3492***	0.1378***
	(12.85)	(13.37)	(13.22)	(12.89)	(13.11)	(11.97)	(3.09)
Lagged leverage	-0.1470	-0.1715*	-0.1350	-0.1297	-0.1263	-0.0538	-0.5723***
	(-1.46)	(-1.70)	(-1.38)	(-1.34)	(-1.31)	(-0.44)	(-3.05)
Lagged Q	0.2059***	0.2073***	0.2141***	0.2074***	0.2042***	0.2795***	0.1254***
	(8.14)	(8.23)	(8.40)	(8.24)	(8.07)	(7.33)	(3.46)
Lagged cash flow	0.2059	0.1430	0.1746	0.1970	0.2130	-0.4592	0.5580
	(0.81)	(0.56)	(0.69)	(0.78)	(0.85)	(-1.47)	(1.63)
$Pop.Growth_{t-2}^{a}$		8.7657			6.7466	3.9875	14.0916
		(1.00)			(0.77)	(0.36)	(1.02)
$Pop.Growth_{t-1}^{a}$		5.5573			6.8935	4.3668	11.9982
		(0.42)			(0.52)	(0.27)	(0.58)
$Pop.Growth_{t}^{a}$		-1.7846			7.5661	2.7428	13.8319
		(-0.15)			(0.59)	(0.16)	(0.66)
$Pop.Growth_{t+1}^{a}$		-1.2623			3.8763	-5.6224	19.5596
		(-0.12)			(0.31)	(-0.32)	(0.96)
$Pop.Growth_{t+2}^{a}$		-3.8172			-10.6556	-7.5782	-16.2803
		(-0.52)			(-1.21)	(-0.62)	(-1.06)
$Emp.Growth_{t-2}^{a}$			1.2719		-2.5827	0.0234	-7.4236**
			(0.68)		(-1.14)	(0.01)	(-2.28)
$Emp.Growth_{t-1}^{a}$			0.1128		-0.9006	0.0905	-2.1693
			(0.05)		(-0.32)	(0.02)	(-0.50)
$Emp.Growth_{t}^{a}$			-4.4667*		-5.6068*	-2.3816	-9.6500**
			(-1.86)		(-1.95)	(-0.64)	(-2.21)
$Emp.Growth_{t+1}^{a}$			2.6391		1.2464	2.5780	-1.8399
			(1.02)		(0.45)	(0.76)	(-0.45)
$Emp.Growth_{t+2}^{a}$			1.7638		0.4189	1.5470	-1.3512
			(0.90)		(0.18)	(0.56)	(-0.36)
$WageGrowth_{t-2}^{a}$				0.0657	0.8793	0.2833	1.9628
				(0.04)	(0.55)	(0.15)	(0.76)
$WageGrowth_{t-1}^{a}$				-0.8811	0.0142	-0.8250	0.8853
				(-0.56)	(0.01)	(-0.40)	(0.31)
$WageGrowth_{t}^{a}$				0.0229	1.2528	0.1835	4.6998
				(0.02)	(0.73)	(0.09)	(1.56)
$WageGrowth_{t+1}^{a}$				2.9141*	2.6404	1.3645	5.0246*
				(1.96)	(1.61)	(0.59)	(1.90)
$WageGrowth_{t+2}^{a}$				0.3703	0.8507	1.2277	1.2209
				(0.26)	(0.55)	(0.56)	(0.48)
Constant	-6.5317***	-6.5456***	-6.5221***	-6.5332***	-6.5521***	-7.3573***	-5.5881***
	(-38.17)	(-39.81)	(-39.28)	(-38.77)	(-39.67)	(-29.92)	(-23.76)
Market Fraud Rates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	90,208	86,962	86,962	86,962	86,962	44,876	42,068
Pseudo R^2	0.0582	0.0572	0.0571	0.0570	0.0585	0.0715	0.0304

Table 5: Controlling for Environmental Variables (Continued)

Panel B: Fraud Initiations							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Subsample:	All Firms	All Firms	All Firms	All Firms	All Firms	Large Firms	Small Firms
Dependent Variable:	FraudInit	FraudInit	FraudInit	FraudInit	FraudInit	FraudInit	FraudInit
$FraudInit_{p,t}^{-i,a}$	23.5471***	22.4102***	23.2866***	24.0756***	22.0948***	23.0499***	17.9364
	(3.74)	(3.49)	(3.72)	(3.71)	(3.31)	(2.97)	(1.22)
$FraudInit_{p,t}^{i,-a}$	15.3639	14.4595	15.8037	15.1815	14.7487	17.9005	-5.6631
	(1.19)	(1.12)	(1.22)	(1.19)	(1.12)	(1.37)	(-0.24)
$FraudInit_{p,-j,t}^{i,a}$	3.2030	3.0565	3.2096	3.2288	3.0175	3.4462	-3.9116
	(1.42)	(1.36)	(1.52)	(1.46)	(1.37)	(1.48)	(-0.51)
$\overline{FraudInit}_{p}^{-i,a}$	-55.1867	-82.2545	-55.9700	-53.9174	-92.8906	-83.0663	-104.6879
	(-0.66)	(-0.99)	(-0.68)	(-0.65)	(-1.09)	(-0.97)	(-0.64)
Lagged stock return	0.0769***	0.0736***	0.0813***	0.0833***	0.0824***	0.1076**	0.0738***
	(3.24)	(3.20)	(3.61)	(3.73)	(3.81)	(2.11)	(3.48)
Lagged asset	0.1845***	0.1875***	0.1873***	0.1817***	0.1830***	0.2784***	-0.0402
	(4.90)	(5.00)	(4.92)	(4.78)	(4.85)	(4.46)	(-0.44)
Lagged leverage	-0.5368**	-0.5571**	-0.5591**	-0.5225**	-0.5530**	-0.3397	-1.1555***
	(-2.30)	(-2.36)	(-2.35)	(-2.18)	(-2.30)	(-1.04)	(-2.82)
Lagged Q	0.1911***	0.1901***	0.1987***	0.1882***	0.1826***	0.2153***	0.1178*
	(3.71)	(3.65)	(3.75)	(3.51)	(3.35)	(2.81)	(1.72)
Lagged cash flow	0.1409	0.0718	0.0979	0.1542	0.1665	0.2119	0.2608
	(0.32)	(0.16)	(0.22)	(0.35)	(0.38)	(0.28)	(0.46)
$Pop.Growth_{t-2}^{a}$		-6.6124			-11.6767	-15.1224	-5.2660
		(-0.41)			(-0.67)	(-0.66)	(-0.20)
$Pop.Growth_{t-1}^{a}$		13.2661			11.6494	-2.3295	25.2257
		(0.60)			(0.45)	(-0.07)	(0.61)
$Pop.Growth_{t}^{a}$		16.9399			24.5933	29.6983	20.1217
		(0.79)			(1.08)	(1.01)	(0.57)
$Pop.Growth_{t+1}^{a}$		-0.0467			13.0450	15.8145	10.0751
		(-0.00)			(0.57)	(0.50)	(0.31)
$Pop.Growth_{t+2}^{a}$		-12.1127			-15.5342	-13.1131	-20.1500
		(-0.92)			(-0.94)	(-0.55)	(-0.82)
$Emp.Growth_{t-2}^{a}$			3.0294		-1.8653	0.9685	-5.7571
			(0.76)		(-0.37)	(0.14)	(-0.88)
$Emp.Growth_{t-1}^{a}$			4.0028		0.3095	0.7884	1.0889
			(0.80)		(0.05)	(0.10)	(0.13)
$Emp.Growth_{t}^{a}$			-7.3842		-8.7593	-8.3155	-9.4742
			(-1.61)		(-1.47)	(-1.00)	(-1.28)
$Emp.Growth_{t+1}^{a}$			-0.6661		-3.1593	-4.0234	-3.6961
			(-0.16)		(-0.66)	(-0.61)	(-0.54)
$Emp.Growth_{t+2}^{a}$			4.4818		2.0416	0.7733	3.5596
			(1.20)		(0.46)	(0.14)	(0.58)
$WageGrowth_{t-2}^{a}$				2.8022	3.4528	7.3323	-1.5611
				(0.89)	(1.03)	(1.59)	(-0.27)
$WageGrowth_{t-1}^{a}$				-1.7508	-1.4814	-2.4471	0.9820
				(-0.61)	(-0.47)	(-0.56)	(0.21)
$WageGrowth_{t}^{a}$				-0.4838	2.1233	3.3683	1.6789
				(-0.18)	(0.64)	(0.85)	(0.28)
$WageGrowth_{t+1}^{a}$				4.0720*	4.4020*	6.2977**	2.1046
				(1.78)	(1.67)	(2.15)	(0.45)
$WageGrowth_{t+2}^{a}$				1.1097	1.3835	2.3158	1.2159
				(0.51)	(0.55)	(0.65)	(0.27)
Constant	-7.3242***	-7.3133***	-7.3835***	-7.3447***	-7.3385***	-8.2919***	-6.0507***
	(-17.77)	(-17.58)	(-17.19)	(-16.88)	(-16.66)	(-14.24)	(-9.36)
Market Fraud Rates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	90,208	86,962	86,962	86,962	86,962	44,876	42,068
Pseudo R^2	0.0409	0.0410	0.0411	0.0406	0.0438	0.0628	0.0269

Table 6: The Revelation of Corporate Misconduct

This table contains parameter estimates from panel logit regression predicting the revelation of fraud. The dependent variable in the first column is $FraudExposed_{j,t}^{i,a}$, is a dummy variable denoting the exposure of financial misconduct by firm j, operating in industry i, in area a, during year t. This includes financial misconduct initiated in any year up to year t. The dependent variable in the second and third columns are $FraudExposed_{j,t}^{i,a}|FraudInit_{j,t-1}^{i,a}$ and $FraudExposed_{j,t}^{i,a}|FraudInit_{j,t-2}^{i,a}$, respectively. They are dummy variables denoting the exposure during year t of financial misconduct by firm j but only if the misconduct is initiated in year $t-1$ or $t-2$, respectively. The main dependent variables of interest are $FraudExposed_{p,t}^{-i,a}$, $FraudExposed_{p,t}^{i,-a}$, and $FraudExposed_{p,-j,t}^{i,a}$. They are the fraud exposure 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, and other firms operating in the same industry and located in the same area, respectively. The first variable is broken down into $FraudExposed_{p,t}^{-i,a}|FraudInit_{p,t-1}^{-i,a}$ and $FraudExposed_{p,t}^{-i,a}|FraudInit_{p,t-2}^{-i,a}$ in models (2) and (3) to capture the exposure rates of misconduct initiated in years $t-1$ and $t-2$, respectively. The set of control variables also include the market fraud exposure rate excluding firms in the same area and/or industry. 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)
Dependent Variable:	$FraudExposed_{j,t}^{i,a}$	$FraudExposed_{j,t}^{i,a}$ $\|FraudInit_{j,t-1}^{i,a}$	$FraudExposed_{j,t}^{i,a}$ $\|FraudInit_{j,t-2}^{i,a}$
$FraudExposed_{p,t}^{-i,a}$	21.7316***		
	(2.61)		
$FraudExposed_{p,t}^{-i,a}\|FraudInit_{p,t-1}^{-i,a}$		38.2183	37.8208
		(1.57)	(1.53)
$FraudExposed_{p,t}^{-i,a}\|FraudInit_{p,t-2}^{-i,a}$		6.5062	68.1700***
		(0.18)	(6.69)
$FraudExposed_{p,t}^{i,-a}$	27.5809***	33.4214***	26.1272***
	(4.99)	(7.52)	(4.76)
$FraudExposed_{p,-j,t}^{i,a}$	1.0992	1.6409	-1.3370
	(0.81)	(0.63)	(-0.31)
Lagged stock return	0.0432	-0.0222	0.0708
	(0.91)	(-0.30)	(1.18)
Lagged asset	0.2268***	0.1742***	0.0482
	(6.54)	(3.27)	(0.76)
Lagged leverage	1.0539***	1.2494***	1.8238***
	(5.18)	(2.91)	(3.62)
Lagged Q	0.2918***	0.4970***	0.2472***
	(5.58)	(6.00)	(2.99)
Lagged cash flow	-0.7573*	-1.1257	0.7109
	(-1.85)	(-1.46)	(0.70)
Constant	-8.2731***	-9.6090***	-8.9433***
	(-35.30)	(-22.67)	(-20.97)
Market Fraud Exposure Rates	Yes	Yes	Yes
Observations	90,208	90,208	90,208
Pseudo R^2	0.0585	0.0553	0.0459

Table 7: 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:	1^{st} stage	2^{nd} stage	1^{st} stage	2^{nd} stage
Dependent Variable:	$Fraud^{Dom,a}_{p,t}$	$Fraud^{i,a}_{j,t}$	$Fraud^{Dom,a}_{p,t}$	$Fraud^{i,a}_{j,t}$
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 8: Peer Effects of Similar Local Firms and/or Managers

This table contains parameter estimates from panel logit regression predicting our fraud measure. The dependent variable in all regressions is $Fraud_{j,t}^{i,a}$, 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 in Panel A are the fraud rates of subsample of firms located in the same area but operating in a different industry: $Fraud_{large,t}^{-i,a}$, $Fraud_{small,t}^{-i,a}$, $Fraud_{same\ size,t}^{-i,a}$, and $Fraud_{diff.\ size,t}^{-i,a}$. These are local large firms (above the annual median asset size), local small firms (below the annual median asset size), local firms in the same size group as firm j, and local firms in the opposite size group, respectively. The set of control variables also includes fraud rates of firms operating in the same industry but located in a different area, other firms operating in the same industry and located in the same area, and the market fraud rate excluding firms in the same area and/or industry. The first two columns are restricted to large firms and small firms as defined above, respectively. The last two columns include all firms, with the column (4) also includes lagged/contemporaneous/lead city-level growth rates. In Panel B, the main dependent variables of interest are the fraud rates of subsample of firms located in the same area but operating in a different industry: $Fraud_{young\ CEO,t}^{-i,a}$, $Fraud_{old\ CEO,t}^{-i,a}$, $Fraud_{same\ age,t}^{-i,a}$, and $Fraud_{diff.\ age,t}^{-i,a}$. These are local firms with CEO younger than 55 years old, with CEO older than 55, with CEO in the same age group as firm j's CEO, and whose CEO is in the opposite age group, respectively. The sample in Panel B is restricted to firms in Execucomp. 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.

Table 8: Peer Effects of Similar Local Firms and/or Managers (Continued)

Panel A: Size-Matching				
	(1)	(2)	(3)	(4)
Subsample:	Large Firms	Small Firms	All Firms	All Firms
Dependent Variable:	Fraud	Fraud	Fraud	Fraud
$Fraud^{-i,a}_{large,t}$	3.9326***	0.1819		
	(3.65)	(0.06)		
$Fraud^{-i,a}_{small,t}$	0.6743	13.2194***		
	(0.30)	(6.40)		
$Fraud^{-i,a}_{same\ size,t}$			7.1821***	6.6658***
			(7.07)	(5.49)
$Fraud^{-i,a}_{diff.\ size,t}$			-2.1865	-3.5713**
			(-1.40)	(-2.09)
$Fraud^{i,-a}_{p,t}$	30.6662***	5.8859	21.3121***	17.5833***
	(5.59)	(0.81)	(4.68)	(4.15)
$Fraud^{i,a}_{p,-j,t}$	10.7599***	20.3592***	13.2251***	12.5792***
	(3.16)	(4.01)	(4.28)	(5.65)
Lagged stock return	0.0221	0.0788***	0.0677***	0.0691***
	(0.51)	(4.29)	(4.10)	(3.33)
Lagged asset	0.2815***	0.0484	0.2328***	0.2339***
	(9.78)	(1.05)	(11.76)	(13.92)
Lagged leverage	-0.0766	-0.5016***	-0.1458	-0.1225
	(-0.64)	(-2.69)	(-1.43)	(-1.19)
Lagged Q	0.2524***	0.1646***	0.2119***	0.2078***
	(6.73)	(4.57)	(8.24)	(8.59)
Lagged cash flow	-0.3190	0.5804*	0.1610	0.1798
	(-1.00)	(1.65)	(0.65)	(0.82)
Constant	-6.9425***	-5.6268***	-6.4939***	-6.5057***
	(-33.09)	(-32.75)	(-47.91)	(-51.83)
Market Fraud Rates	Yes	Yes	Yes	Yes
City-level Growth Rates	No	No	No	Yes
Observations	46,597	43,611	90,208	86,962
Pseudo R^2	0.0640	0.0268	0.0601	0.0607

Table 8: Peer Effects of Similar Local Firms and/or Managers (Continued)

Panel B: CEO Age-Matching				
	(1)	(2)	(3)	(4)
Subsample:	Young CEO	Old CEO	All Firms	All Firms
Dependent Variable:	Fraud	Fraud	Fraud	Fraud
$Fraud^{-i,a}_{young\ CEO,t}$	2.5486**	1.6900		
	(2.20)	(1.21)		
$Fraud^{-i,a}_{old\ CEO,t}$	1.4176	1.2996		
	(1.12)	(0.87)		
$Fraud^{-i,a}_{same\ age,t}$			2.1107**	1.9145*
			(2.22)	(1.92)
$Fraud^{-i,a}_{diff.\ age,t}$			1.3469	1.2474
			(1.30)	(1.18)
$Fraud^{i,-a}_{p,t}$	29.3283***	34.1916***	32.1589***	28.2395***
	(3.66)	(4.15)	(4.81)	(3.27)
$Fraud^{i,a}_{p,-j,t}$	17.5348***	11.4095**	13.5796***	13.2689***
	(3.62)	(2.31)	(3.11)	(3.15)
Lagged stock return	0.0634	0.1415	0.0910	0.0570
	(1.07)	(1.27)	(1.43)	(0.80)
Lagged asset	0.3005***	0.3771***	0.3311***	0.3358***
	(3.81)	(5.48)	(6.60)	(6.64)
Lagged leverage	0.3148	0.0118	0.1672	0.2136
	(1.10)	(0.04)	(0.83)	(1.02)
Lagged Q	0.0979*	0.2088***	0.1484***	0.1380***
	(1.76)	(3.40)	(3.42)	(3.11)
Lagged cash flow	-0.0234	-2.3608***	-1.0437**	-1.0871**
	(-0.04)	(-3.83)	(-2.30)	(-2.28)
Constant	-7.0389***	-7.6079***	-7.2684***	-7.4207***
	(-10.67)	(-13.82)	(-16.42)	(-16.27)
Market Fraud Rates	Yes	Yes	Yes	Yes
City-level Growth Rates	No	No	No	Yes
Observations	6,138	7,428	13,566	12,550
Pseudo R^2	0.0653	0.0626	0.0616	0.0613

Table 9: 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)-(5), we use $High\ \overline{PolCor}^a$ and $Low\ \overline{PolCor}^a$, indicator variables for cities in the top and bottom quintiles of $\overline{PolCor}^a$, respectively. In Model (5) we use $LargeFirm$ and $MediumFirm$, indicator variables for firms in the top quintile and middle three quintiles of total asset, respectively. 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 control variables in Model (7) include lagged employment and population growth rates in the city. 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)	(5)
Dependent Variable:	Fraud	Fraud	Fraud	Fraud	Fraud
$\overline{PolCor}^a$	0.0395**				
	(2.55)				
$High\ \overline{PolCor}^a$		0.1768***		0.1119*	-0.1718
		(2.98)		(1.77)	(-1.08)
$Low\ \overline{PolCor}^a$			-0.3201***	-0.2670***	-0.2693***
			(-3.61)	(-2.83)	(-2.85)
$High\ \overline{PolCor}^a * LargeFirm$					0.3785**
					(2.01)
$High\ \overline{PolCor}^a * MediumFirm$					0.2598
					(1.61)
Lagged stock return	0.0853***	0.0860***	0.0843***	0.0851***	0.2830***
	(4.97)	(5.02)	(4.80)	(4.84)	(11.84)
Lagged asset	0.2984***	0.2980***	0.3014***	0.3003***	0.0842***
	(14.25)	(14.27)	(14.40)	(14.33)	(4.78)
Lagged leverage	-0.1135	-0.1148	-0.1248	-0.1258	-0.1249
	(-0.97)	(-0.98)	(-1.07)	(-1.07)	(-1.07)
Lagged Q	0.2658***	0.2645***	0.2758***	0.2758***	0.2788***
	(8.70)	(8.63)	(9.13)	(9.12)	(9.20)
Lagged cash flow	-0.2192	-0.2174	-0.2522	-0.2422	-0.2719
	(-0.81)	(-0.80)	(-0.93)	(-0.89)	(-1.02)
Constant	-6.3337***	-6.2707***	-6.1841***	-6.2314***	-6.1326***
	(-45.41)	(-46.75)	(-45.10)	(-45.15)	(-41.66)
Observations	90,274	90,274	90,274	90,274	90,274
Pseudo R^2	0.0371	0.0374	0.0378	0.0381	0.0384

Table 9: The Relation Between Political and Corporate Corruption (Continued)

Panel B: City-Level Analysis							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	$Fraud_t^a$	$Fraud_t^a$	$Fraud_t^a$	$Fraud_t^a$	$Fraud_t^a$	$Fraud_t^a$	$Fraud_t^a$
$PolCor_t^a$	0.0547***		0.0151	0.0425*		0.0160	0.0186
	(2.80)		(0.67)	(1.74)		(0.68)	(0.84)
$PolCor_{t-1}^a$		0.0713***	0.0615**		0.0674**	0.0605**	0.0610**
		(3.45)	(2.39)		(2.55)	(2.22)	(2.29)
$EmpGrowth_{t-1}^a$							-10.1760*
							(-1.81)
$PopGrowth_{t-1}^a$							20.2409
							(1.36)
City Fixed Effects				Yes	Yes	Yes	Yes
Observations	613	613	613	613	613	613	613
R^2	0.013	0.022	0.023	0.090	0.097	0.098	0.119

Table 10: 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)