The Geography of Financial Misconduct*

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Abstract

Financial misconduct (FM) varies across cities, and over time. City-level FM is highly correlated with political corruption (enforced non-locally), and with ethically questionable (but not illegal) behavior by local physicians, both inconsistent with regional variation in enforcement. Further, among firms for whom detection is likely very similar (ex-clients of Arthur Andersen forced to switch auditors), a similar ranking of FM rates across cities emerges. FM is uncorrelated with economic and demographic variation, but is consistent with social norms transmitted by peer effects. In particular, FM rates in a city's "dominant" industry predict FM among local firms outside the dominant sector.

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

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Abstract – Financial misconduct (FM) varies across cities, and over time. City-level FM is highly correlated with political corruption (enforced non-locally), and with ethically questionable (but not illegal) behavior by local physicians, both inconsistent with regional variation in enforcement. Further, among firms for whom detection is likely very similar (ex-clients of Arthur Andersen forced to switch auditors), a similar ranking of FM rates across cities emerges. FM is uncorrelated with economic and demographic variation, but is consistent with social norms transmitted by peer effects. In particular, FM rates in a city's "dominant" industry predict FM among local firms outside the dominant sector.

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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] Their conclusion is that *social factors* play an important role in a person's decision to break rules, or to respect them.

In this paper, we investigate the geographical patterns and causes of white collar crime. Specifically, we characterize regional patterns in financial misconduct (FM) among the twenty largest U.S. cities, using the hand-collected sample compiled by Karpoff, Koester, Lee, and Martin (2013), hereafter KKLM. As described in Section 2, examples of corporate misbehavior in this data include misrepresenting a firm's earnings, failing to disclose relevant news, or trading on proprietary information.

We begin in Section 3 by documenting large geographic effects, both in the cross-section and time series. Cross-sectionally, average FM rates differ considerably between cities. For example, FM is observed about once in every 190 firm-years in Indianapolis, Seattle, and Minneapolis; but in Dallas (1:62), St. Louis (1:61), and Miami (1:60) the rate of detection is more than three times higher. Very little of these differences reflect industry clustering (Figure 1). There are also important fluctuations within cities over time: when predicting the likelihood of observing FM for a given firm-year, variation in the FM rates of local, non-industry peers matters almost as much as industry-level ebbs and flows. Thus, insofar as the objective is to predict detected FM events, the relevance of area factors – whatever their origin – rivals that of industry factors.

When interpreting these patterns, it is important to acknowledge the presence of two distinct econometric problems. The first, which we term the *measurement problem*, arises because our

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

data record only detected FM, rather than committed FM. As a result, what we observe may be, at least in part, due to differences in regional enforcement and/or other attributes influencing the detection of FM. The second challenge is the so-called *reflection problem*,[2] first identified by Manski (1993), and applies in any setting "when a researcher, observing the distribution of behaviour in a population, tries to infer whether the average behaviour in some group influences the behaviour of the individuals that comprise the group (p. 532)." In our context, the concern is that there may be other determinants of FM, like economic activity, which can vary independently of social factors.

We start with an analysis of the detection problem in Section 4. In our first test, we follow the methodology developed by Dyck, Morse, and Zingales (2014), which examines the demise of auditor Arthur Andersen (AA). Following the Enron scandal, AA surrendered its license to practice accounting, forcing its several thousand clients to find new auditors. DMZ argues that because new auditors would have little incentive to overlook frauds occurring under AA's watch, this setting provides a strong, upward shift in the probability that existing FM is detected. To the extent that detection probabilities among this sample of switchers is similar (and likely very high), this experiment provides a way to rank cities by FM incidence, largely purged of local detection effects. As it turns out, this 'clean' ranking is similar to our benchmark ranking ($\rho \approx 0.6$), suggesting that most of the variation across cities reflects actual differences in behavior.

Our second test examines the timing associated with each FM event. There are two relevant dates: when misconduct is *initiated*, and when it is subsequently *detected*. Generally, we expect city-specific detection efforts to generate temporal clustering in detection dates, e.g., higher scrutiny in Atlanta and Philadelphia during 1998 resulting in higher FM detection rates in those cities. Further, if the time between initiation and detection were fixed, say two years, we would expect similar clustering in the dates of initiation (1996 in the example). But, because there is considerable dispersion in the time between initiation and detection – ranging from a few months to over a decade – it is possible to determine the extent to which clustered detection is causing the appearance of clustered initiation.

We do this by looking across cities at FM events that are detected in *specific* years, and seeing

[2]Manski uses the analogy of a person examining his or her own reflection in a mirror. Without an understanding of optics, it would be impossible to tell whether a person's image caused the persons' movements, or simply reflected them.

whether their initiations are clustered in *different* years. For example, if FM events detected in Atlanta in 1998 tend to be initiated in 1996, while FM events detected in Philadelphia in 1998 tend to begin in 1994, this would indicate that the actual initiations cluster in a given city.[3]

To be clear, these patterns do not rule out geographic effects in detection efforts, whether they originate with local SEC offices, media, industry regulators, rivals, or even employees. However, by holding constant the sample of detected FM events within each city-year, these observations suggest common underlying behavior by proximally located executives, whatever the (perhaps incomplete) local detection environment may be. Accordingly, the remainder of the paper takes as given that at least part of what we observe reflect differences in actual (mis)behavior, and seeks to better understand the underlying mechanism.

We take up that question in Section 5, which uses the Manski (1993) taxonomy for describing reasons why behavior may correlate between groups. He presents three possibilities. The first, adapted to our context, is that cities differ in terms of long-standing factors like cultural origin (e.g., Minnesota being home to many Scandinavian descendants), wealth, or religion.[4] In the second alternative, what differs across cities is not so much the people, as much as the contextual factors reflected in their local environment, such as economic conditions or enforcement. The final alternative is that the behavior spreads within a region via peer effects, i.e., that social norms are transmitted through interpersonal interactions.[5]

It is straightforward to see that relatively fixed factors do not provide a complete account of our findings, as they do not speak to the regional ebbs and flows indicated in the data. On the other hand, both contextual factors and social norms have the capacity to vary within regions, and consequently, can be consistent with the observed patterns.

Perhaps the most important contextual alternative to social factors is institutional (i.e., non-social) enforcement. While enforcement may induce measurement problems – as we discuss in

[3] An alternative would involve city-specific forensic efforts that retrospectively investigate different time periods, e.g., authorities investigating FM up to (say) one year ago in some cities, up to two years ago in others, etc. This strikes us as implausible, but we have no direct data to falsify this possibility.

[4] 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).

[5] Manski (1993) terms these *exogenous*, *contextual*, and *endogenous*, respectively.

Section 4 – it can also give rise to a second confounding effect through deterrence. That is, if institutional enforcement is stricter in, for example, Dallas versus Miami, executives in Dallas may be less likely to engage in FM.

We address this problem by examining the behavior of other local residents that: 1) are not subject to the same institutional enforcement, but 2) via proximity, are likely influenced by the same social norms. We consider two groups: an area's local politicians, and its doctors. The reasons institutional enforcement should play a minor role (if any at all) for these groups are different. In the former, it is because the corruption of elected officials is enforced centrally, via the U.S. Department of Justice. As Glaeser and Saks (2006) discuss, because "the Federal judicial system is relatively isolated from local corruption, [it] should treat people similarly across space (p. 1054)," and thus, mitigate the impact of local enforcement. In the latter, it is because the activity of interest – physicians receiving and responding to payments from drug companies – is not illegal, and thus, not subject to enforcement.

Both in the cross-section and time-series, we find a strong correlation between city-level financial misconduct and political corruption. This is visually apparent in Figure 5. Moreover, this relation is strongest for the *largest* firms in the region, a finding supportive of a norm-based explanation for two reasons. First, opportunities for social interaction with local politicians are undoubtedly higher for executives of large firms (Johnson and Mitton (2003); Faccio (2006)). Second, to the extent that one is concerned about non-enforcement contextual alternatives like local economic conditions (though, see an explicit discussion of this below), these should be a relatively minor concern for large firms, whose customer base and labor supply are less regionally conecentrated.

A second, similar test involves an area's physicians. Using data on payments (e.g., dinners, speaking fees) from specific drug companies to physicians, combined with the latter's prescription drug referrals, we can rank physicians in each city by their "sensitivity" to payments from the pharmaceutical industry.[6] As with political corruption, we find a strong cross-city correlation with

[6] An example might involve comparing prescriptions for the clinically substitutable "statins," widely prescribed cholesterol reducers with similar mechanism of action. Physicians with financial relationships with AstraZeneca are more likely to prescribe the drug it manufactures (*Crestor*), whereas physicians having received payments from Pfizer are more likely to prescribe its branded alternative (*Lipitor*). For more discussion of the data and measurement of prescription-payment sensitivities, see Engelberg, Parsons, and Tefft (2015).

average rates of financial misconduct. Although many patients would be uncomfortable with the knowledge that physicians' prescription choices may be influenced in this way, there are, as of this writing, no statutes precluding financial relationships between physicians and drug companies, and accordingly, no enforcement of this activity.[7] However, this correlation is consistent with common social norms influencing both types of behavior.

While the above tests are difficult to explain via local enforcement, it is possible for other non-enforcement environmental factors to generate waves in FM. Possibilities include local economic conditions or demographics, either of which may influence a manager's incentive to invest in his/her reputation. To examine the importance of economic conditions we augment our benchmark peer regressions with various measures of city-level economic health and demographics, including education, population growth, age, employment, and income growth. That their inclusion leaves the impact of the peer variables essentially unaltered suggests that alternative measures of economic conditions are unlikely to be meaningful sources of omitted heterogeneity (Altonji, Elder, and Taber (2005)). An additional piece of relevant evidence is that it is the largest firms, for whom local economic conditions are comparatively less important, which are the most sensitive to the FM rates of other local firms.

There are, of course, other contextual factors which, unlike economic conditions, are difficult to include in our regressions. Possibilities include changes in local elections or tax rates, common auditors (e.g., Arthur Andersen had a disproportionate percentage of Houston-based clients), or even extreme weather events like Hurricane Katrina or the Loma Prieta Earthquake that devastated the Bay Area in 1989. Due to the generic nature of this critique, we conclude with a test designed to rule out contextual effects of unspecified form.

Specifically, we borrow from the labor economics literature and employ the so-called "Bartik (1991) instrument." The basic idea is to first use *non-local* variation to instrument for the behavior of some (but importantly not all) firms in an area, and then in the second stage, to look for spillovers to other local firms not (otherwise) subject to the instrument.[8]

[7]The Sunshine Act of 2014 is intended to increase transparency of any financial relationships between prescribers and drug companies, but laws limiting prescribers' behavior do not currently exist.

[8]Recent applications of this methodology include Autor and Duggan (2003), which studies the impact of disability insurance (DI) on employment, and Luttmer (2005), which explores the impact of relative earnings and well-being.

We implement this test among the four cities in the our sample (Atlanta, San Francisco, Detroit, and Houston) characterized by a single, dominant industry. For each of these cities, we use *national-level* variation in the FM rates of each dominant industry as an instrument for the *local* FM rates of firms in that same industry. This eliminates the impact of any generic local contextual factor by construction. Then, in the second stage, we relate these instrumented FM rates of a city's dominant industry to the FM rates of local firms that operate outside the dominant sector.

To give a specific example, suppose that the oil and gas sector incurs a spat of FM from 2008-2010. We use this national variation to instrument for the FM rates of Houston-based energy firms Apache, Halliburton, and ConocoPhillips. However, the dependent variable in the second stage includes only firms outside the energy sector, such as suit retailer Men's Warehouse. The key result is that relative to non-Houston retailers like San Francisco's Gap, Men's Warehouse is more likely to engage in FM when FM rates in the *non-locally measured* energy sector are high. Because there is no local (here Houston-specific) information on the right hand side of the regression, the estimated effects are purged of contextual shocks of unspecified form. Consequently, the chain of causation goes from financial misconduct in the oil and gas industry (which could be generated by economic or other factors operating at the industry level) to changes in social norms in Houston, which influence (perhaps through social interactions) the behavior of non-oil firms in Houston.

The results in this paper contribute to a vast literature that investigates the causes and consequences of financial misconduct. This literature identifies a number of relevant factors, which include firm performance (Harris and Bromiley (2007)), manager or director career concerns (Fich and Shivdasani (2007)), compensation arrangements (Erickson, Hanlon, and Maydew (2006)), institutional monitoring, and the strength of enforcement (Kedia and Rajgopal (2011)).[9] Our study suggests that social norms, identified by observing the behavior of a firm's local peers, is 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

[9]Dechow et al. (2010) provides a comprehensive summary of various proxies of the quality of financial statements (particularly earnings), and their determinants. For the latter, most existing studies focus on firm characteristics (e.g., size (Francis et al., 2005), accounting performance (Collins and Kothari, 1989), leverage (Core and Schrand, 1999; Dechow et al., 1996), managerial compensation (Burns and Kedia, 2006)) as well as industry characteristics (e.g., Biddle and Seow, 1991). In addition to those factors, recent studies examine cross-regional determinants of earnings management, in particular religiousity (Dyreng et al. 2012; McGuire et al. 2012; Grullon et al. 2009).

(1890), economists have sought to understand the reasons behind spatial clustering of firms and 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.[10] 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

Financial misconduct. The primary source for our financial misconduct data is Karpoff, Koester, Lee, and Martin (2013), hereafter KKLM. The paper details the hand-collection of over 10,000 events related to over 1,000 cases of corporate fraud and/or financial misconduct in which the SEC or DOJ file charges.[11] Here, we provide a brief summary of the types of fraudulent events included in their dataset, and refer the reader interested in further details (e.g., regarding the data collection method itself and comparisons with other measures of fraud) to their paper.

In addition to hand-collecting data from primary sources (e.g., the SEC website), KKLM incorporate information from four widely used 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 databases contain (mostly) information on financial statement "restatement" announcements, and therefore are good sources for detecting a firm's attempt to manipulate earnings.[12] The third, the SCAC, maintains a registry of Federal class action securities litigation; compared with the first

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

[11]More precisely, the charges allege corporations of violating: (i) Section 17(a) of the 1933 Securities Act for fraudulent interstate transactions related to the issuance of a security; or (ii) Section 10(b) of the 1934 Securities Exchange Act for manipulative and deceptive devices related to the trading of an already issued security.

[12]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. KKLM dataset distinguishes between intentional and unintentional errors by linking misstatements to subsequent SEC and/or DOJ action. Therefore, this approach "has the advantages of objectivity and replicability, as it relies on SEC and DOJ classifications rather then the researcher's personal judgment." (KKLM, p. 25)

two, this database reflects a wider variety of corporate misbehavior including accounting fraud, fraudulent transfers in mergers and acquisition, misrepresentation, and insider trading. The last database, the AAER, contains releases announcing enforcement or action. There is substantial overlap among all four databases, both in terms of events covered and timing (see KKLM, section 2.3).

A significant advantage of the KKLM data is that it distinguishes between dates when a firm commits FM (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 commit financial misconduct (FM) within a given geographic area. However, some of our tests exploit the revelation/detection dates as well.

Table 1 contains summary statistics of our FM 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 $FM_{j,t}$, a dummy variable denoting financial misconduct by firm j during year t. The average value of $FM_{j,t}$ is 0.0146 across all years and firms, indicating that at any point in time, 1-2% of firms are actively engaging in financial misconduct.[13]

At the city (Panel B) and industry (Panel C) levels, FM is defined using rates instead of dummy variables, e.g., the average FM rate for Seattle in the year 2001 is simply the sum of FM 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 FM rates are similar to the average at the firm level (FM), 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.

While the unit of observation for almost all of our analysis is firm-year, we also perform an analysis (in Table 7) that employs each FM episode as the observation unit. Panel D summarizes the duration (in years) of those episodes. The typical episode lasts 2 (median) to 3 years (mean). About 25% of the cases are detected the year after they begin ($FM\ Duration = 1$), with another

[13]Dyck, Morse, and Zingales (2014) argues, however, that detected FM is likely to be merely the "tip of the iceberg," and that actual rates of misconduct are likely several times higher. We return to this discussion in Section 4.2.

quarter detected two years after initiation ($FM\ Duration = 2$). Duration for the remaining episodes range from three to fifteen years.

Firm location. Our dataset includes firms headquartered within or 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.

Political corruption. Data on federal convictions for corruption-related crimes by elected officials comes from the "Report to Congress on the Activities and Operations of Public Integrity Section," published by the U.S. Justice Department (DOJ). Following Glaeser and Saks (2006), we use the number of DOJ-prosecuted convictions for each DOJ district headquartered in the area.[14] Dividing this number by population results in a conviction rate per 1,000,000 inhabitants in the relevant region for each city. On average, the typical city has a rate of DOJ-prosecuted convictions of 3:1,000,000, but with considerable regional variation (see the penultimate column of Table 2).

Drug company payments to physicians. We obtain data on monetary transfers from twelve major pharmaceutical firms to prescribing physicians from ProPublica's "Dollars for Docs" website (https://projects.propublica.org/docdollars) during the year 2010. Payments are listed by firm, doctor, date, amount in dollars, and activity (if any), such as a speaking engagement, consulting arrangement, dinner, or gift. Transfers are common, with about 60% of doctors receiving transfers from one or more of the firms in our sample. We merge these payment data with Medicare (Part D) prescriptions for each physician, using ProPublica's Prescriber CheckUp

[14] Each area in our sample contains at least one DOJ district headquarter (e.g., the North Texas DOJ district is heaquartered in 1100 Commerce Street, Dallas, TX 75242). The only exception is Orlando, which is part of the Middle Florida DOJ district whose headquarters are in Tampa.

(projects.propublica.org/checkup/).[15] Of particular interest is the number of prescriptions corresponding to each drug-doctor combination, e.g., Doctor X writes 350 prescriptions for *Crestor* (manufactured by AstraZeneca), Doctor Y writers 180 prescriptions for *Lipitor* (Pfizer).

The resulting variable of interest is the prescription-payment sensitivity, which captures the cross-sectional relation between payments by a drug company to a given doctor, and prescriptions written (by that doctor) for the paying firm's drugs.[16] The last column of Table 2 summarizes these sensitivities by city. Taking Indianapolis as an example, the reported amount indicates that on average, the typical Indianapolis-based doctor writes 17 additional prescriptions for companies with whom he/she has a financial relationship, relative to those where no such arrangements exist. Higher numbers indicate a steeper slope between payments and prescriptions.

Other variables. Finally, our tests include a number of standard control variables, all of which are obtained from standard sources. Stock returns are from CRSP, firm fundamentals from COMPUSTAT, and CEO age from EXECUCOMP. Most of our FM 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 for these variables are reported in Panel A of Table 1.

3 Regional patterns in financial misconduct

In this section, we establish some basic patterns, quantifying the extent to which FM tends to be regionally clustered. We begin in subsection 3.1 by documenting large differences in the average rates of FM across cities. Then, in subsection 3.2, we extend the analysis to an empirical framework that considers time-variation in FM within each city. The workhorse models here are logistic regressions predicting FM at the firm-year level. After controlling for traditional determinants of FM, including various firm and industry factors, we nevertheless find that the FM rates of a firm's local neighbors is a strong predictor of its own FM activities. Subsection 3.3 provides a refinement

[15]ProPublica obtained these data from a Freedom of Information request, and made them available (and searchable) online. Prescriber Checkup also lists various details about each physician, including full name, address, and specialty.

[16]For more details on the data and methodology, see subsection 5.1 later in the paper, as well as Engelberg, Parsons, and Tefft (2015).

of these results, ranking a firm's local neighbors by two proxies for social interaction: firm size and CEO age. As we will see, firms tend to 'mimic' the FM behavior of neighbors particularly when they match on these variables.

3.1 Average FM rates 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, $FM_{j,t}$, taking a value of one if firm j is prosecuted for financial misconduct in year t. 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 FM 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, economic effects, or changes in the sample composition toward industries more/less apt to engage in FM. 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, based on the Fama French-12 classification. Here too, the R^2 is quite low, but the significance of the industry fixed effects is strong, significant at the 1% level, and indicative of persistent cross-industry differences in financial misconduct. The industry with the highest average FM rate over our sample is software, with approximately 1.9% of firm-years being associated with an FM event. At the other end of the spectrum, the health care and energy sectors are least likely to commit financial misconduct, 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 appreciated by examining Table 2, which reports 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 average annual FM rates 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 FM rate exceeding 2%. The regression model in the third column 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 Time series variation of FM within cities

Although the fixed effects regressions in Table 3 indicate long-lived differences in the financial misconduct propensities of firms located across different geographic regions, one obvious objection is the 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 incentives to commit financial misconduct. To address this concern, we estimate logistic models of firm-level FM events:

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

As before, $Pr(FM_{j,t})$ is the probability that firm j commits financial misconduct in year t. The main coefficient of interest is β_1, measuring whether, at a given point in time (t), firm j is more likely to commit FM when local firms (a) *outside* its industry ($-i$) commit more FM. Similarly, β_2 measures the influence of the FM rate of the firm's same-industry, local peers ($FMRate_{-j,t}^{i,a}$). 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 due to the incidental parameters problem (Chamberlain (1980)), we include as a control variable the yearly average of FM rates for firms in the same industry (i), but located outside the firm's city ($-a$). Yearly fluctuations in $FMRate_t^{i,-a}$ capture industry dynamics, implying that any local effects (β_1 and β_2) are identified net of these. Additional $Controls$ include the average FM 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 financial misconduct in a given year (say 2005). In this case, we would control for the FM 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 financial misconduct, 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 FM rates of a firm's local, non-industry peers increases the odds ratio of it committing FM by about $e^{.0811} - 1 \approx 8.45\%$. Against a baseline average FM rate of 1.46%, this implies an FM rate of about 1.59% when a firm is surrounded by non-industry peers whose average FM rates is one standard deviation above the mean, with an equal sized reduction (to about 1.31%) for a one percent decrease in surrounding firms' average FM rates. The interquartile range of $FMRate_t^{-i,a}$ 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 financial misconduct (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 FM investigations, which is consistent with fraudulent accounting being, at least temporarily, effective in fooling the market.

In the second column, we estimate firm-level FM sensitivities to industry FM 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 FM 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 pure (non-local) industry portfolio.* Moreover, most (about 80%) of the significance of the local portfolios comes from firms outside the firm's main industry.

The fifth column adds to the model $\overline{FMRate}^{-i,a}$, 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, $FMRate_t^{-i,a}$, 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 FM rates in each portfolio are converted to discrete variables, like the firm-level FM indicator itself. In each case, "High FM" takes a value of one if the average FM 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 FM rates above 1.2%, FM rates are elevated by about 46%, or about 67 basis points against a benchmark average FM rate of 1.46%.

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 financial misconduct waves is not consistent with slowly trending city-level attributes such as differences in wealth, culture, religion, or ethnic background, since these do not fluctuate appreciably year-to-year.

3.3 A refinement: within local social networks

Table 5 presents the same results, but for each firm-year, splits the local, non-industry portfolio into two mutually exclusive groups based on proxies for the likelihood of social interaction between respective managers. The first proxy is firm size, and the second is CEO age. The intuition is that if social norms are transmitted locally, links in behavior should be stronger for those belonging to the same social network. Although admittedly noisy, each proxy is likely to be at least somewhat informative in this respect.

Starting 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)).[17] 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/social 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. 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 how close they are in age.

Panel A of Table 5 presents the size split, and Panel B the age split. For the size 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 local portfolio of a firm's non-industry peers ($FMRate_t^{-i,a}$), we estimate the sensitivity to two, mutually exclusive local covariates, $FMRate_{large,t}^{-i,a}$ and $FMRate_{small,t}^{-i,a}$.

Column 1 shows the estimates only for large firms, and column 2 only for small firms. In

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

both cases, a clear pattern emerges: each group is much more sensitive to the behavior of its size-matched local counterparts, compared to those in the other local 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 FM rate for each city (as we did in Table 4), resulting in minimal changes 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.

To summarize the findings of this section: 1) average rates of FM vary considerably by city, across broadly defined industry sectors, 2) FM tends to occur in local 'waves,' such that the likelihood of a firm committing FM depends on the contemporaneous FM rates of its locally headquartered neighbors, and 3) the sensitivity of a firm's FM to that of its neighbors is much stronger if peers are similar in size, and to some extent, when matching on CEO age.

4 The measurement problem

Because we do not observe actual occurrences of FM, but rather instances when formal enforcement action is brought against a firm, we need additional tests to corroborate that a city's executives are actually committing FM at the same times, rather than simply being caught simultaneously. To fix ideas, denote the average propensity of a firm in city i to commit FM as X_i, and the conditional probability of being detected as Y_i. What we observe is the product X_iY_i, rather than either X_i or Y_i separately. Consequently, differences in detected FM can reflect variation in X_i, variation in Y_i, or both simultaneously. In this section, we provide evidence that indicates that a significant portion of the variation in X_iY_i reflects variation in X_i.

4.1 Local whistle-blowers?

To assess the importance of regional factors in detection, it is useful to consider which parties typically expose financial misconduct. Dyck, Morse, and Zingales (2010) track 216 cases of corporate fraud in detail, paying specific attention to how these events were exposed and became public. Their headline finding is that no single entity plays a dominant role: industry regulators, law firms, equity holders, the national media, and industry competitors all bring financial misconduct to light, with no party being responsible for more than 20% of detections. Perhaps the most surprising result is the relatively minor role played by the Securities and Exchange Commission (SEC), which blew the whistle on merely ten cases in their sample, or about 7%.

For us, the most important question is whether whistle blowers concentrate their forensic efforts on specific cities (Tables 2), during certain times (Tables 4), and for specific groups within those cities (Table 5). In some cases, this seems plausible. For example, if equity holders tend to hold stocks of local companies, they may more carefully scrutinize local management. If local investors in some cities happen to be particularly vigilant, we might see more misconduct detected there, versus other cities. Similarly, local auditors may be stricter in cities than in others.

Although the SEC appears relatively unimportant as a whistle-blower for FM, a "tough" officer rotating to a local office may heighten the chance that FM is detected and/or prosecuted, as shown by Kedia and Rajgopal (2011). Broadly consistent with this idea, the rate of FM detection in our sample is slightly higher in cities containing an SEC regional office (1.2%) compared to those that do not (1%), though as reflected in Table 2, this variation is small compared to the overall cross-sectional variation in detected FM.[18]

For most of the remaining players identified in Dyck, Morse, and Zingales (2010), it is harder (though not impossible) to imagine geographic motives being first order. The financial media, for example, exposed almost one-sixth of fraud cases in their sample, 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-wide enforcement can generate our main results.

Nonetheless, to the extent that institutional detection efforts are concentrated by regions, and vary meaningfully over time within them, some of the patterns documented in the last section may represent common detection, even if the underlying rates of FM are similar. The next two subsections describe analyses that are less subject to this concern.

4.2 The Arthur Andersen experiment

Following the accounting scandal of Houston-based energy giant Enron in the Fall of 2001, auditor Arthur Andersen (AA) was indicted (March 2002) and later convicted (June 2002) of obstruction of justice. It immediately surrendered its license to practice as Certified Public Accountants, which effectively forced its corporate clients to seek alternative auditors. In an innovative paper, Dyck, Morse, and Zingales (2014; DMZ) use the transition to new auditors as a quasi-exogenous shock to the detection of ongoing financial misconduct since, for example, Ernst and Young or KPMG would have little incentive to overlook fraud initiated or perpetuated under AA's auditorship.

Following the notation above, if a firm's new auditors catch all FM events perpetuated under

[18] The cities containing regional SEC offices are Washington, D.C. (headquarter), Atlanta, Boston, Chicago, Denver, Fort Worth (Dallas economic area), Los Angeles, Miami, New York, Philadelphia and San Francisco. Salt Lake City also houses a regional office, but is excluded from our sample because it is not one of the top 20 cities in terms of corporate headquarters. Including a dummy variable indicating whether a city has an SEC regional office has virtually no effect on our main regressions.

AA's regime, then $Y_i = 1$ for the subset of firms in city i that, prior to 2002, were clients of AA. There is no easy way to tell how close the AA experiment approaches complete detection for switching firms, but as DMZ discuss, multiple factors suggest that the majority of ongoing fraud was likely exposed.

First, as mentioned above, post-AA auditors had no incentive to certify pre-existing fraud. Moreover, given that AA has presided over the most egregious corporate fraud case in U.S. history, suspicion regarding its other clients was likely high. Third, the types of events in the KKLM dataset – financial misconduct generally related to accounting – is precisely the type of fraud auditors would have the expertise and information to detect. Finally, the magnitudes reported by DMZ are enormous: new auditors uncovered fraud about four times the rate as during the years prior to the switch, leading the authors to speculate that at any point in times, perhaps 15% of firms are actively engaging in some type of fraud. Rates much beyond this seem implausible.

To the extent that the combination of these factors leads to complete, or near complete, detection, the AA experiment permits a convenient way to approximate the cross-city variation in actual FM. If detection is virtually complete among the sample of AA-switchers, then trivially, $Y_i \approx 1 \implies Var(XY) \approx Var(X)$.[19] We take the FM rates of ex-AA clients in the years immediately after switching (following DMZ) as cross-city estimates of FM largely purged of detection effects. Then, we compare this 'clean' city ranking to that calculated using the rest of the sample, i.e., firms having no prior association with AA.

Under the null hypothesis that FM rates are identical across cities, i.e., $Var(X) = 0$, then the variation in XY comes solely from differences in detection probabilities. Thus, to the extent that detection probabilities are fixed across cities – which we take to be the case among the sample of ex-AA firms – there should be little remaining cross-sectional variation. Further, if variation in detection probabilities (Y_i) is responsible for the variation observed in our benchmark results, the

[19]Note that full detection is not necessary for this rank comparison to remain valid. Expanding the variance of XY, and ignoring the covariance terms (to which we return in the next section),

$$\sigma_{XY}^2 = \sigma_X^2 \sigma_Y^2 + \mu_Y^2 \sigma_X^2 + \mu_X^2 \sigma_Y^2.$$

As long as the variance of detection probabilities across cities is small for firms that switched auditors, i.e., $\sigma_Y^2 \approx 0$, the remaining variance in XY will only depend on the variance of X. The scaling parameter μ_Y changes the absolute variation of XY, but will leave the ranking unchanged relative to the case in which $Y_i = 1$ for the set of switching firms.

ranks between ex-AA clients and other firms in the same city should bear little relation to one another.

The results in Table 6 clearly reject this hypothesis. Cities with high rates of detected FM among ex-AA clients tend to have high rates of detected FM among non-clients of AA. The rank correlation is 59% ($p < 0.001$), and is plotted in Figure 2. The strength of this relation is remarkable given that several cities had very few AA clients (e.g., Indianapolis (6), Orlando (13), and St. Louis (18)), and thus provide noisy measures of FM for the AA-client group.

4.3 Ex post detection versus ex ante initiation

The previous subsection addresses cross-sectional differences in detection by exploiting a unique experiment in which detection probabilities are, for a subset of firms across cities, likely to be very similar. Here, we have the same goal – accounting for city-level differences in detection effects – but approach the problem from a different perspective. Rather than comparing FM rates between different types of firms within a city (i.e., ex-clients of AA and other firms), we compare the start dates of FM events, *conditional on a sample of FM events detected in the same year and city*, in order to hold constant the detection environment. In other words, for FM events detected in a given city-year pair, we are interested in their duration – having started one year ago, two years ago, three years ago, and so on. The presence of city-specific clustering in start dates would be interpreted as evidence of common behavior, rather than (or perhaps in addition to) common detection.

The identifying assumption here is that potentially dynamic, city-specific detection efforts do not, by themselves, create city-specific clustering in detected start dates. That is, we are taking as given that if a tough SEC officer rotates to the Atlanta office in 2007, or an activist investor investigates Seattle firms with special vigilance in 1999, these parties do not systematically limit themselves to retrospective efforts of certain horizons – i.e., the Atlanta SEC officer only exploring FM cases extending up to one year ago (2006), while the Seattle-based investor investigates two years back (to 1997), etc. While not impossible, this strikes us as fairly implausible, especially against the alternative of common clustering in underlying behavior. In any regard, we note this caveat when interpreting our results.

To visualize the distinction our analysis is trying to make, consider Figure 3, which shows hypothetical FM cases for four firms each in Atlanta and Philadelphia. In both panels, the number of ongoing FMs in each city is identical, when standing in the year 2007. The key to our test comes from comparing Panels A and B, which have the same overall number of FM events, but differ in the distribution of *start dates*. In the left panel (A), the distribution of start dates (and thus FM duration) is the same between the two cities, with half starting in 2006, and the other half in 2005. In the right panel (B) however, the distribution of duration is different, with most ($\frac{3}{4}$) of Atlanta's FM starting in 2006, and most of Philadelphia's a year earlier. Even if Atlanta and Philadelphia differ in terms of overall detection probabilities, this will not (given the caveat above) generate city-specific differences in conditional start dates.

Essentially, we want to see whether Panel A or Panel B provides a better description of the empirical patterns – i.e., whether there exist city-specific start dates for a given city-year 'vintage' of detection. We operationalize this with the following Poisson regression model:

$$
\begin{aligned}
FM\ Duration_j \mid Detected\ at\ t = \beta_1(FM\ Duration^{-i,a} \mid Detected\ at\ t) + \qquad (2)\\
\beta_2(FM\ Duration^{i,-a} \mid Detected\ at\ t) + \qquad (3)\\
\beta_3(FM\ Duration^{-i,-a} \mid Detected\ at\ t) + \\
\gamma Controls_{j,t} + \epsilon_{j,t},
\end{aligned}
$$

The dependent variable, $FM\ Duration_j \mid Detected\ at\ t$ is a count variable (in years), measured at the firm level. For every case of detected FM, we calculate the number of years from the initiation date (determined retrospectively as a result of investigation) to the detection date. Empirically, about 25% of cases are detected the year after they begin, with another quarter detected two years after initiation (27%). Duration for the remaining events range from three to fifteen years, with no single duration accounting for more than 15% of our observations, with most less than 1%.

There are three covariates. The primary one of interest, $FM\ Duration^{-i,a} \mid Detected\ at\ t$, corresponds to the average duration of all FM events detected at t, for firms headquartered in the same area (a), but in different sectors ($-i$). Coefficient β_1 indicates whether, for a set of FM events detected in the same city-year, start dates (durations) tend to be clustered by city. In the examples shown in Figure 3 Panel A, there is no such clustering ($\beta_1 = 0$), but in Panel B, there is ($\beta_1 > 0$).

The second and third covariates, presented for comparison, are calculated similarly, but for a firm's out-of-area ($-a$) industry peers, as well as the average duration for the market ($-i, -a$) as a whole. Coefficients β_2 and β_3 capture cross-sectional or temporal effects in the duration of detected FM respectively, at the industry and market level. These would account, for example, for FM duration increasing over the sample period (i.e., if firms get better at evading detection), or if FM in some industries takes longer to detect than others. Finally, we include various firm level $Controls$, measured at the firm (j, t) level. Note that relative to our previous regressions, which predict FM detection, the number of observations is dramatically reduced ($N = 184$).

Table 7 shows the results, presented as marginal effects. In the first column, the marginal effect of β_1 is estimated to be 0.32, with a t-statistic of 2.79. To interpret this coefficient, suppose that in 2007, we know that FM has just been detected for Atlanta-based retailer Home Depot. When trying to predict when HomeDepot's FM began (2006? 2005? 2004?), β_1 indicates that for every yearly increase in the FM duration of other Atlanta-based firms (e.g., CocaCola or United Parcel Service) who were also prosecuted in 2007, we expect an increase in the duration for HomeDepot's FM of about four months.

Importantly, the magnitude and significance of β_1 is stable in the second and third columns, suggesting that this phenomenon is not explained by either trends at the market or industry level. In the final column which includes firm-level controls – only one (Q) of which predicts FM duration – we note that of the three covariates, the city-level rate is, by far, the strongest predictor of FM duration. For a visual representation of this relation, see Figure 4, which plots on the y-axis the market-adjusted (using $FM\ Duration_t^{-i,-a} \mid Detected\ at\ t$) duration for each FM event at the firm level, and on the x-axis the market-adjusted city-level average duration. The positive relation is clear, and confirms the regression results reported in Table 7.[20]

Overall, the evidence in Table 7 suggests that while regionally correlated whistle-blowing may provide a partial explanation for the clustering of detection dates, nearby executives seem to be initiating financial misconduct at the same time. Combining this with our earlier analysis of Arthur Andersen's clients suggests that the measurement problem is unlikely to drive the time-series and

[20] The influence of outliers is clearly apparent. The estimated slope more than doubles if data are winsorized at the 2.5% level.

cross-sectional geographical patterns in financial misconduct that we observe.

5 Why do we observe FM clustering?

The results in the last section indicate that at least part, and potentially most, of the regional clustering in misconduct can be attributed to clustering in FM occurrence. In this section, we take this as given, and attempt to better understand the nature of city-level factors giving rise to these apparent differences in behavior.

When thinking about these local influences, we are guided by Manski's (1993) description of what he calls the *reflection problem.* Broadly speaking, behavior can differ between groups (cities in our context) because of differences in: 1) inherent attributes like geographical features or cultural origin (e.g., Minnesota being home to many Scandinavian descendants), 2) environmental factors (e.g, enforcement or local economic conditions), and 3) social/peer effects (e.g., ethical norms).

On its face, it is clear that the first group of factors, because they are relatively static by definition, do not provide a complete explanation of our findings. Although they may have something to say about the average differences between cities (Table 2), they cannot account for the regional ebbs and flows indicated in Tables 4 and 5. On the other hand, because both environmental (type 2 above) and social effects (type 3) are potentially dynamic, they are more difficult to distinguish.

Perhaps the most important 'contextual' alternatives to peer effects are regional differences in enforcement. As we have already seen Section 4, cross-city variation in enforcement can create a measurement problem, since we observe only the subset of FM events that were subsequently detected. Here, the concern is different. If executives are deterred against committing FM because of stringent local enforcement, cities will differ in the rates at which FM is actually committed (rather than simply detected). Accordingly, our empirical strategy is to examine the behavior of other local parties *not* subject to the same enforcement, and thus, deterrence effects (subsection 5.1). If we nevertheless find correlated misbehavior between these groups, we can conclude that some other factor than enforcement is responsible.

Possibilities here include local economic conditions or changing demographics. For example, perhaps fluctuations in the regional economy may change the incentives to engage in financial misconduct, i.e., executives in declining cities worrying less about reputation.[21] Or, maybe areas become more/less educated, wealthy, or religious, all of which have the potential to alter behavior. Using the model presented in Table 4 as a benchmark, we include numerous measures of local economic conditions and demographics as explanatory variables in subsection 5.2. Yet, these variables appear to have very little impact on our results, suggesting that the regional correlations in misbehavior we find operate independently of local economic shocks or demographics.

While we believe that enforcement-based deterrence and local economic conditions/demographics are the most likely confounding environmental factors, they are not exhaustive. Accordingly, in our final tests in subsection 5.3, we adapt an approach from the labor economic literature, using instrumental variables to rule out the effects of *any* local environmental influence(s) by construction.

5.1 Local parties not subject to common enforcement

In this section, we present evidence not easily reconciled with enforcement-based deterrence. The strategy is to examine regional clustering of unethical behavior *not* subject to the types of enforcement factors that may deter local executives from committing FM. First, we consider a region's politicians, and second, its prescribing physicians.

Politicians. We first examine the corruption-related activities of a city's elected officials. As mentioned in the data description in Section 2, 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."[22] The types of activities prosecuted include electoral fraud, conflicts of interest, campaign violations, and obstruction of justice. The analysis here correlates the rate of FM to the per-million capita number of Federal convictions for corruption-related crimes, both in the cross-section and in the time-series within cities.

A key advantage of this corruption measure is that it is largely unaffected by local enforcement efforts, with prosecutions being conducted at the Federal level, by the U.S. Department of Justice. This does not, of course, preclude complementarities in enforcement – e.g., local whistle blowers

[21]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.

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

may detect the contrails of misbehavior, which authorities in Washington D.C. may use in their own detection and enforcement efforts. Still, because the relevant authorities and/or whistle-blowers are largely distinct, common enforcement would seem less relevant for comparisons between executives and politicians, rather than those between two groups of executives.

To appreciate the lack of regulatory/enforcement overlap, recall the earlier discussion of Dyck, Morse, and Zingales (2010), which identifies the specific parties responsible for exposing corporate fraud. Most of these entities would have little incentive to investigate and/or expose political corruption including financial analysts, auditors, clients or competitors, employees, equity holders, the SEC, or short sellers. These entities in aggregate exposed over 70% of the fraud cases in Dyck, Morse, and Zingales (2010). Only the media appears to have a plausible detection role in both settings, but even here, details of the Dyck, Morse, and Zingales (2010) study make this unlikely. First, the media was credited with exposing corporate fraud only in cases where another whistle-blower could not be identified (see their Table 1). Second, all such media-generated exposures involved national, not local, media outlets. Thus, the remaining concern would seem to involve national media outlets focusing their forensic efforts on a particular city and time, and spanning both executives and elected officials.

Although we characterize the relation more fully below, the regional correlation between political convictions and FM is apparent in inspecting Table 2. To facilitate this inspection, Figure 5 provides a graphical representation of the correlation, plotting the 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.

Doctors. In our second test, we measure the average 'sensitivity' of a region's physicians to pharmaceutical payments, when making their prescribing decisions. That is, are doctors in some cities more likely to write prescriptions for drugs from companies from whom they have received gifts, dinners, or other payments? Crucially, although ethically questionable, there are no current statutes either forbidding such financial relationships, or regulating physicians' medical decisions potentially influenced by them. Consequently, regional differences in this payment-prescription sensitivity cannot reflect enforcement or variation thereof.

To estimate these differences across cities, we collect data on payments to physicians provided on the Dollars for Docs website, a searchable web interface allowing a user to observe transfers from pharmaceutical companies to specific physicians, hosted by independent journalist consortium ProPublica.[23] In 2010, eleven pharmaceutical firms reported payments on Dollars for Docs, including most major drug firms such as Pfizer, Merck, Glaxo, Astrazeneca, and Johnson and Johnson. Payments are broken down by both dollar amount and type, e.g., gifts, meals, speaking, travel, consulting, and on occasion, proprietary research. To this dataset we merge Medicare (Part D) prescription information for each doctor, also in the year 2010. Together, these data sources allow us to test for an association between pecuniary transfers and the 129,594 registered physicians practicing in economic areas corresponding to the twenty largest U.S. cities.

Our primary tests are cross-sectional regressions, comparing prescribing patterns of doctors who differ in whether, or how much, they are paid by a given drug company. Specifically, we estimate the following regression, with all variables measured in the year 2010:

$$Scripts_{doc,firm} = \beta Payment_{doc,firm} + \gamma GenScripts_{doc} + \phi Specialty_{doc} + \epsilon_{i,j}. \tag{4}$$

The unit of observation is at the doctor-company level, such that the dependent variable is the number of prescriptions written by a given doctor, for drugs manufactured by a given company. We are interested in the coefficient β_1, which tells us, on the margin, whether payments between a given doctor-firm pairing are associated with more prescriptions. In addition to $Payments$, we also control for the number of generic prescriptions written by each doctor, in order to account for differences in general prescription intensity. Also included are dummy variables for 416 precisely defined medical specialties, such as pediatric oncology or oculoplastic surgery.

We parametrize $Payment_{doc,firm}$ with a dummy variable, taking a value of one if any payment is observed for that doctor-firm pair, and zero otherwise.[24] Table 2 indicates that on average, the presence of a payment – the median of which is a mere \$61 – is associated with about 14 additional prescriptions for the paying firm's drugs. However, more interesting is the variation across

[23]The data and methodology are described in much greater detail in Engelberg, Parsons, and Tefft (2015). We are especially grateful to Joey Engelberg, who was responsible for the original dataset, and graciously permitted us to adapt this dataset to the current context.

[24]However, we note that the results are quite robust to alternative specifications, including continuously measured payments, flexible dummy variables for small/medium/large payments, logarithmic transformations, and various other alternatives; see Engelberg, Parsons, and Tefft (2015) for more details.

cities, with Miami's doctors being almost three times as sensitive (39 prescriptions) compared to the average, and Minneapolis's physicians much less so (5). Further, like we saw with political corruption, cities ranking high in prescription-payment sensitivity also rank high in financial misconduct rates, e.g., Miami has both the highest financial misconduct rate as well as the highest prescription-payment sensitivity (and the second highest political corruption rate).

Before proceeding, it is worth mentioning that there are numerous reasons to expect a positive relation between payments and prescriptions. Possible mechanisms include reverse causality (i.e., where doctors already prescribing a company's drugs are rewarded with payments), information being transferred to doctors from meetings with pharmaceutical representatives, and simple rent-seeking and/or corruption, whereby the payments themselves provide incentives for doctors to tilt prescriptions in the way observed. Although a detailed discussion of the various mechanisms is beyond the scope of this paper – see Engelberg, Parsons, and Tefft (2015) instead – note that the correlations observed in Table 2 provide prima facie evidence for a rent-seeking explanation. It would be a striking coincidence if Miami's doctors, where political corruption and financial misconduct rates are very high, happened to find educational offerings by pharmaceutical representatives more persuasive than those in Minneapolis, where corruption of any type is unusual; the latter is in the bottom three for all three misconduct proxies examined in this paper.

Accordingly, we take city differences in the prescription-payment relation as a proxy for the extent to which ethically questionable behavior is tolerated within an area. Importantly, and as mentioned before, this activity is neither regulated nor enforced, making any relation with FM unlikely to be driven by detection or deterrence effects.

Results. To more formally characterize the relation between political, medical, and corporate corruption, we re-estimate the benchmark logistic regression shown in Equation (1), but instead of the corruption-related activities of a firm's corporate peers, we include each city's rate of local political corruption and medical rent-seeking as covariates. Recall that we only observe medical rent-seeking behavior in 2010; so, $MedCor$, cannot enter the specification dynamically. We do, however, observe time-series variation in political corruption within cities, but to facilitate comparison with $MedCor$, we first include each city's average rate of political convictions, $\overline{PolCor}$, averaged over the sample period. Firm-level controls – measured annually – are identical to previous tables.

Our estimates of these regressions are presented in Panel A of Table 8. In the first column, we relate the probability of corporate FM 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 5 that cities ranking high in political corruption also rank high in corporate corruption.

In the second column, we relate the probability of FM to the average payment-prescription sensitivity of each area's local doctors. The coefficient is 0.0179 ($t = 3.97$), translating to an increase in the odds ratio of $e^{0.0179} - 1 \approx 1.81\%$, indicating that cities whose physicians' are more sensitive to payments from medical companies also rank high in corporate corruption.

In the third column, we include both $\overline{PolCor}$ and $MedCor$ in the same regression. The parameter estimates are similar, though marginally stronger, compared to the previous columns, suggesting that the two proxies for regional misbehavior are distinct. Column 4 presents the same regression, but with clustering at the city level, and thereby accounting for arbitrary correlation in the residuals across firms within a city, or over time within firms. This reduces the t-statistic on the political variable to 1.89, but leaves the significance of $MedCor$ virtually unchanged.

Because the means and variances of $\overline{PolCor}$ and $MedCor$ are different, inferring the magnitudes of their respective coefficient estimates from columns 3 and 4 is difficult. Accordingly, in columns 5 (city-year clustering) and 6 (city clustering), we present the coefficient estimates from regression models where, instead of including the raw rates for each variable, we include as covariates the rank of each city in each respective dimension (i.e, Minneapolis is assigned a rank of 2 for $\overline{PolCor}$ and 1 for $MedCor$). In these regressions, the coefficients are comparable to one another, although the magnitude and statistical significance of $MedCor$ is somewhat higher. In the case of medical corruption, the parameter estimates suggest that moving up five slots in the 20-city ranking – say, comparing Atlanta (11^{th}) to Chicago (16^{th}) – is associated with an increase in detected FM of $e^{5*.0183} - 1 \approx 10\%$ or, relative to the baseline average of 1.20%, an increase in the rate of observed FM in the city to 1.32%. The corresponding effect for $MedCor$ is about 12.5%.

Recall that unlike $MedCor$, which can be estimated only cross-sectionally, we observe data on political convictions for each year of our time-series. Consequently, we can test whether, within a given city, spikes in convictions for corruption-related offenses coincide with spikes in detected FM committed by local executives. We conduct this analysis in Panel B of Table 8, which regresses the city-level aggregate FM rate on the rate of local political corruption. The advantage of running the regression in aggregate rather than at the firm level is twofold. First, with a single observation for each city, any remaining concerns (Panel A showed results clustered at the city- and city-year level) about correlated residuals across firms in each city are removed by construction. Second, the dependent variable can now be defined as a continuous rate of FM defined at the city-year level, rather than as a discrete indicator variable at the firm-year level. Consequently, we can estimate the model using OLS with city fixed effects (which the incidental parameters problem makes undesirable for logit models), providing a more robust account of cross-regional covariation in financial misconduct and political corruption.

Consistent with Panel A, Panel B indicates that cities ranking high in political corruption are associated with high rates of FM. In the first column, we regress city-level FM rates against the rate of political convictions observed the same year (t). The point estimate indicates that in relation to a standard deviation increase in the rate of political fraud (about three additional convictions per million inhabitants), corporate FM rates increase by ($2.9 \times 0.0425\% \approx .12\%$). Recalling that the average rate of corporate FM is a little over one percent, this represents a meaningful increase on a percentage basis. Note that because we include city fixed effects, the relevant source of variation is time-series fluctuation in both political and corporate corruption within each city.

In the next column, we lag political convictions, which results in an even stronger pattern, both statistically and economically. When we include both the lagged and contemporaneous rate of political convictions simultaneously, only the lagged rate maintains statistical significance. Some of this likely due to power – the lagged rate implicitly captures the effect of all previous lags (i.e., $t-2$, $t-3$, etc.), whereas the contemporaneous rate captures only innovations between $t-1$ and t. Indeed, in unreported results, we observe a significant relation for up to three years between lagged political corruption and current FM; notably, the opposite lead-lag relation is absent entirely. One interpretation is that to the extent that these effects capture social norms, local executives take their cues disproportionately from local politicians, rather than vice versa. (This is admittedly highly speculative, and secondary to the purpose of this section, which is to address common enforcement effects.)

While local enforcement effects are unlikely to explain the time-series in corporate and political corruption, perhaps fluctuations in local economic effects simultaneously drive both. In column 4, we include a number of economic and demographic variables. Though there appears to be some evidence that a stronger job market is associated with less FM, the relation with political corruption remains unchanged. (See the next section for a more detailed analysis of economic and demographic factors.)

The last four columns show the results of the same analysis, but broken down into FM rates for a region's large (above median size) and small firms. There are two reasons for this decomposition. First, it helps further rule out local economic conditions as simultaneous drivers of both FM and political corruption (though column 4 provides no evidence of this). Intuitively, large companies with more geographically diversified customer bases and labor forces should be less exposed to local economic conditions surrounding its headquarters. The second reason is that compared to executives of small firms, opportunities for social interactions with politicians is almost certainly elevated for large-firm CEOs and other top management.[25]

Comparing the results for large firms (columns 5 and 6) to those involving small firms (columns 7 and 8), we see that fluctuations in local political convictions has a much stronger effect for the former. With or without economic controls, the effect is 60% larger for large firms. Note also that, as expected, FM rates for small firms are sensitive to proxies for local economic conditions (albeit with opposite signs).

5.2 Local demographic and economic trends

While the results in the last section are difficult to reconcile with regional differences in enforcement, there are other possible local shocks which may have a similar effect. Examples include local economic conditions, wealth, education, population growth, or other demographic variables.

To explore these issues, we augment Equation (1) with various measures of demographics and local economic health. We report this analysis in Table 9. To ease comparison, the first column

[25] Johnson and Mitton (2003) and Faccio (2006) report that larger firms are more likely to exhibit political connections. Faccio, Masulis, and McConnell (2006) report that politically connected firms tend to be larger than non-politically connected firms, and are more likely to receive bailouts even when they tend to perform worse during and following the bailouts.

reproduces the penultimate column of Table 4. Column 2 adds population growth, along with two lags. Employment growth and wage growth are added in columns 3 and 4, respectively. None of these variables appear related to local rates of FM, and more importantly, have virtually no impact on the variable of interest, the FM rate of a firm's local peers, $FM\ Rate_t^{-i,a}$. Columns 5 and 6, likewise, show virtually no effect for local measures of education or religiosity, the latter obtained from the Association of Statisticians of American Religious Bodies and the Glenmary Research Center. When all of these variables are included simultaneously, though current employment growth becomes statistically significant at the 5% level (switching sign relative to column 3), the vast majority do not appear related to FM.

As a further check on intuition, columns 8 and 9 split the sample into large and small firms. The idea is that because of their less localized customer bases and labor supply, the importance of local economic and/or demographic factors should be less important for larger firms. On the other hand, any peer effects would not expected to be weaker, and arguably could be stronger due to the enhanced opportunities for social interactions between executives of large firms (e.g., Gao, Engelberg, and Parsons (2012)). Indeed, columns 8 shows that among large firms, there is (expectedly) no evidence whatsoever that local variables matter for predicting FM. Moreover, the estimated impact of a firm's local peers is virtually identical to the full sample, and remains strongly significant.

5.3 Bartik test for peer effects

The analyses in the preceding subsections consider two specific types of environmental factors that might conceivably differ across cities: enforcement and economic conditions. Neither provides a satisfactory account for the findings presented in the preceding two sections, and although we view these as the most important possibilities, there are others. Accordingly, in this section, we present analysis intended to rule out the effect of *any* generic local, environmental influence.

To do so, we borrow from the economics literature, and apply the so-called 'Bartik' test (1991), which uses non-local variation as an instrument for some, but importantly not all, members of a particular group (here cities). Recent example of this approach involve the identification of relative consumption effects (Luttmer (2005)) and spillovers from the effects of disability insurance (Autor and Duggan (2003)).

In our context, the goal is to obtain variation in FM propensity for a subset of a city's resident firms, and then to look for spillovers to other local firms, i.e., those not otherwise influenced by the instrument. We use our earlier finding that financial misconduct 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 FM 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 FM using the FM 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 FM rates of firms in Houston's energy sector means that time-varying, local contextual (e.g., Houston-specific) 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 10, we formalize this test in an instrumental variables regression. We estimate a variant of Equation (1), with two main changes. First, we estimate firm-level FM with a linear probability model. 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, $FM_t^{Dom,a}$, is the average FM rate of firms in the city's dominant industry (e.g., Houston energy firms). We instrument for fluctuations in $FM_t^{Dom,a}$ using fluctuations in that industry's FM 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 rate 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.

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 coefficient estimate from 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 10 are based solely on the fraction of variation in $FMRate_t^{Dom,a}$ 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, etc. Rather, these results indicate peer effects in misbehavior – in this specific case, from executives in a city's dominant industry, to those outside dominant sectors.

6 Summary and Conclusion

The evidence in this paper reveals large cross-sectional differences in the average rate of detected financial misconduct among the twenty largest U.S. cities. Indeed, the average rates in high-misconduct cities like Miami and Dallas tend to be about three times that of low-misconduct ones like Minneapolis and Indianapolis. City-level rates of financial misconduct also tend to ebb and flow over time and, when used as predictors of misconduct of individual firms, are almost as important as the time-series variation at the industry level.

Although some of these regional patterns may be caused by differential enforcement – via either detection or deterrence effects – we present a number of results, summarized in Table 11, that cannot be explained by this channel. For example, the rate of financial misconduct within a city is correlated, both in the time series and cross-sectionally, with conviction rates for political corruption in the city in which they are headquartered. Further, we find a relation between the incidence of financial misconduct in a city and the tendency of doctors in that city to be influenced by payments from pharmaceutical companies. Given that political corruption is prosecuted non-locally (by the Justice Department), and the questionable behavior of the doctors is not illegal, these correlations cannot be driven by enforcement.

Likewise, we find little relation between FM and economic or demographic characteristics. Various measures of such variables – local population growth, education, and income and employment growth – fail to predict financial misconduct. So, at least for these observable characteristics, regional patterns in FM do not appear to merely reflect a city's prospects.

On the other hand, as shown at the bottom portion of Table 11, social norms, propagated by "peer effects" are capable of explaining all the paper's results. The general idea is that the misbehavior of one person can change perceptions of acceptable behavior, causing spillovers that create "waves" of local misbehavior. While obtaining direct evidence of spillovers is replete with challenges, we provide indirect evidence for such spillovers by examining firms headquartered in industry clusters, but which are *not* in the dominant industry (e.g., retailer Men's Warehouse headquartered in energy hub Houston). Using the energy industry as an example, our evidence indicates that when the rate of financial misconduct in this sector is high, measured outside of Houston, the rate of misconduct for non-energy firms headquartered in Houston (e.g., Men's Warehouse) is also relatively high.

The exact mechanism for transmitting a tendency to commit financial misconduct across firms within a city warrants additional research. Perhaps, information that facilitates questionable behavior is spread by accountants, financiers, and lawyers in a region. (Yet, it is difficult to think that these service providers explain the correlation with misbehavior in the political and medical sectors.) Alternatively, the desire to "keep up with the Joneses" may provide an incentive for executives to commit fraud when they see their neighbors getting ahead by cutting corners. Finally, the social stigma associated with engaging in questionable behavior may be diminished if there is a perception that "everybody is doing it."

Finally, we should stress that although we find no evidence that economic growth rates influence the rate of financial misconduct, it is possible that financial misconduct rates can affect a city's future growth rate. Indeed, if the rate of financial misconduct reflects dysfunctional social norms, we might expect financial misconduct to predict declines in the level of trust, which can make it more difficult to conduct business in the city. We explore these issues in a companion paper (Parsons, Sulaeman, and Titman (2015)), which finds that spikes in regional FM precede increases in borrowing costs, reductions in investment for constrained firms, and higher bankruptcy rates.

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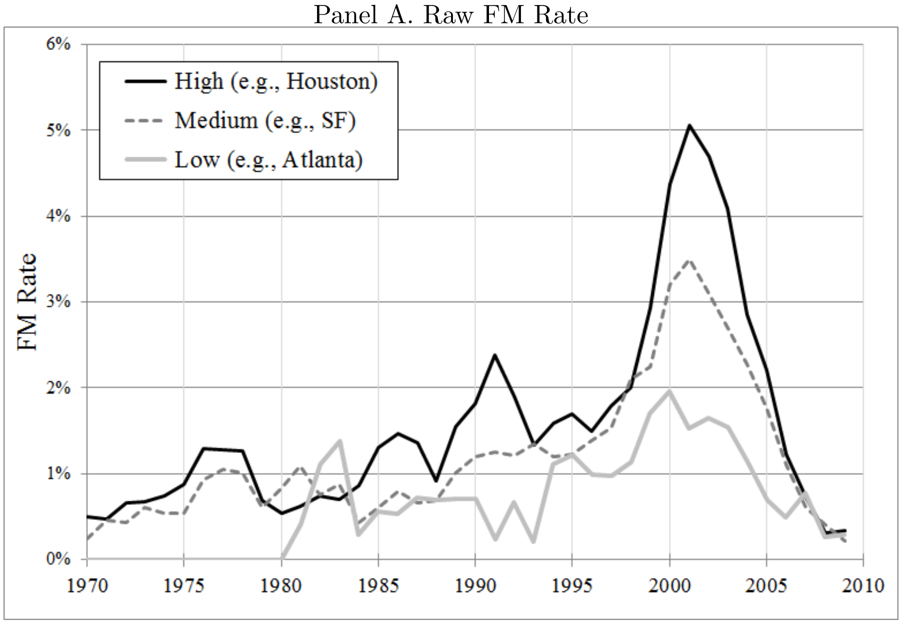
Figure 1: Time Series of Financial Misconduct Rate

This figure reports the time-series pattern of city-level financial misconduct (FM) rates for three different groups of cities sorted by their time-series average of FM 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 FM rates, while Panel B reports the industry-adjusted FM rates, for which the average industry FM rates (outside of our 20-city sample) are deducted from the raw FM rates.

Panel A. Raw FM Rate



Panel B. Industry-Adjusted FM Rate

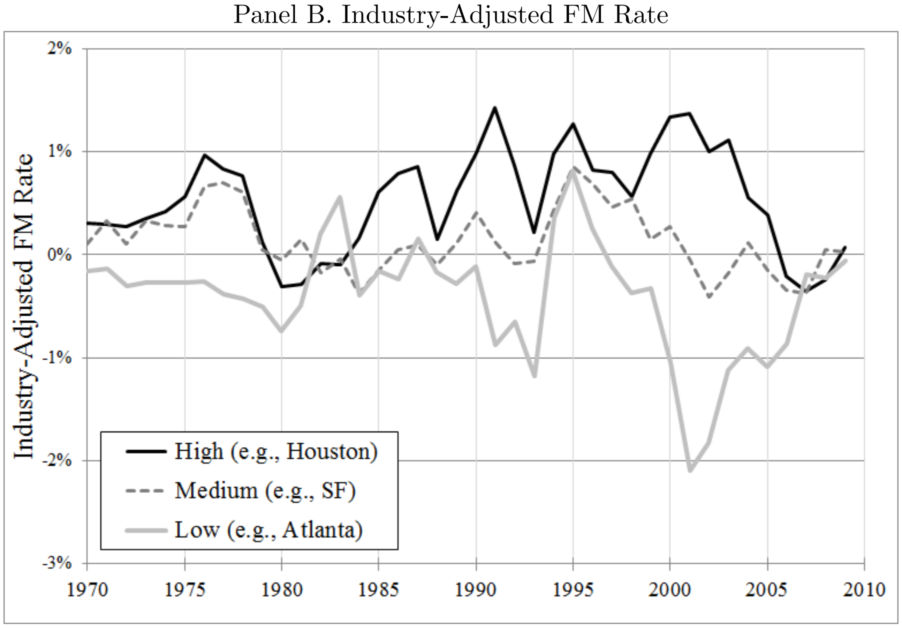


Figure 2: Arthur Andersen Analysis

This figure reports the scatterplot of the ranking of financial misconduct rates for Arthur Andersen clients (y axis) and non-Arthur Andersen clients (x axis) for 20 cities in 2000-2002 period. The numbers used to calculate the rankings in this scatterplot are reported in Table 6.

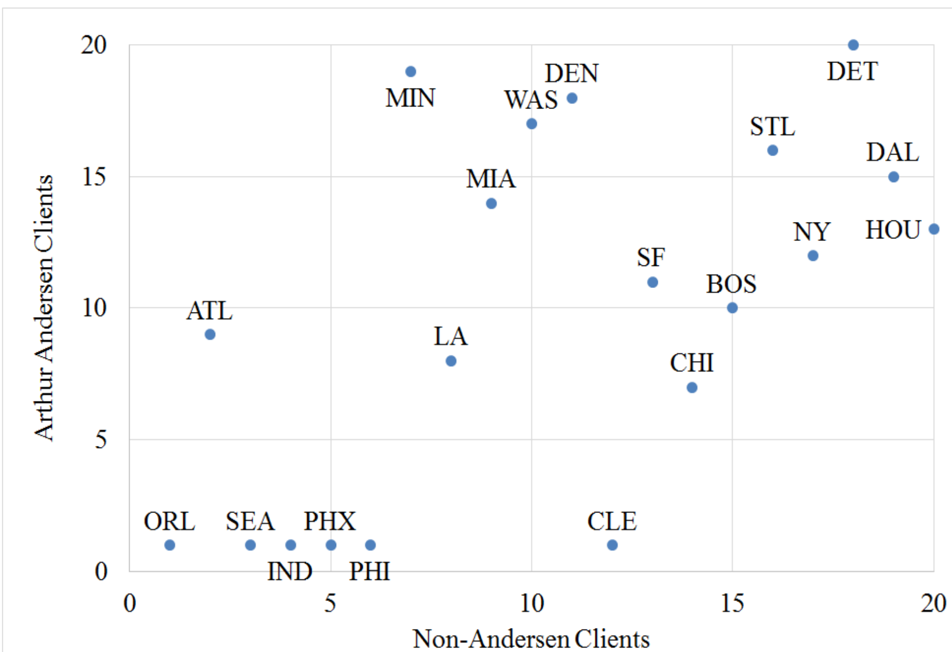


Figure 3: FM Initiation and Detection

This figure depicts the hypothetical scenarios of initiations and exposures of financial misconduct. Panel A displays the scenario in which two cities (Atlanta and Philadelphia) experience the same number of financial misconduct events in 2007 as well as equal numbers of misconduct durations (2 firms each of 1-year and 2-year durations). Panel B displays an alternative scenario in which the two cities experience the same number of financial misconduct events in 2007 but different numbers of misconduct durations: shorter durations in Atlanta (3 firms of 1-year duration and 1 firm of 2-year durations) and longer durations in Philadelphia (1 firms of 1-year duration and 3 firms of 2-year duration).

Panel A **Panel B**

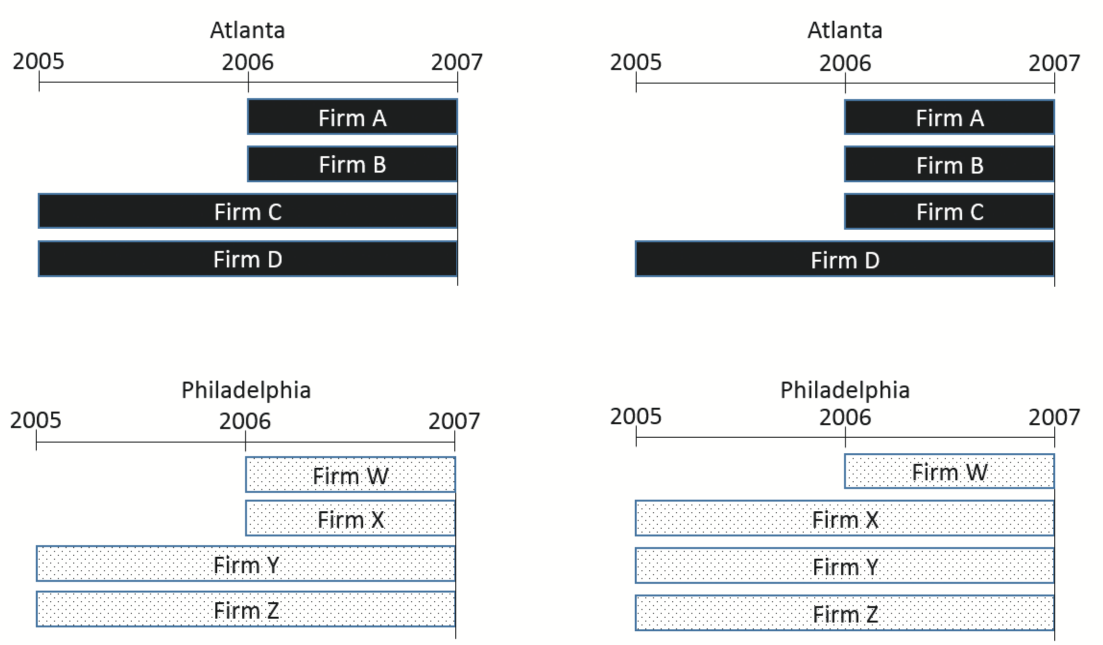


Figure 4: Duration of Detected FM

This figure depicts the duration of each detected financial misconduct (FM) event, and the average duration of FM events *detected in the same year* committed by other firms in the same city, but operating in a different sector. The former is in the y-axis, and the latter is in the x-axis. Both axes are in years, and normalized by the average duration of FM events (excluding the firm-city pair of interest) for all firms in the sample that year. The straight line depicts the best-fit line.

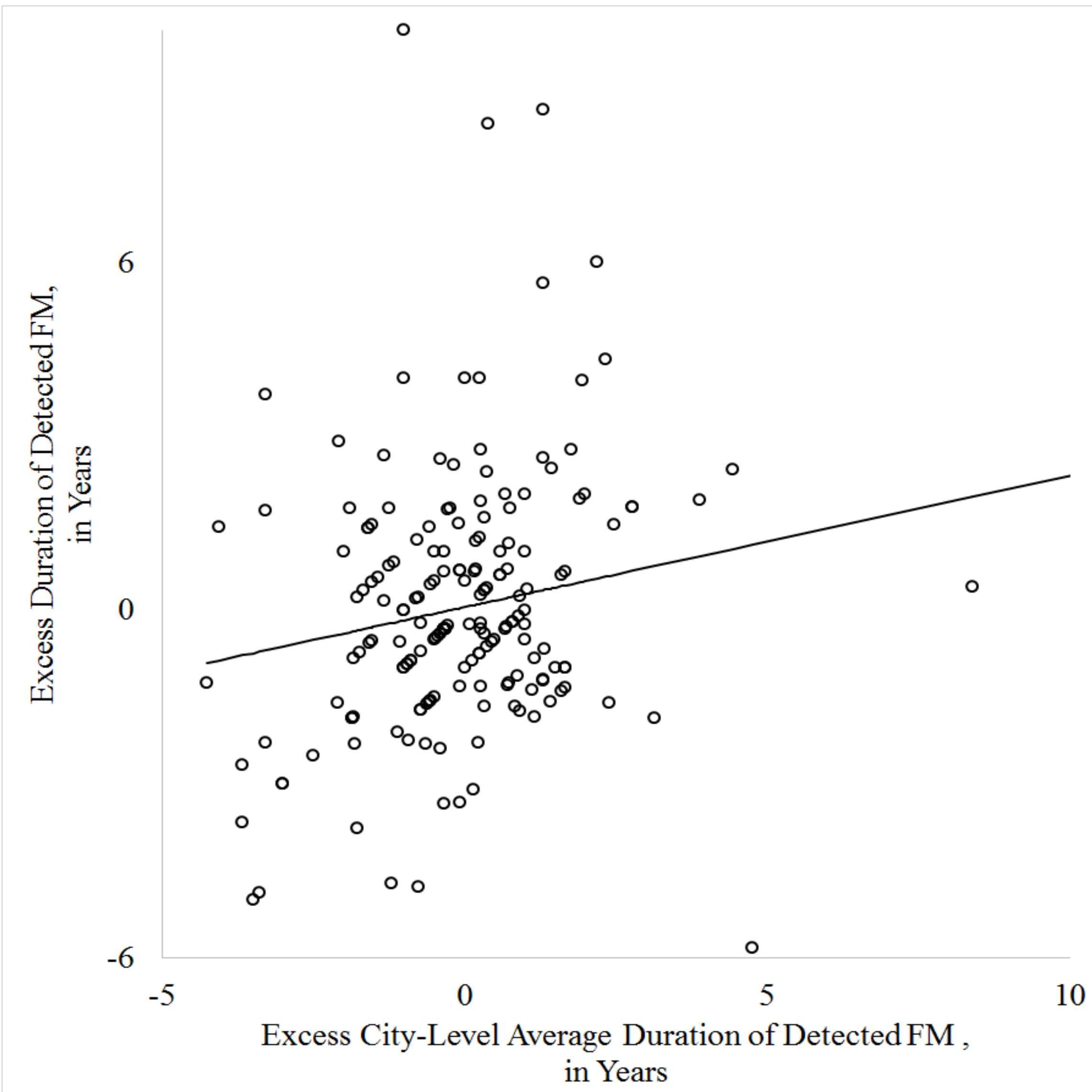


Figure 5: Political Fraud and FM

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.

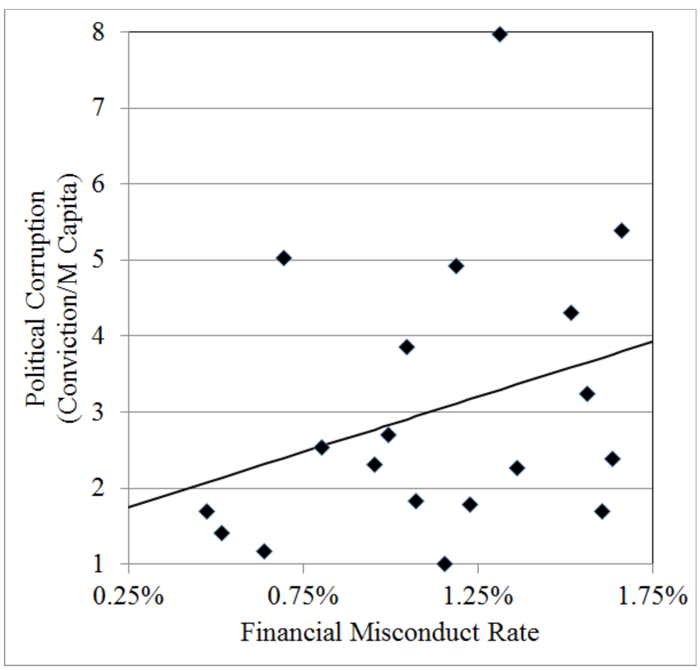


Table 1: Summary Statistics

This table contains summary statistics related to financial misconduct. 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, $FM_{j,t}$, is a dummy variable denoting financial misconduct by firm j, operating in industry i, in area a, during year t. At the city (Panel B) and industry (Panel C) levels, $FMRate$ is defined as rates instead of dummy variables, e.g., the average FM rate for area a in year t is simply the sum of FM in area a during year t, divided by the number of firms headquartered in area a that year. The same applies to industry-level rates. 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 in Panels A-C. Panel D presents the summary statistics of the *FM Duration* variable, which is the number of years from the start of a financial misconduct episode (determined retrospectively as a result of investigation) to its detection.

Variable	**Mean**	**Std. Dev.**	25^{th} **Pctl.**	**Median**	75^{th} **Pctl.**
Panel A: By Firm-Year					
$FM_{j,t}$; 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					
$FMRate_t^a$	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					
$FMRate_t^i$	0.0120	0.0133	0.0000	0.0101	0.0170
Panel D: By FM Episodes					
$FM\ Duration_j$	3.0954	2.5503	1	2	4

Table 2: Summary Statistics, by City

This table contains summary statistics of financial misconduct for each city in our sample. $FMRate^a_t$, is the average FM rate for area a in year t, i.e., the sum of $FM_{j,t}$ of all firms in area a during year t, divided by the number of firms headquartered in area a that year. $Political\ Fraud^a_t$ is the count of prosecutions of elected and appointed public officials at all levels of government and/or of election crimes (per million of population) in area a during year t. $Medical\ Sensitivity^a$ is the area-level average of β_1, estimated from Equation (4). It captures the average increase in prescriptions observed when a doctor has a financial relationship (e.g., gift, speaking fee, meal) with a given drug company, compared to when he/she is not observed to have a financial relationship. We report time-series summary statistics. Economic areas are sorted in ascending order by the mean FM rate. The numbers in parentheses in the last two columns represent the economic area's ranking in each category, with 1 corresponding to the lowest rate.

		$FMRate^a_t$					
Economic Area	Number of Firms	Mean	Std. Dev.	25th Pctl.	75th Pctl.	$Political\ Fraud^a_t$	$Medical\ Sensitivity^a$
Indianapolis	28.03	0.48%	1.34%	0.00%	0.00%	1.70 (5)	17.00 (14)
Seattle	47.90	0.52%	1.12%	0.00%	0.00%	1.42 (3)	12.50 (8)
Minneapolis	123.05	0.64%	0.82%	0.00%	0.98%	1.18 (2)	5.00 (1)
Cleveland	76.65	0.69%	1.06%	0.00%	1.19%	5.03 (17)	17.70 (15)
Atlanta	98.08	0.80%	0.83%	0.00%	1.11%	2.53 (11)	21.30 (17)
Boston	219.20	0.96%	1.11%	0.00%	1.59%	2.31 (9)	12.80 (9)
Orlando	27.78	0.98%	2.12%	0.00%	0.00%		13.90 (11)
Phoenix	46.25	0.99%	1.18%	0.00%	1.84%	2.70 (12)	8.40 (5)
Philadelphia	138.63	1.05%	0.96%	0.23%	1.61%	3.86 (14)	8.30 (4)
Detroit	68.90	1.07%	1.61%	0.00%	1.67%	1.83 (7)	6.10 (2)
San Francisco Bay	234.55	1.16%	1.25%	0.00%	1.46%	1.00 (1)	21.30 (17)
Chicago	180.10	1.19%	1.06%	0.00%	1.97%	4.92 (16)	10.20 (7)
Denver	96.40	1.23%	1.40%	0.00%	2.23%	1.78 (6)	9.50 (6)
Washington, DC	133.18	1.31%	1.22%	0.00%	1.91%	7.97 (19)	7.30 (3)
Los Angeles	270.88	1.36%	0.73%	0.85%	1.91%	2.27 (8)	25.00 (19)
New York	599.13	1.52%	0.99%	0.75%	1.94%	4.30 (15)	13.90 (11)
Houston	136.83	1.56%	2.05%	0.00%	1.75%	3.24 (13)	13.40 (10)
Dallas	154.73	1.61%	1.87%	0.00%	2.15%	1.69 (4)	15.20 (13)
St. Louis	45.45	1.64%	1.95%	0.00%	3.06%	2.39 (10)	19.10 (16)
Miami	105.45	1.66%	1.44%	0.00%	2.69%	5.39 (18)	39.10 (20)

Table 3: City Effects in Financial Misconduct

This table reports the statistics of regressions predicting financial misconduct that include various fixed effects. The dependent variable is $FM_{j,t}$. 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 financial misconduct. The dependent variable in all regressions is $FM_{j,t}$, is a dummy variable denoting financial misconduct by firm j, operating in industry i and in area a, during year t. The main dependent variables of interest are $FMRate_t^{-i,a}$, $FMRate_t^{i,-a}$, and $FMRate_{-j,t}^{i,a}$. They are the FM rates of firms located in the same area (a) but operating in a different industry ($-i$) from firm j, operating in the same industry (i) but located in a different area ($-a$), and other firms operating in the same industry and located in the same area as firm j, respectively. The set of control variables also include the market FM rate excluding firms in the same area and/or industry (in all models), and the time-series average of $FMRate_t^{-i,a}$ (in the last two models). In the last column, the rates are replaced with high FM rate indicator variables, which take the value of 1 if the respective FM rate is higher than 1.2%. The t-stats reported in parentheses are adjusted for clustering at the industry-year level. The significance levels are abbreviated with asterisks: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Peer FM Variables:			Raw FM Rate			High FM Indicator
Dependent Variable:	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$
$FMRate_t^{-i,a}$	8.1118***			7.9641***	9.3299***	0.3708***
	(4.79)			(4.69)	(4.76)	(4.38)
$FMRate_t^{i,-a}$		13.1937***		12.6083***	12.1558***	0.3702***
		(4.29)		(4.18)	(4.06)	(5.39)
$FMRate_{-j,t}^{i,a}$			2.1368***	1.7517***	1.7069***	0.3856***
			(3.84)	(3.02)	(2.94)	(4.37)
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 FM Rates	Yes	Yes	Yes	Yes	Yes	Yes
City Average FM Rates					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 5: A Refinement: Common Firm Size and CEO Age

This table contains parameter estimates from panel logit regression predicting financial misconduct. The dependent variable in all regressions is $FM_{j,t}$, 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 FM rates of subsample of firms located in the same area but operating in a different industry: $FMRate^{-i,a}_{large,t}$, $FMRate^{-i,a}_{small,t}$, $FMRate^{-i,a}_{same\ size,t}$, and $FMRate^{-i,a}_{diff.\ size,t}$. 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 FM 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 FM 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 FM rates of subsample of firms located in the same area but operating in a different industry: $FMRate^{-i,a}_{young\ CEO,t}$, $FMRate^{-i,a}_{old\ CEO,t}$, $FMRate^{-i,a}_{same\ age,t}$, and $FMRate^{-i,a}_{diff.\ age,t}$. 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 5: A Refinement: Common Firm Size and CEO Age (Continued)

Panel A: Size-Matching				
	(1)	(2)	(3)	(4)
Subsample:	Large Firms	Small Firms	All Firms	All Firms
Dependent Variable:	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$
$FMRate^{-i,a}_{large,t}$	3.9326***	0.1819		
	(3.65)	(0.06)		
$FMRate^{-i,a}_{small,t}$	0.6743	13.2194***		
	(0.30)	(6.40)		
$FMRate^{-i,a}_{same\ size,t}$			7.1821***	6.6658***
			(7.07)	(5.49)
$FMRate^{-i,a}_{diff.\ size,t}$			-2.1865	-3.5713**
			(-1.40)	(-2.09)
$FMRate^{i,-a}_{t}$	30.6662***	5.8859	21.3121***	17.5833***
	(5.59)	(0.81)	(4.68)	(4.15)
$FMRate^{i,a}_{-j,t}$	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 FM 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 5: A Refinement: Common Firm Size and CEO Age (Continued)

Panel B: CEO Age-Matching				
	(1)	(2)	(3)	(4)
Subsample:	Young CEO	Old CEO	All Firms	All Firms
Dependent Variable:	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$
$FMRate^{-i,a}_{young\ CEO,t}$	2.5486**	1.6900		
	(2.20)	(1.21)		
$FMRate^{-i,a}_{old\ CEO,t}$	1.4176	1.2996		
	(1.12)	(0.87)		
$FMRate^{-i,a}_{same\ age,t}$			2.1107**	1.9145*
			(2.22)	(1.92)
$FMRate^{-i,a}_{diff.\ age,t}$			1.3469	1.2474
			(1.30)	(1.18)
$FMRate^{i,-a}_{t}$	29.3283***	34.1916***	32.1589***	28.2395***
	(3.66)	(4.15)	(4.81)	(3.27)
$FMRate^{i,a}_{-j,t}$	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 FM 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 6: Arthur Andersen (Former) Clients, by City

This table contains the rates of financial misconduct committed by clients of Arthur Andersen and of other major auditing firms (KPMG, PwC, E&Y, and Deloitte), respectively. The sample includes firm-year observations of firms headquartered in the 20 largest U.S. economic areas over the 2000-2002 period.

	Financial Misconduct		Number of Firm-Years in 2000-2002		
Economic Areas	Non-AA Clients	AA Clients	Non-AA Clients	AA Clients	Fraction of AA Clients
Atlanta	1.05%	1.91%	287	105	26.79%
Boston	5.04%	2.50%	615	160	20.65%
Chicago	4.36%	1.03%	367	97	20.91%
Cleveland	4.14%	0.00%	169	51	23.18%
Dallas	6.62%	5.09%	408	59	12.63%
Denver	3.79%	6.78%	211	59	21.85%
Detroit	6.50%	19.05%	123	21	14.58%
Houston	8.71%	2.91%	333	103	23.62%
Indianapolis	1.61%	0.00%	62	6	8.82%
Los Angeles	3.07%	1.24%	716	81	10.16%
Miami	3.42%	3.03%	234	66	22.00%
Minneapolis	2.42%	9.09%	289	55	15.99%
New York	5.88%	2.84%	1190	211	15.06%
Orlando	0.00%	0.00%	60	13	17.81%
Philadelphia	2.37%	0.00%	338	56	14.21%
Phoenix	2.31%	0.00%	130	54	29.35%
San Francisco Bay	4.22%	2.68%	1089	112	9.33%
Seattle	1.14%	0.00%	175	33	15.87%
St. Louis	5.43%	5.56%	129	18	12.25%
Washington, DC	3.72%	6.45%	323	124	27.74%

Table 7: Duration of Detected Financial Misconduct

This table contains marginal effect estimates from Poisson regression model predicting the duration of detected financial misconduct. The dependent variable is $FM\ Duration_j \mid Detected\ at\ t$, which is the number of years from the beginning of firm j's financial misconduct episode to its exposure at year t. The main independent variable of interest are $City\ FM\ Duration^{-i,a} \mid Detected\ at\ t$, which is the average of $FM\ Duration \mid Detected\ at\ t$ of other financial misconduct episodes in firm j's city (a) for firms operating outside of firm j's industry ($-i$). Other independent variables include $Industry\ FM\ Duration^{i,-a} \mid Detected\ at\ t$, and $Non\text{-}City\ Non\text{-}Industry\ FM\ Duration^{-i,-a} \mid Detected\ at\ t$, which are the average of $FM\ Duration \mid Detected\ at\ t$ of financial misconduct episodes outside of firm j's city for firms operating in and out of firm j's industry, respectively. The t-stats reported in parentheses are adjusted for clustering at the city 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:	$FM\ Duration_j \mid Detected\ at\ t$		
$City\ FM\ Duration^{-i,a} \mid Detected\ at\ t$	0.3191***	0.2306**	0.3364***
	(2.79)	(2.21)	(3.30)
$Industry\ FM\ Duration^{i,-a} \mid Detected\ at\ t$		0.0705	0.0746
		(0.78)	(0.80)
$Non\text{-}City\ Non\text{-}Industry\ FM\ Duration^{-i,-a} \mid Detected\ at\ t$		0.3160*	0.2942*
		(1.79)	(1.90)
Lagged stock return			0.0995
			(0.72)
Lagged asset			0.0714
			(0.72)
Lagged leverage			-0.2541
			(-0.43)
Lagged Q			-0.3589**
			(-2.25)
Lagged cash flow			-1.0541
			(-0.78)
Observations	184	134	111
Pseudo R^2	0.0416	0.109	0.146

Table 8: Locals Not Subject to Common Enforcement

This table contains estimates from regressing corporate financial misconduct on misbehaviors in the political and medical arenas, respectively. Panel A contains parameter estimates from panel logit regression predicting firm-level FM measure. The dependent variable in all regressions in Panel A is $FM_{j,t}$, 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 $\overline{PolCor}^a$ and $MedCor^a$. The former is the time-series mean of $PolCor^a_t$, 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. $MedCor^a$ is the sensitivity of physicians in area a to financial payments by medical companies. In columns (5) through (8), we employ the rank of $\overline{PolCor}^a$ and $MedCor^a$. The t-stats reported in parentheses are adjusted for clustering at either the city-year or city levels. Panel B contains parameter estimates from panel regression predicting *city-level* FM rate. The dependent variable in all regressions is FM^a_t, the city-level average of $FM_{j,t}$ for all firms operating in any industry in area a during year t. The main dependent variables of interest is $PolCor^a_{t-1}$, 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 $t-1$. The control variables in Models (2)-(4) include employment and population growth rates in the city measured in year $t-1$. All models in Panel B include city fixed effects. Columns (3) and (4) replicate column (2) for firms above and below the city's annual median asset size, respectively. 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)	(6)
Dependent Variable:	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$
$\overline{PolCor}^a$	0.0395*		0.0516**	0.0516*		
	(1.80)		(2.33)	(1.89)		
$MedCor^a$		0.0179***	0.0194***	0.0194***		
		(3.97)	(4.65)	(4.48)		
$\overline{PolCor}^a$ Rank					0.0183**	0.0183*
					(2.32)	(1.65)
$MedCor^a$ Rank					0.0235***	0.0235**
					(3.74)	(2.24)
Lagged stock return	0.0853***	0.0868***	0.0849***	0.0849***	0.0853***	0.0853***
	(4.97)	(5.24)	(5.05)	(6.15)	(5.05)	(6.25)
Lagged asset	0.2984***	0.3004***	0.3029***	0.3029***	0.3007***	0.3007***
	(13.55)	(13.73)	(13.88)	(8.10)	(13.72)	(7.92)
Lagged leverage	-0.1135	-0.1278	-0.1280	-0.1280	-0.1272	-0.1272
	(-1.07)	(-1.22)	(-1.21)	(-1.16)	(-1.21)	(-1.17)
Lagged Q	0.2658***	0.2611***	0.2651***	0.2651***	0.2668***	0.2668***
	(10.63)	(10.71)	(10.58)	(8.64)	(10.52)	(8.52)
Lagged cash flow	-0.2192	-0.2332	-0.1962	-0.1962	-0.2194	-0.2194
	(-0.94)	(-1.02)	(-0.84)	(-0.70)	(-0.94)	(-0.77)
Constant	-6.3337***	-6.4793***	-6.6959***	-6.6959***	-6.6760***	-6.6760***
	(-37.67)	(-43.49)	(-37.88)	(-23.33)	(-33.82)	(-20.04)
Observations	90,274	91,146	90,274	90,274	90,274	90,274
Clustering	City-Year	City-Year	City-Year	City	City-Year	City
Pseudo R^2	0.0371	0.0380	0.0390	0.0390	0.0386	0.0386

Table 8: Locals Not Subject to Common Enforcement (Continued)

Panel B: City-Level Analysis								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:		All firms			Large Firms		Small Firms	
Dep. Variable:				$FMRate_t^a$				
$PolCor_t^a$	0.0425*		0.0160	0.0186				
	(1.74)		(0.68)	(0.84)				
$PolCor_{t-1}^a$		0.0674**	0.0605**	0.0610**	0.0872**	0.0886**	0.0519*	0.0535**
		(2.55)	(2.22)	(2.29)	(2.31)	(2.37)	(2.00)	(2.21)
$EmpGrowth_{t-1}^a$				-10.1760*		-10.7213		-9.7134***
				(-1.81)		(-1.17)		(-3.14)
$PopGrowth_{t-1}^a$				20.2409		17.0109		20.4656*
				(1.36)		(0.81)		(1.73)
Constant	1.150***	1.079***	1.049***	0.970***	1.391***	1.366***	0.7469***	0.6617***
	(7.36)	(7.16)	(6.84)	(6.18)	(6.24)	(5.10)	(5.53)	(4.83)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	613	613	613	613	613	613	613	613
R^2	0.090	0.097	0.098	0.119	0.107	0.116	0.118	0.137

Table 9: FM and Local Economic Conditions and Demographic Characteristics

This table contains parameter estimates from panel logit regression predicting financial misconduct. The dependent variable in all regressions is $FM_{j,t}$, 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 $FMRate_t^{-i,a}$, $FMRate_t^{i,-a}$, and $FMRate_{-j,t}^{i,a}$. They are the FM 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 the market FM rate excluding firms in the same area and/or industry, and the time-series average of city fraud rate ($FMRate_t^{-i,a}$). In each of columns (2)-(7), we add lagged and contemporaneous variables reflecting city-level economic conditions: population, employment, and wage growth rates, and demographics: education and religiosity. Education is measured as the percent of college educated individuals in the population. Religiosity is measured as the percent of religious adherents in the population. Columns (8) and (9) replicate column (7) for firms above and below the annual median asset size, 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 9: FM and Local Economic Conditions and Demographic Characteristics (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Subsample:	All Firms	All Firms	All Firms	All Firms	All Firms	All Firms	All Firms	Large Firms	Small Firms
Dep. Variable:	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$	$FM_{j,t}$
$FMRate_t^{-i,a}$	9.3299***	8.6647***	9.3223***	9.4024***	9.3159***	9.6878***	7.8658***	7.8550***	6.8467*
	(4.76)	(4.54)	(4.78)	(4.91)	(4.63)	(4.81)	(3.93)	(3.21)	(1.73)
$FMRate_t^{i,-a}$	12.1558***	11.9304***	12.0623***	12.1847***	11.9373***	12.1113***	11.7275***	10.3229***	16.2074***
	(4.06)	(4.03)	(4.08)	(4.09)	(4.02)	(4.10)	(4.17)	(3.16)	(3.45)
$FMRate_{-j,t}^{i,a}$	1.7069***	1.6168***	1.7168***	1.7213***	1.7160***	1.7084***	1.5375**	2.3923***	-2.9318
	(2.94)	(2.80)	(2.96)	(2.95)	(2.93)	(2.93)	(2.56)	(4.03)	(-1.28)
Lagged stock ret.	0.0655***	0.0650***	0.0631***	0.0629***	0.0597***	0.0630***	0.0629***	0.0055	0.0770***
	(4.00)	(3.96)	(3.77)	(3.85)	(3.35)	(3.73)	(3.90)	(0.12)	(4.31)
Lagged asset	0.2609***	0.2629***	0.2592***	0.2589***	0.2668***	0.2667***	0.2700***	0.3383***	0.2523***
	(12.85)	(13.14)	(12.85)	(12.69)	(13.81)	(13.17)	(14.01)	(11.64)	(4.84)
Lagged leverage	-0.1470	-0.1619	-0.1457	-0.1367	-0.1543	-0.1535	-0.1508	-0.0689	-0.6463***
	(-1.46)	(-1.61)	(-1.48)	(-1.40)	(-1.52)	(-1.53)	(-1.57)	(-0.57)	(-3.50)
Lagged Q	0.2059***	0.2068***	0.2078***	0.2093***	0.2021***	0.2083***	0.2081***	0.2762***	0.1421***
	(8.14)	(8.25)	(8.19)	(8.27)	(8.06)	(8.12)	(8.32)	(7.13)	(3.81)
Lagged cash flow	0.2059	0.1871	0.2226	0.2199	0.1730	0.1563	0.1646	-0.5159	0.3984
	(0.81)	(0.74)	(0.89)	(0.88)	(0.68)	(0.61)	(0.66)	(-1.63)	(1.16)
$Pop.Growth_{t-2}^a$		9.7630					7.3533	4.7765	11.9424
		(1.12)					(0.85)	(0.42)	(0.92)
$Pop.Growth_{t-1}^a$		4.8832					11.0120	9.3523	13.2138
		(0.37)					(0.84)	(0.56)	(0.69)
$Pop.Growth_t^a$		-6.2004					4.4327	-0.8019	10.6663
		(-0.72)					(0.47)	(-0.06)	(0.73)
$Emp.Growth_{t-2}^a$			1.4562				-3.4483	-0.5564	-8.0956***
			(0.74)				(-1.51)	(-0.18)	(-2.62)
$Emp.Growth_{t-1}^a$			-2.5860				-0.0537	0.8470	-0.3616
			(-1.09)				(-0.02)	(0.22)	(-0.09)
$Emp.Growth_t^a$			1.1266				-5.0266**	-1.8409	-10.0449***
			(0.61)				(-1.98)	(-0.55)	(-2.72)
$WageGrowth_{t-2}^a$				-0.8153			-0.4440	-0.8990	1.0856
				(-0.47)			(-0.26)	(-0.42)	(0.42)
$WageGrowth_{t-1}^a$				-0.7803			-1.1190	-1.0771	-0.8924
				(-0.49)			(-0.67)	(-0.46)	(-0.33)
$WageGrowth_t^a$				0.8703			0.5707	-0.0763	2.4605
				(0.67)			(0.35)	(-0.03)	(0.85)
$Education_{t-2}^a$					0.0669		0.0501	0.3871	-0.3758
					(0.31)		(0.21)	(1.13)	(-0.90)
$Education_{t-1}^a$					-0.3771		-0.3966	-0.6299	-0.4322
					(-1.00)		(-0.87)	(-0.94)	(-0.52)
$Education_t^a$					0.3161*		0.3551	0.2564	0.7963*
					(1.68)		(1.44)	(0.73)	(1.79)
$Religiousity_{t-2}^a$						-7.3012	-1.3097	11.7073	-22.8078*
						(-0.88)	(-0.15)	(0.88)	(-1.81)
$Religiousity_{t-1}^a$						9.0373	-4.9901	-27.3646	35.3598*
						(0.59)	(-0.30)	(-1.03)	(1.73)
$Religiousity_t^a$						-1.2537	7.3030	17.8706	-13.5747
						(-0.15)	(0.80)	(1.23)	(-1.28)
Constant	-6.5317***	-6.5930***	-6.5248***	-6.5359***	-6.7436***	-6.7377***	-7.3954***	-8.6061***	-5.6337***
	(-38.17)	(-39.70)	(-38.37)	(-38.78)	(-27.57)	(-27.34)	(-20.53)	(-17.84)	(-10.47)
Market FM Rates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Avg. Rates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	90,208	89,390	89,390	89,390	87,780	87,780	86,962	44,876	42,068
Pseudo R^2	0.0582	0.0583	0.0577	0.0577	0.0574	0.0573	0.0592	0.0737	0.0342

Table 10: Bartik Test for Peer Effects

This table contains parameter estimates from linear probability model regressions predicting financial misconduct. The dependent variable in the second stage regression is $FM_{j,t}$, 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 $FMRate_t^{Dom,a}$, which is the FM propensities of firms in the dominant industry in area a, instrumented using $FMRate_t^{Dom,-a}$, the dominant industry's FM rate calculated using only firms headquartered outside the relevant area ($-a$). Models (3) and (4) employ the lagged value of the instrument variable rather than the contemporaneous value. The t-stats reported in parentheses are adjusted for clustering at the industry-year level. The significance levels are abbreviated with asterisks: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)
2SLS Stage:	1st stage	2nd stage	1st stage	2nd stage
Dependent Variable:	$FMRate_t^{Dom,a}$	$FM_{j,t}$	$FMRate_t^{Dom,a}$	$FM_{j,t}$
Instrumented $FMRate_t^{Dom,a}$		1.94***		0.53**
		(3.43)		(1.98)
$FMRate_t^{Dom,-a}$	0.46***			
	(7.78)			
$FMRate_{t-1}^{Dom,-a}$			0.53***	
			(8.87)	
Lag 1 $\text{return}_{-j,t}^{i,a}$	-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 11: Potential Explanations and Tests

This table contains various potential explanations of our results and a list of our tests. A checkmark indicates that the explanation is consistent with a specific test. An X indicates that the explanation is not consistent with that test. A question mark indicates that the test has no or ambiguous implication for the explanation. T2-T10 indicate Tables 2 to 10, while F1-F5 indicate Figures 1 to 5.

	Main Results							
	Static (T2, T3)	Dynamic (T4, F1)	Same Group (T5)	AA Clients (T6, F2)	FM Duration (T7, F4)	Med & Pol (T8, F5)	Local Econ (T9)	Bartik (T10)
Measurement problem (Detection)	✓	✓	?	X	X	X	✓	X
Reflection problem								
• Exogenous (Fixed attributes)	✓	X	X	✓	X	?	X	X
• Contextual								
◇ Enforcement (Deterrence)	✓	✓	?	✓	✓	X	✓	X
◇ Local economic condition	✓	✓	X	✓	✓	?	X	X
• Peer Effects								
◇ Information (How to cheat?)	✓	✓	✓	✓	✓	X	✓	✓
◇ Relative motives (Keeping up with the Joneses)	✓	✓	✓	✓	✓	?	✓	✓
◇ Norms (OK to cheat?)	✓	✓	✓	✓	✓	✓	✓	✓